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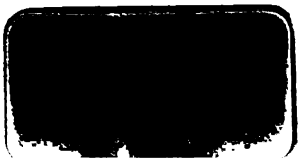

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PREFACE.

TO-DAY "THE CHEMIST" makes its appearance in an entirely new form. The plan of the work has been completely changed, and nothing appertaining to the former work has been retained, except the name. The alterations which we have made have been decided on only after very long and careful consideration; even now, our arrangements are not quite complete, and we hope to render our improvements more conspicuous in our next Number.

It has been a general complaint against this, and all other chemical periodicals, that original articles are seldom given. The blame in this case attaches rather to the great body of Chemists than to us, who would only be too glad to receive such assistance. In future, however, we shall make greater exertions to obtain original articles; and we hope that now, when we have allotted a separate space for original papers, our readers will assist us as much as possible in filling it. We have numerous promises of contributions, and shall be glad to hear from those who intend to contribute. So various are the applications of chemistry, that it is impossible for one or two men, however talented and industrious, to be acquainted with minute details of every branch of the science, or of the arts connected with and dependent on it. In conducting a journal like "THE CHEMIST," cases occasionally arise in which the advice and assistance of a chemist who has devoted himself to one special department of chemistry must be procured. We have, therefore, obtained the aid of several gentlemen who, for some years, have made certain fields of chemical and physical science their particular study, who will, in future, aid us with their knowledge when necessary, and, from time to time, criticise and discuss such papers in "THE CHEMIST" as may appear to call for remark. The responsibility of all editorial observations will, of course, rest as heretofore on the Principal Editor, who alone will exercise the entire control of the work, and on whom alone the selection or rejection of articles will depend.

Instead of being divided, as heretofore, into Chemistry, Chemical Manufactures, and Agricultural Chemistry, Pharmacy, Materia Medica, Therapeutics, &c., "THE CHEMIST" will be arranged as follows:—

The First part of each Number will be apportioned to original articles, whether communicated or editorial, on all the subjects to which the work is devoted: Chemistry in relation to Physiology, Pathology, Industry, and Agriculture, to Metallurgy, Photography, Pharmacy, Public Health, Toxicology, and Physics, in its relation to Chemistry.

Under the head of Industrial Chemistry will be given every month an abstract and critical review of the specification of every Chemical Patent filed within the period of publication. In order that we may be able to form a correct estimate of the value of patents, Patentees will do well for their own sakes to afford us all the information in their power. We hope that our monthly article on New Patents will prove highly useful to those for whose benefit we shall publish it; and that "THE CHEMIST" will soon be referred to by all about to acquire interests in patents.

The Second division will consist of translations of all papers published in the French, German, Italian, and other foreign scientific journals, and abstracts of all those published in England and America. The articles in each division will be arranged under special heads.

The Third department, Bibliography, will be devoted to a careful and impartial review of all works on the subjects treated of in "THE CHEMIST." If found practicable, a list of the papers contained in the English and Foreign periodicals will also be given. This will serve as a reference to such articles as we may not have sufficient room for.

Chemical Notes and Queries will form the Fourth division of the work. In this department it is expected that much valuable and practical knowledge may be diffused by that system of mutual information which has been found to work with such advantage in other branches of Literature. As it is impossible to anticipate the wishes, or meet the requirements, of all classes of readers, an opportunity will likewise be offered to those who may desire information on such points as might not in an ordinary way find a place in the pages of this Journal.

In the Fifth division, the Proceedings of Scientific Societies will be reported whenever Lectures or Papers of a Chemical nature are read before them. In this department the assistance of the Secretaries and Committees of London and Provincial Societies and Institutions is particularly invited.

A Digest of Chemical and Physical Science will be given half-annually, in which all recent contributions to the various departments of Chemical Science will be surveyed and commented on. To those who have not an opportunity of consulting the numerous British and Foreign Journals, Works, Transactions of Societies, &c., this will prove of considerable value, as it will enable them to refer to the original source, when more detailed information on any subject they may be specially interested in, is required. The Number containing the Digest will be enlarged to such an extent as may be found necessary.

We intend to give wood-cuts as often as illustration is required.

Our readers will perceive that great improvement has been made in the mechanical part of the Work. "THE CHEMIST" is now printed in larger and more legible type; and to make up for the deficiency of matter this would otherwise have occasioned, sixteen pages have been added.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On some Modifications of the Electricity of a Jet of Steam.

By REUBEN PHILLIPS.

[This paper being a continuation of papers which have been printed in the "Philosophical Magazine," to avoid confusion, the paragraph numbers are a continuation of the paragraph numbers of those papers.]

196. The steam apparatus employed consists of a cylindrical iron boiler, 2 feet long and 1 foot diameter, with an internal fire-place, and insulated on glass legs. The condensing pipe through which the steam passed is 8 inches long and about $\frac{1}{2}$ inch internal diameter. This pipe passes through a brass box; that part of the pipe within the box being a length of about 6 inches and having an external diameter of ·8 inch, is covered with a mass of loose cotton, which dips into water contained in the brass box. The cover of the box is moveable, so that when required, the cotton can easily be removed and the condenser dried. In these, as in my former experiments, distilled water was employed in the boiler; and as the boiler was not liable to *prime*, the steam and water discharged from the condenser must have been very pure. Throughout the following experiments the steam was used at about 40lbs. on the inch, except where a lower pressure is mentioned, and water was kept in the box of the condenser. The different parts of the apparatus communicated with the electrometers by means of wire. The steam was discharged from a brass jet having a cylindrical opening 1–4th inch long and 1–12th inch diameter.

197. A straight glass tube 3 feet 10 inches long and 1·1 inch internal diameter was placed before the brass jet, so that the axes of the jet and of the tube were in a straight line, the brass jet also projecting into the tube to the extent of about 1–5th inch. On allowing the steam to pass, the boiler and tube were positive, as I had previously discovered ("Phil. Mag." vol. 35, p. 493, § 52). About 15 drops of sperm oil were now placed in the pipe of the condenser, and on again letting off the steam, the tube and boiler took a much stronger positive charge than I had ever before seen with this arrangement.

198. A glass tube 14 inches long, and of the same diameter as the preceding, was substituted for it. The liberation of the steam caused the tube and boiler to become violently positive. A wire-gauze screen was held by a tube-holder in the steam-cloud at a distance of a foot, or of 6 inches from the end of the tube from which

the steam-cloud escaped. The gauze took a strong negative charge. With steam and water without oil, I had always found the gauze to collect a positive charge. It might be inferred from this, that in these experiments with oil, the electricity is generated solely by the friction of the oil and water in the brass jet (Faraday's "Researches," vol. ii. p. 117), which must make the tube positive from the connection which exists between the positive boiler and the tube through the steam-jet, and the steam-cloud negative.

199. The tube was taken away, and the wire-gauze set at a distance of 18 inches from the end of the brass jet of the boiler, the pipe of the condenser being still supplied with oil as before. When the steam was discharged the wire-gauze became strongly positive, and the boiler was also positive. This result was obtained by using steam at various pressures.

200. I now returned to the former arrangement (198), and found that the strong negative charge given by the steam-cloud to the gauze soon diminished when the blast was continued, after which the leaves of the electrometer kept expanding and closing, and when this took place the charge was generally found to be positive. The tube and boiler were always positive.

201. It now follows that the species of electrical excitation which I regard as produced by the friction of gaseous matter on water, is rather increased than extinguished by the addition of oil. I think the modifications introduced by oil may be adequately explained by admitting that the oil causes a finer division of the water. Now, it is evident from Dr. Faraday's balance-pan experiment ("Researches in Electricity," vol. ii. p. 120), that oil facilitates the division of water. And a small globule of water on being shot out of the orifice will expose the film to friction against the gaseous matter, which, considering the perfect fluidity of the oil at a high temperature, may sweep off the oil, leaving a surface of water. Added to this, the film of oil so long as it remains upon the water must prevent its evaporation, and, consequently, the film may be bursted by the generation of steam within it, on the particle escaping from the pressure of steam inside the orifice. Taking, for example, the experiment (198). The oil produces a more minute division of the water than is obtained by steam and water alone, thereby exposing a larger surface of water to the friction of steam and air, and hence the water becomes more positive and the gaseous matter more negative; therefore, the steam-cloud in pouring through the tube more readily imparts positive electricity, while the negative intensity of the cloud augments from the removal of the positive particles; owing to which increase in the negative intensity, the minute particles of water, by the time they arrive at the gauze, have received a negative charge by conduction from the gaseous matter. The negative electricity acquired by the particles of water by friction with the oil in the brass orifice, must also contribute to this final result, but its influence is comparatively unimportant; for returning to (199), the steam being at 40lbs. on the inch, I could perceive no difference in the intensity of the positive charge communi-

cated to the gauze whether the boiler was insulated or not. At lower pressures the positive electricity of the boiler became very feeble, sometimes, indeed, nearly neutral; while the gauze alone, or the gauze and boiler united, were always strongly positive.

202. The separation of the positive drops of water from the negative steam-cloud causes a transformation of mechanical force into electric intensity, which the following considerations appear to render evident. If an electrophorus be taken with its resinous plate charged, and if the metallic plate is laid on the resin, then on touching the metal, its electrical indications cease. If we now begin to raise the metal plate as usual, electricity is immediately manifested, which increases in intensity as the plate recedes from the resin. During the recession of the plate, force is expended in overcoming the electrical attraction of the plate and the resin; let A be the force which is thus expended in entirely withdrawing the metal plate from the resin.

203. Having removed the plate from the resin, and having reduced the metal plate to its neutral condition, let it be again approached to the resin. As the plate approaches the resin an attraction is manifested, and mechanical force generated, which can only result from a new distribution of the electricity, whereby the intensity of the force in the lines of the electric induction becomes diminished. Let C stand for the amount of mechanical power thus given out by the plate on its coming from a distance to the resin.

204. The force of the positive and negative electricity of the electrophorus must be mechanically equivalent to the difference between the force consumed in raising the metal plate and the power returned by the descent of the plate, that is to A—C; or if B stands for the negative electricity given out on touching the metal plate as it lies on the resin, the positive charge on the plate when it is withdrawn is A—C—B.

205. A quantity of drops of water positively electrified and mixed with a mass of steam-cloud negatively electrified to an equivalent degree, must be neutral to external objects; but when these positive drops become separated from the steam-cloud, the motion of the cloud becomes in some measure absorbed and converted into electric intensity, as when the cover of the electrophorus is raised. Similarly, if positive drops of rain fall from a negative cloud, the mechanical motion produced by gravitation becomes converted into electric intensity. And this is, I suppose, the most effective cause of the production of the lightning flash by a sudden fall of rain.

206. A straight glass tube, about 3 feet long and ·4 inch internal diameter, was placed in the same position as the tubes in the foregoing experiments, but the end of the tube at the brass jet was fitted with a good cork, through which the end of the brass jet was inserted. Some oil was placed in the condensing pipe. On discharging the steam, the electrical power of this arrangement was found to be much greater than when steam and water only were used. I have before given what I regard as an explanation of this experiment when steam and water is used ("Phil. Mag." vol. 36, p. 510, § 126), and the oil evidently acts as in the experiments with tubes open at both ends.

207. The apparatus was now thoroughly ridded of the oil. A piece of common hard yellow soap, about the size of a pea, was placed in the pipe of the condenser, and allowed to rest there for a few minutes to dissolve in the hot water which the pipe contained. There was now no tube before the jet. On fully opening the cock of the boiler, it became feebly positive—this effect, however, soon went off, and the boiler became strongly negative, much more so than with pure water.

208. Having arranged the apparatus as in (198), another similar piece of soap was thrust into the condensing pipe. Passing the steam caused the tube and boiler to become strongly positive, as was also the gauze. The soap at first evidently increased the electrical power of the apparatus, so far resembling oil; but on continuing to let off the steam, and at the same time testing the electrical state of the different parts of the apparatus, I found, after various capricious alternations, that the apparatus passed into a state in which no electricity was produced by the friction of gaseous matter on water. The boiler became feebly negative, the tube neutral, and the gauze also neutral after a second or so, from the time that a puff commenced striking on it. This neutral state was very durable, but it was only necessary to put a little soap into the condensing pipe, to restore the electrical powers of the apparatus.

209. The glass tube was removed, the apparatus being still in the neutral state. I now found that it was impossible to render the boiler positive at very low pressures, as when pure steam and water are discharged; I could only obtain at such pressures a feeble negative charge from the boiler. This feeble negative effect which remains, results, I suppose, from the friction of the water on the brass sides of the orifice, which, when pure water and steam are alone used, becomes overpowered by the positive electricity then generated. The boiler was strongly negative at the higher pressures. Thus the mode of electrical excitation, which I regard as produced by the projection of particles of water through gaseous matter, and that produced by the friction of steam on the stratum of moisture which lines the discharging passage, disappear together.

210. The apparatus was again cleaned and the experiments repeated with common soft yellow soap. All the foregoing effects were again observed. It seemed, however, that the negative state of the boiler produced just after the soap is added, was more intense; and that the neutral state was sooner obtained, but not so enduring.

211. It having been found, by Dr. Faraday, that saline matters prevent the production of electricity by the friction of water against solids, and since a minute trace of soap does the same for the electricity produced by the friction of gaseous matter on water, it may follow that by employing a mixture of these substances the electrical powers of the steam-jet should be altogether suppressed. Two solutions in water were accordingly prepared, one consisting of equal weights of soft soap and chloride of potassium—the other of equal weights of soft soap and hydrate of potash. On placing some of either of these

solutions in the condensing pipe, all the different species of electrical excitation nearly vanished, and would doubtless have disappeared altogether by properly adjusting the proportions of the soap and saline matters. The potash solution was the more effective in bringing about this neutral state.

7, *Prospect Place, Ball's Pond Road, Aug.* 28, 1852.

The LAW *of* VOLUMES *extended to* LIQUIDS.

"Aeriforms unite in volumes which are in simple ratio to each other, the product of their union when aeriform being also in simple ratio."

Such is the grand law which, ever since the year 1809, when Gay Lussac first made it public, has enabled chemists to proceed with a somewhat firm step in the way opened up by Wenzel, Richter, and Dalton, and now the only way followed; for, eliminating the palpable volume and reducing the law to its elementary, or as we may say, its theoretical form, we obtain the fact that *the equivalents of chemical bodies in the aeriform state occupy volumes which are in simple ratio to each other*—a deeply interesting fact, especially when taken in connexion with the discovery of Petit and Dulong, that *the specific heats of the equivalents of chemical bodies are also in single ratio.* In fact, this coincidence immediately suggests the question, whether the agreement of chemical equivalents in heat be not the cause of their agreements in volume; for heat, so far as it is known to us, apart from the sensation it gives, is merely a cause of volume, or of elasticity, which is the safeguard of volume (or of currents which are related to it, &c).

If so, then we obtain a sanction to the hypothesis which has been so often reproduced in science, viz., that bodies consist of atoms or molecules, each enclosed in an atmosphere (or may we rather not say, *atomosphere*) of caloric, which caloric it is, and not the material nucleus, that determines the volume of the atom or molecule—the law of the diffusion of heat bringing all atomic or molecular volumes to an equality, or at least to the simplest ratio which that law can accomplish harmoniously with the co-operation of co-ordinate laws.

But if such be the ground of the constitution of bodies, we are plainly to expect that the law of volumes, which has been verified so extensively in reference to aeriforms, shall hold good in reference to liquids and crystals also; for it is certain in reference to both, that their constitutive molecules are individualised, as well as those of aeriforms, though not so perfectly perhaps, especially those of solids.

Let us, in this paper, investigate the point in reference to liquids, postulating nothing more respecting their relations to aeriforms and their intimate construction, than that which is indeed generally supposed respecting them, namely, that when an aeriform condenses into a liquid, the *aeriform atoms* or equivalents, unite or group together into *liquid molecules* or equivalents, which, considered mechanically, are

statical combinations or structures of chemical atoms. And here let us first endeavor to obtain some hint as to the number of aeriform atoms which haply may go to form one liquid molecule.

Water, alcohol, and oil of turpentine, are three familiar liquids, all of them somewhat stable as such at the surface of our planet, while yet they are easily convertible into aeriforms. They are therefore suitable for our purpose. Now of equal volumes of all the three, it has been ascertained that at their respective boiling points, and under the same pressure, each gives an aeriform volume as under :—

	Boiling point.	Volumes of vapor.
Water	212	1689
Alcohol	173	493·5
Oil of Turpentine	313	192.15

(*See* "Daniel's Chemistry," p. 131.)

But these aeriform volumes, when duly expanded or contracted, as required to suit the same temperature, and when the liquid volumes from which they have been derived are adjusted as nearly as possible, so as to represent the respective liquids in analogous states, come out, as nearly as can be estimated, at,—

	Temperature.	Volumes.	Ratios.
Vapor of Water	212°	1690	9
„ Alcohol	ditto	560	3
„ Oil of Turpentine	ditto	190	1

This is, indeed, it must be confessed, but a rough way of handling such a delicate enquiry; but until the specific gravities of liquids shall have been taken at different temperatures, so as to ascertain the law of dilatation in each, labor is thrown away, and an exact agreement between theory and practice would be more against the theory which gave such agreements than considerable differences would be. The precaution of taking the specific gravity of a fluid at different temperatures has indeed been of late found necessary even in reference to not a few aeriforms, which, far from maintaining the same specific gravity, and far from expanding uniformly in every case at all temperatures, are now found to display variations in this respect analogous to those long known to occur in liquids, thus doing away at once with a difference which was believed to exist between these two classes of fluids as to their respective phenomena of dilatation, and with a discouragement to the enquiry, whether the law of volumes might not extend to liquids also. If indeed the element of a liquid—if a liquid molecule—be a little aggregate composed of a greater or a lesser number of aeriform atoms, it is only to be expected that liquid molecules shall undergo variations in their structures (aggregating into larger ones, or partitioning into smaller ones), as the temperature may vary, and thus introducing quite another element besides mere temperature, as an immediate cause of volume in liquids, and giving in liquids varia-

tions of volume under variations of temperature, which the mere variation in temperature must be very inadequate to represent. But to proceed.

When viewed in reference to liquids, the numbers 1, 3, 9, according to what has been advanced already, are but ratios. Unity is the coefficient of an aeriform equivalent, not of a liquid equivalent. The liquid equivalent—the liquid molecule—must be a multiple of unity. But what multiple? In answer to this, it may be said, that proceeding absolutely in the dark as we are now doing (in common with chemists generally, when speculating about the molecules of bodies), with nothing but the laws of intelligence and of statics to guide us, our most legitimate conclusion is, that the liquid molecule, and consequently the multiple in the lowest of the three substances chosen, that is, in turpentine, must be the smallest number compatible with the laws of statical equilibrium among the constituent aeriform atoms; for oil of turpentine is very light (sp. gr. = 0·86—0·87), and its vapor is very heavy (sp. gr. = 4·698). Now the smallest number of dynamical points centres of force, or chemical atoms or agents, which can be combined in truly statical equilibrium, in reference to all directions in space, is a group of four, a tetratom (of which the tetrahedron is the geometrical conception). Let us suppose therefore that the liquid molecule of oil of turpentine consists of four aeriform atoms, or that it is a tetratom, our ratio of 1 : 3 : 9 will thus become a physical conception. Each term needs only to be multiplied by four in order to give us the number of aeriform atoms in one liquid molecule of oil of turpentine, of alcohol, and of vapor. We thus obtain 4, 12, 36, as the numbers of gaseous elements in one liquid element of the three substances compared.

But it will be agreed that we have proceeded far enough in what, when thus laid down, is plainly very speculative and uncertain. It is time to return upon our steps, by way of verification, if it be possible. And that it is possible, provided the law of volumes apply to these liquids, fully appears; for we are in a condition to deduce their specific gravities theoretically. Their specific gravities must in fact be in simple ratio to the quotient obtained from their liquid equivalents and that of water.

Now the liquid equivalent, or the weight of a unit volume or particle, of—

Water . . . $36 \times HO = 36 \times 9 = 324$
Alcohol . . $12 \times C^4H^6O^2 = 12 \times 46 = 552$
Turpentine $4 \times C^{20}H^{16} = 4 \times 136 = 544$

Whence their specific gravities must be:

Water . . . $\frac{324}{324} = 1{\cdot}00$

Alcohol . . $\frac{552}{324} \div 2 = 0{\cdot}85$

Turpentine $\frac{544}{324} \div 2 = 0{\cdot}84$

Now there is every ground for affirming that these numbers, if the liquids were compared in those states in which they are analogous and truly comparable, would give their true specific gravities. Thus, alcohol, which boils about 173° F., has been found at — 28° F., that is, 200° under its boiling point to have a specific gravity of 0·842; and turpentine at 280° below its boiling point 0·875; and at 240° below it 0·86, so that somewhere, as far beneath, as in the case of alcohol, it would have about 0·84, as given by theory.

From the division by 2 in the preceding operations, it appears that the liquid molecules of alcohol and oil of turpentine occupy two volumes analogous to their aeriform atoms, as admitted in inductive chemistry. Others, again, have only half, as for instance,—

Wood-spirit $C^2H^4O^2=32$, which, adopting the lowest multiple for a liquid molecule, gives,—

$$\frac{4 \times 32}{324} \times 2 = 0{\cdot}791. \quad \text{Expt. } 0{\cdot}798.$$

It may be thought that these remarks are too general and rapid for the occasion, but, as has been already stated, it is needless to go into details until the specific gravities of liquids shall have been taken, not for curiosity merely, but for scientific purposes.

Meantime, and at all events, we have fallen upon a general divisor, viz., the weight of an unit-volume, liquid molecule or particle of water, by the aid of which, if the law of volumes apply to liquids as well as to aeriforms, and the process by which we have got at this divisor be legitimate, we ought to be able to obtain, theoretically, approximations at least to the specific gravities of those liquids whose chemical formulæ are truly known.

But previous to venturing on the trial, it is necessary still further to pave the way; for only three multiples or molecular numbers have been as yet obtained by us, while the variety of nature and of types in pure geometry leads us to expect many more. What are the others then? To this it is to be answered that, as there is no room to doubt that the principles of statics, when carried up to their most abstract forms, resolve themselves into the principles of geometry, and that the conditions of statical equilibrium in a system of centres of force, or of any atoms, which are everyway and everywhere similar to themselves and to each other, are also the conditions of symmetry in the architecture of the group, our question may be resolved into this form,—what are the conditions of utmost symmetry in the grouping of similar elements? Now in this form the question may be answered. The required conditions are realized to the utmost in those forms towards which, as the culminating points of his science, the genius of Euclid directed all his labors, the regular polyhedrons. Their number, as commonly reckoned, is five, but to these as equally the type and the limit of them all on the side of symmetry, there ought to be added another, namely, the sphere. Now, of these polyhedrons, the constituents (planes or faces) are either 4, 6, 8, 12, or 20. But it admits of being shewn that the pentagon, besides giving the 12-hedron

may also give a 3-hedron, which, considered mechanically, may be very stable; and, though the law of simple ratio can of course always be brought out as well by 12 for a multiple as by 3, yet it is desirable to add the latter to the series of the poly-atomic numbers. We thus obtain as multiples for the construction of liquid molecules out of aeriform atoms, the numbers 3, 4, 6, 8, 12, 20.

Another fact, however, needs here to be considered. In consequence of the law of electrical induction, viz., the fact that, given a body manifesting one polarity in any region, it constantly tends to induce the opposite polarity in its neighbors, we are to expect that a liquid molecule once constituted, shall exert an influence to the effect, that the next that falls to be constructed beside it shall not be identical with it, but such as is proper to the opposite electrical state, and therefore somehow different from the first. Hence in all cases but those in which the molecules first developed are perfectly neutral (or combine within their own compass both electricities in equilibrio), we are to expect (if the aeriform atoms are easily able to condense in more than one way) two sorts of molecules constituting one liquid, and it may be more. And hence a second class of multiples, formed by the addition of the first to each other, as, for instance, of 3 and 4, of 12 and 20, &c. Nothing less than all this science leads us to expect; and yet in admitting such an extensive series of multiples, we are plainly opening the way for a dialectic which may deny the whole theory, and maintain that the coincidences found are merely products of the method of trial and error, building on the doctrine of chances! But doubtless as the science of morphology advances, limitations will be found in the phenomena, if the theory be true, which chance could never account for. Nay, will chance account even for the coincidences which have been shewn already in reference to water, alcohol, oil of turpentine, and wood-spirit; or will it account for those which here follow as a specimen of the method.

GROUP I.—The liquid element a molecule consisting of three aeriform atoms, occupying one aqueous volume (or of 12 aeriform atoms, occupying 4 aqueous volumes).

Naphtha, $C^{12}H^{10} = 82$. Sp. gr. $\frac{3 \times 82}{324} = 0{\cdot}76$. Expt. best naphtha 0·76.

Potato Spirit, $C^{10}H^{12}O^2 = 88$. Sp. gr. $\frac{3 \times 88}{324} = 0{\cdot}815$. Expt. 0·818

Essence of Bitter Almonds, $C^{14}H^6O^2 = 106$. Sp. gr. $\frac{3 \times 106}{324} = 0{\cdot}981$. Expt. 1·04.

Creasote, $C^{14}H^8O^2 = 108$. Sp. gr. $\frac{3 \times 108}{324} = 1{\cdot}00$. Expt. 1·03

GROUP II.—The liquid element, a molecule consisting of four aeriform atoms, occupying one aqueous volume (or of eight occupying two

aqueous volumes, or of twelve occupying three aqueous volumes (but this last ought not to be admitted).

Nitric Acid, $2(NO^5HO) = 126$ Sp. gr. $\frac{4 \times 126}{324} = 1{\cdot}55$. Expt. 1·522—1·55

Acetic Acid, $C^4H^3O^3HO + 3HO = 87$ Sp. gr. $\frac{4 \times 87}{324} = 1{\cdot}074$. Expt. 1·079

Chloroform, $C^2HCl^3 = 121$ Sp. gr. $\frac{4 \times 121}{324} = 1{\cdot}494$. Expt. 1·491

Dutch Liquor, $C^4H^4Cl^2 = 100$ Sp. gr. $\frac{4 \times 100}{324} = 1{\cdot}23$. Expt. 1·256

GROUP III.—The liquid element a molecule, composed of twelve aeriform atoms, occupying one aqueous volume.

Bromine, Br. $= 78$ Sp. gr. $\frac{12 \times 78}{324} = 2{\cdot}86$. Expt. 2·96

Sulphuric Acid, $SO^3HO = 49$ Sp. gr. $\frac{12 \times 49}{324} = 1{\cdot}813$. Expt. 1·843

Methylamine, $C^2H^5N = 31$ Sp. gr. $\frac{12 \times 31}{324} = 1{\cdot}14$. Expt. 1·08

Sulphuret of Carbon, $CS^2 = 38$ Sp. gr. $\frac{12 \times 38}{324} = 1{\cdot}40$. Expt. (while still rapidly giving off vapor at 32° F.) = 1·293

GROUP IV.— The liquid element composed of two sorts of molecules, in equal numbers.

Ether, $C^4H^5O = 37$ Sp. gr. $\frac{(3 \times 4)\ 37}{324} = 0{\cdot}78$. Expt. (at 32° F., while still giving off vapor abundantly) = 0·736. In this instance it appears that each sort of liquid molecule (they differ but a little from each other) occupies half an aqueous volume. In the following example, where the constituent molecules are very dense dodecatoms and icosatoms, the larger sort occupy half, and the smaller one fourth, of an aqueous volume, or unit volume of water.

Mercury, $Hg = 100$.

$$\text{Sp. gr.} \left\{ \begin{array}{l} \frac{20 \times 100}{324} \times 2 = 12{\cdot}35 \\ \\ \frac{12 \times 100}{324} \times 4 = 14{\cdot}81 \end{array} \right\} \text{Mean } 13{\cdot}85. \quad \text{Expt. } 13{\cdot}85.$$

To reach the integrant molecules of crystals from liquid molecules, the same steps must be taken as are necessary to reach liquid molecules from aeriform. Thus the familiar symbol C^{12} must be con-

structed into tetratoms and these into tetratoms again, to reach the integrant molecule of the

Diamond, $4(4 \times C^{12})$ Sp. gr. $\frac{4 \times 4 \times 12 \times 6}{324} = 3{\cdot}55$. Expt. 3·55.

Hence we see why the more exquisite polyhedrons (the dodecahedron and icosahedron) are scarcely to be looked for in the palpable forms of crystals, which in aiming at the sphere, the type of all symmetry, take a shorter cut, viz., by bevellments, truncations, &c., on the simpler isometric forms, especially the octohedron, which must ordinarily succeed in supplanting all others, when the crystalising molecules and forces are such as to admit of an equiaxial form at all.

The conception given above of a particle of water Aq. (= 36,HO auctorum) = 324, will be found to explain a multitude of phenomena of nature and the laboratory, which remain hitherto unexplained.

Snow Flakes and Ice. The multiple 6 × 6, which is that of the molecule of water here given, is obviously rhombohedral or hexagonal. Now this is altogether the character of the crystalline forms of snow flakes and ice.

The Characteristic Numbers in Endogenous Plants. The monocotyledones are also remarkable for the extent to which the number six, its sub-multiples and multiples, appear in them. Now, this class of plants may also be described as that in which the aqueous tissue predominates over the carbonaceous, and, therefore, we are to expect the manifestation of the numbers proper to the aqueous element.

The Percentage of Salt in the Ocean and in Urine. The simplest symmetrical combination of salt and water which can be constructed (omitting the conception of an atom of salt as a nucleus to a particle of water (Aq. = 36 HO), which cannot be touched upon here), is a particle of water with one of salt in or on each pole, which may be written thus: NaCl, Aq., ClNa, or rather, indeed, ClNa, Aq., NaCl = 2 (NaCl) + 36 HO = 117 + 324 proportional to 26·5 + 73·5, that is, 26·5 per cent., or adopting Prout or Thomson's atomic weights, 27·3 per cent of salt. Now, experiment gives uniformly about 27 per cent. as the maximum quantity of salt which water can be made to take up and hold in solution. Encase such an atom of brine in fresh water, for which everything indicates that twelve particles of Aq., will be required, which gives thirteen particles of water in all to two of salt, and we obtain

2 NaClAq + 12 Aq = 117 + 4212 proportional to 2·77 + 97·23;

that is, 2·77 per cent. of salt and 97·23 of water, that is, the percentage of the waters of the ocean, which is the beginning, as also of the urine, which is the end of the cycle of nature, and in both of which a symmetrically constructed molecule is to be looked for.

The Blood, &c. It is well known that blood contains from 79 to 80 per cent. of water, and not blood only, but the proteine-yielding

compounds generally when alive, both in the Vegetable and the Animal Kingdom. This seems very wonderful; but the views here advanced enable us to perceive that there is the same reason in the living world for this percentage that there is for it in the waters of the ocean's bed, and of the bladder. In order to life, it appears that the chemical organic elements must in every instance be enclosed in a shell of water. Thus, taking proteine as the type (rather the resultant) of the class of compounds referred to, its formula, according to Mulder, is $C^{36}H^{24}N^4O^{12} + 2\ HO$; according to Dumas and Cahours, $C^{40}H^{36}N^6O^{14} + HO$, formulæ which do not conflict but represent, the former a tetratom, the latter a hexatom, or rather the half of a dodecatom of a merorganic or quasi-aeriform element, the ground of the organic molecules, and now $C^6H^4NO^2$, now $C^6H^6NO^3$, now $C^8H^6NO^3$, and $C^7H^4NO^3$, according to its position in the current of life. To any of these give one particle of water, or to a tetratom constructed of them give four (which meets Mulder's formulæ), or to a hexatom give six, which meets Dumas' formula; or rather take the molecules as possessing their highest structure viz., as dodecatoms or icosatoms, and allow a shell of water around, as in an element of the ocean, we obtain for all the same result. In the dodecatom, for instance, that is, doubling Dumas' formula, we obtain $2\ (C^{40}H^{36}N^6O^{14}) + 12\ Aq = 1040 + 3890$ proportional to $20 + 79$ that is about 80 per cent. of water. From Mulder's we obtain $(C^{36}H^{24}N^4O^{12} + 2\ HO) + 4\ Aq = 314 + 1314$ proportional to $18{\cdot}3 + 80{\cdot}7$, and so ranging between 78·2 and 81·1 per cent of water, as might be shewn by the consideration of the elementary formulæ. J. G. M.

THE POTATO DISEASE.

To the Editor of The Chemist.

Sir,—During the course of the last few months that dreadful scourge, the potato-blight, has again made its appearance amongst us, and has already committed great devastation in this and the sister island. My avocation having lately afforded me a few weeks leisure, I determined to devote it to the re-investigation of the subject, in the hope of ascertaining, if possible, the cause or origin of the disease, as well as a means of curing it or preventing its occurrence. I will not now occupy your time and space with a long account of all the experiments and researches that I have made with this object, but will content myself with making you acquainted with the general conclusions at which I have arrived. They are as follows:—

1st. That the potato-blight is neither directly nor indirectly caused by the ravages of any parasitical insect.

2nd. That it is the effect of a species of putrefactive fermentation or incipient decomposition of the nitrogenous (i.e. albumenoid) constituents of the sap or cell contents.

3rd. That this decomposition is either directly produced by a peculiar fungus, the *botrytis infestans*—to which public attention has been already directed by other writers—or, what is, in my opinion, a still

more probable supposition, the fungus referred to only makes its appearance after the fermentative processes have been in action for some time, and consequently, is *an effect* and not *the cause* of the disease.

4th. That the blight has been in some measure produced by the long-continued and indiscriminate use of animal nitrogenous manure, which has over-stimulated the potato-plant, and thus rendered it more susceptible of disease, and has, in fact produced the same effect upon it that alcoholic drinks, when taken in excess, do on the human system; that is to say, it has injured the stamina of the plant, and rendered the organism more readily affected by atmospheric and other influences.

5th. That animal or highly nitrogenous organic manures should be used with great caution in the cultivation of the potato, and, indeed, in that of all root crops; the best manure for the potato-plant being the inorganic composts, such, for instance, as those which are, or were at one time, used in some parts of the Continent.

6th. That the disease having once established itself has become epidemic.

7th. That it is contagious, if not infectious.

8th. That the only mode of eradicating it, is to restore the original constitution of the plant.

9th. That this desirable result can be only brought about by introducing a complete alteration in the mode of cultivation that is adopted.

10th. That the changes in question should consist:—1st. In thoroughly drying the seed potatoes, by the process now followed in some parts of Germany; 2ndly, in steeping them for a short time in a dilute solution of the sulphate of copper (blue vitriol or blue stone), of about the same strength as that used far "pickling" wheat; 3rdly, in planting them in poor, well drained land; 4thly, and lastly, in substituting for the farm-yard manure, &c., now employed, some *inorganic* compost, similar to those before alluded to.

In conclusion, I would suggest that the following simple experiment should be tried in storing the potato crop during the present season:—Let the tubers be stored in the usual way, but in the centre of each heap or sackful let there be placed a quantity of *unslacked* lime, not in actual contact with the roots, but enclosed in some porous vessel—an old wicker basket for instance—and covered over with, and surrounded by, a thick layer of straw or hay. By this means the tubers will be kept *dry*, and, as the presence of humidity in the air is a great incentive to putrefactive decomposition, one of the main causes of decay will be removed. The lime, as soon as it has become slacked, may be taken away and employed as manure, and, if practicable, should be replaced with fresh lime. The experiment I have described, it must be remembered, can be easily tried, and would cost but little, even if carried out on a large scale; it cannot be productive of any injurious consequences, and will be doubtless attended with beneficial results.—I remain, sir, your obedient servant,

THORNTON J. HERAPATH.

Mansion-house, Old Park, Bristol, August 17th, 1853.

CHOLERA IN 1853.

It is with deep sorrow that we have to notice the reappearance amongst us of the Cholera. After many and fearful ravages in the East, it appeared on the shores of the Baltic; and for many weeks, in the undrained streets and alleys of Copenhagen, the scenes of sorrow, wretchedness, pain, awe, and also of heroic fortitude, have been such as could be equalled only in London during the dread season of 1849. The adjoining countries of Sweden and Norway established the strictest quarantine against the fell invader, but in vain; in spite of all human endeavors, the cholera broke the quarantine, and rages now in both countries.

In the commencement of September, cases of Asiatic cholera occurred in Newcastle-on-Tyne. They have fearfully increased in number, and at least two-fifths of the cases have proved fatal. Fatal cases have also occurred in neighboring towns. The disease has also appeared in isolated, but violent, cases in London, and also at Torpoint on the south-west coast.

If we consider well the localities in which this plague has reappeared in this country, we find matter for the deepest self-reproach. In the year 1849 the results of diligent observations widely spread, and of medical science carefully investigating all branches of the subject, shewed, at least, the circumstances most provocative of the disease, and thereby suggested many obvious remedial measures. But how many of these have been adopted and carried out? When the arch-enemy was amongst us, carrying desolation into almost every circle, great was the outcry to close intramural graveyards, those pests and fever-stills of our large towns; yet some of the worst in London are still unclosed; and in the church adjoining one of them it is hardly possible to carry on service in the evening, owing to the offensiveness of the gases drawn in by the heat of the building. At the same time, it was *decided* that effective sewerage must everywhere be adopted; yet we still find a small town, such as Portsmouth, the same sty of seething corruption that it was—that it has been, for many years; and amongst the large towns, we still find the filth of London poured in vast volumes into a noble river, converting it into a stinking tide-ditch, and the so-called sewers vomiting forth foul gases from innumerable vents over the whole area of the city. At the same time, it was also *decided* that steps must be taken to diminish the fearful overcrowding in the lodging-houses of the very poor; yet now we find that, though an Act of Parliament has been passed expressly ordaining the regulation of this subject, still in such a town as Newcastle the provisions of the Act have not hitherto been applied; and consequently, it is in the very places where the seeds of disease ought, ere this, to have been eradicated, namely amongst the horribly overcrowded lodging houses, that the cholera has landed itself on the shores of England. In consequence of our experience in 1849, authority was granted by Parliament to local bodies, or in aggravated cases to individuals, to compel the removal of dangerous nuisances. Yet now

we find a case of cholera in Southwark, in one of five houses *inhabited beneath some bone-works*, with loads upon loads of bones, both boiled and unboiled, heaped in adjoined yards.

It is impossible but that these subjects must be recurred too. Meanwhile we close our present remarks with an extract from the Registrar General's Return for the Spring quarter of this year.

"The mortality of the quarter was above the average both in the town and in the country districts; the annual rate of mortality was 2·606 (instead of 2·471) in 117 districts comprising the chief towns, with a population of 7,795,882, and 2·196 per cent (instead of 2·067) in 508 districts extending over the rest of the kingdom, or a population of 10,126,886.

"The population of England is, there is reason to believe, collectively healthier than any equal amount of population in any other kingdom, but the rapid increase in the proportion of the town population,—in which the mortality is 27 per cent higher than it is in the country, and the sickness, the suffering, the debility, the physical degeneracy of race are in an equal excess,—makes this question of the health of towns and the fertilisation of the surrounding fields, one of the great questions of the day demanding immediate solution. It is difficult for the imagination to conceive all the beneficent effects that would flow from the possible diminution of the mortality which the subjoined figures express in town and in country throughout the changing seasons of the year:—

	Average Annual Number of*		
In the months of	Deaths to every 10,000 Persons living in Towns.	Deaths to every 10,000 Persons living in the Country.	Lives destroyed by the Matters which are poisonous in houses, streets, and streams, but are fertilising manures in fields.
Jan., Feb., March..	69	56	13
April, May, June .	62	52	10
July, Aug., Sept. .	63	46	17
Oct., Nov., Dec. .	64	49	15
The Year . .	258	203	55

* This Table is derived from the return of 10 years 1843-52.

TRANSLATIONS AND ABSTRACTS.

THE LAWS OF CHEMISTRY.

Researches on Chemical Affinity. By RUDOLPH BUNSEN, Professor of Chemistry in the University of Marburg.

THE force, which is generally regarded as the cause of chemical combinations and decompositions, may be augmented or diminished by various circumstances. Its intensity varies under the influence of light, heat, and electricity; it changes with the mass of the reacting substances, with their state of aggregation, and by the contact of other matters. We may, consequently, look upon this force as a function of all these influences; and if the mathematical form of this function could be determined, it would give the measure of the absolute intensity of the force itself.

Claude Berthollet, the celebrated author of the "*Statique Chimique*," was the first who regarded the cause of chemical phenomena in this light. His profound conceptions led him to formulise the law of masses, which is still recognised as true. According to this law, a body on which two substances, which are different in their nature and in their mass, react, is divided between them, according to a ratio proportional to the products of their masses and of their respective affinities. If we call A and B the masses of the two substances which react on the body C, and which are found in excess, and α and β the co-efficients of their respective affinities with this body, the quantities a and b of the substances A and B, which combined with the body C, are among themselves, as αA is to βB. From this it results that the ratio of the affinity of the substances A and B for the body C, is expressed by the equation

$$\frac{\alpha}{\beta} = \frac{a\mathrm{B}}{b\mathrm{A}}$$

It appeared to me important to subject this law to the test of experiment, which, until now, has never been applied to it. The results have been contrary to him, and have led me to formulise another law, which appears to throw some light on the essence and the manifestations of affinity. This new law may be enunciated in the following four propositions:—

1. When we present to the body A, in circumstances which favour the combination, two or more bodies B,B′, which are found in excess, the body A selects of the bodies B,B′ quantities which are in simple atomic proportions; so that there are formed 1, 2, 3 equivalents of one of the combinations, and at the same time 1, 2, 3, 4 equivalents of the other.

2. In the case in which these are thus formed, with 1 equivalent of the combination A + B, 1 equivalent of the combination A + B′, we may augment, within a certain limit, the mass of the body B in rela-

tion to that of the body B′, without the atomic ratio of the combinations which are formed being modified: but, as soon as this limit is exceeded, this ratio suddenly changes from 1 : 1, it becomes 1 : 2, 1 : 3, 2 : 3—, &c. In these conditions the mass of one of the bodies may be increased *de novo* without this new ratio being changed, until it has attained a certain limit, beyond which it undergoes a new modification.

3. When a body A exerts a reductive action on a combination B + C, which is in excess, so that C is liberated with formation of a compound A + B, in the case in which C may reduce in its turn the compound A + B, the final result of the decomposition is such, that the portion of the combination which is reduced is found to be in a simple atomic ratio with the portion which is not so.

4. In all these reductions the mass of one of the bodies may be augmented within a certain limit, without the atomic ratio of the combinations formed being changed. Beyond this limit, this change is made suddenly, but always according to very simple rational numbers.

It is not surprising that these remarkable relations have not been sooner discovered: they are observed, indeed, only in cases in which the reactions which they govern occur simultaneously. If it happened, for example, that the body A being put in contact with the bodies B and C, the combination A + B might be formed more easily and in less time than the combination A + C; necessarily, the ratio of B to C would be modified throughout the duration of the experiment, and, consequently, the ratio of the combinations formed should undergo the same modifications. Instead of being rational, this ratio would be, consequently, mixed. The same irregularity would be observed if the mixture of the substances were not perfectly homogeneous. All this amounts to saying that it is easy, or even possible, to observe these laws only with combustible gaseous mixtures, in which the circumstances, which have just been pointed out, do not disturb the phenomena. I have, consequently, selected gaseous mixtures as the starting-point of my researches.

When oxide of carbon and hydrogen are detonated with a quantity of oxygen insufficient for determining complete combustion, this oxygen is divided between the two combustible gases, so that the quantities of carbonic acid and water formed are in a simple atomic ratio, in conformity with the law indicated.

When we know the volume O of the oxygen employed, and the contraction C, which the volume of the mixture undergoes after combustion, the volume k of the oxide of carbon burned, and the volume h of the hydrogen burned, may be calculated, with the aid of the following equations:—

$$\frac{1}{2}k + \frac{3}{2}h = C,$$

$$\frac{1}{2}k + \frac{1}{2}h = O;$$

Whence we derive—$3\,O - C = k$
$C - O = h$.

To make these experiments it was important to be able to burn the gases under some pressure. To attain this object, the apparatus, of which the above is a figure, is employed. A is an ordinary and rather thick glass eudiometer; its lower extremity is carefully ground, and passes at *b* into a screw fixed with sealing-wax. By means of this screw it is easy to press forcibly the opening of the eudiometer against a sheet of vulcanised caoutchouc fixed to the bottom of a brass piece B, so as to hermetically close the eudiometrical tube. The piece B, to which are adopted at *d* and *d'* two plates of steel forming a spring, may slide into the interior of the wooden cylinder C, enveloped with a glass sheath, which draws it out. The plates *d* and *d'* are employed in friction in the grooves *i* and *i'* of the wooden cylinder, so that the eudiometer may be kept at any height whatever. By turning it to the right or to the left on its axis, it may be closed or opened, whatever may be the pressure to which the gas is subjected; for, whilst the screw is removed with the eudiometrical tube, the nut remains fixed to the piece B, retained by the grooves, and remains immovable.

The analyses were made by methods which I commonly use. As soon as the gases, suitably measured, are contained in the eudiometer, the instrument closed, is introduced into the cylinder; it is inserted in it so far as to establish the pressure at which it is desired to burn the mixture; it is opened, closed again, and the spark is passed into it.

The hydrogen employed in these experiments was prepared by electrolysis, and the oxide of carbon gas, by the decomposition of formiate of magnesia, by means of sulphuric acid; this last gas is perfectly pure, after having been heated by caustic potassa.

The first experiments were made with the following mixtures.

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m pressure.
Detonating gas prepared by electrolysis	42·7	71°96'F.	0m·6232	24·61
After the addition of oxide of carbon gas	132·0	71°96'F.	0·7350	89·73

Composition of the gas in 100 parts :—

I.

Oxide of carbon	72·57
Hydrogen	18·29
Oxygen	9·14
	100·00

Two portions of this mixture were analysed.
The first portion burned in the dark at $0^m.7338$ and 72°14′F.

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m of pressure.
Before combustion	145·8	72°14′F.	$0^m.7338$	98·92
After combustion	124·1	72°32′F.	$0^m.7318$	83·93
Contraction of 100 volumes of gas				= 15·14
Volume of Oxygen burned in 100 volumes				= 9·14

Gases burned in 100 volumes :—

II.

	Found.		Calculated.
Oxide of Carbon . .	12·28	2 vol.	12·18
Hydrogen . . .	6·00	1 „	6·09
	18·28		18·27

Second portion burned in the dark at 0^m 7324 and 72°5′F.

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m of pressure.
Before combustion	255·3	72° 5′F.	$0^m.7324$	172·76
After combustion	216·2	72°14′F.	$0^m.7318$	146·28
Contraction of 100 volumes of gas				= 15·33
Oxygen burned per 100 volumes of gas				= 9·14

Gases burned in 100 volumes :—

III.

	Found.		Calculated.
Oxide of Carbon . .	12·09	2 vol.	12·19
Hydrogen . . .	6·19	1 „	6·09
	18·28		18·28

In the following experiments this gaseous mixture was employed :—

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m pressure.
Detonating gas prepared by electrolysis . .	57·6	72°·32′F.	$0^m.6422$	34·19
After the addition of oxide of carbon gas . .	130·3	72°· 5′	$0^m.7085$	85·32

Composition of gas in 100 volumes :—

IV.

Oxide of carbon	59·93
Hydrogen	26·71
Oxygen	13·36
	100·00

This mixture burned in diffused light at $0^{m}3952$ and at 72°5′F.' gave :—

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m of pressure.
Before combustion	119·5	72°·5′F.	0^{m}·7293	80·52
After combustion	87·2	72°·5′	0^{m}·7293	58·76
Contraction of 100 volumes of gas				= 27·02
Oxygen burned in 100 volumes of gas				= 13·15

Gases burned in 100 parts :—

V.

	Experiment.	Theory.
Oxide of carbon . .	13·06 1 vol.	13·36
Hydrogen . . .	13·66 1 „	13·36

In experiment No. I., the proportion of the oxygen to the combustible gases was 10 : 99 · 4, and that of the hydrogen to the oxide of carbon 10 : 39 · 7. In two experiments (Nos. II. and III.), this mixture gave, for 1 vol. of hydrogen gas burned, 2 vols. of oxide of carbon gas burned; however, the height occupied by the two gases in the eudiometer was very different. We may draw from this fact the conclusion that, within certain limits, the density of a gaseous mixture does not exert any action on the proportion of the gases which burn in a mixture; for even when the gases are burned under the same pressure, the combustion which is propagated downwards, must produce an expansion, and consequently, on the lower and still unburned parts of the mixture, a compression which must vary with the height of the gaseous column.

In experiment V., the mixture (No. IV.) was employed, which, to 10 parts of oxygen, contained only 64·9 of combustible gases, and to 10 parts of hydrogen, only 22 vol., 2 of oxide of carbon gas. It gave equal volumes of burned products (No. V.).

In burning the mixture (No. VI.) we observed a different proportion, but likewise rational.

	Vol.	Temperature.	Pressure.	Vol. at 32° F. & 1 m pressure.
Detonating gas obtained by electrolysis . .	104·0	72°14′F.	0^{m}·6713	64·55
After the addition of carbonic oxide . .	150·0	72° 5′F.	0^{m}·7358	101·98

Composition of the gas in 100 volumes :—

VI.

Oxide of carbon	36·70
Hydrogen	42·17
Oxygen	21·13
	100·00

This mixture served for three experiments. First portion burned at $0^{m}\cdot7264$ and at 72°5′F.

	Vol.	Temperature.	Pressure.	Vol at 32°F. and under 1^{m} of pressure.
Before combustion	168·1	72°· 5′F.	$0^{m}\cdot7264$	112·82
After combustion	80·2	72°· 68′.	$0^{m}\cdot7252$	53·72
Contraction of 100 volumes of gas				=52·38
Oxygen burned in 100 volumes of gas				=21·13

Gases burned in 100 volumes :—

VII.

	Experiment.	Theory.
Oxide of carbon . .	11·01 1 vol.	10·50
Hydrogen . . .	31·25 3 „	31·70
	42·26	42·26

Second portion burned at $0^{m}7230$, and at 72°·68′F. in the dark.

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1^{m} pressure.
Before combustion	113·4	72°·68′F.	$0^{m}\cdot7234$	75·77
After combustion	58·2	72°·86′	$0^{m}\cdot6667$	35·81
Contraction of 100 volumes of gas				=52·74
Oxygen burned in 100 volumes of gas				=21·13

Gases burned in 100 vols. :—

VIII.

	Experiment.	Theory.
Oxide of carbon . .	10·65 1 vol.	10·57
Hydrogen . . .	31·61 3 „	31·69
	42·26	42·26

Third portion burned at $0^{m}3169$, and 71°6′F. in diffused light.

	Vol.	Temperature.	Pressure.	Vol at 32°F. and under 1^{m} pressure.
Before combustion	272·3	71° 6′F.	$0^{m}\cdot3169$	79·86
After combustion	59·0	72°86′	$0^{m}\cdot6938$	37·79
Contraction of 100 volumes of gas				=52·68
Oxygen burned in 100 volumes of gas				=21·13

Gases burned in 100 volumes :—

IX.

	Experiment.	Theory.
Oxide of carbon . .	10·71 1 vol.	10·59
Hydrogen . . .	31·55 3 „	31·67
	42·26	42.26

The succeeding experiment was made with the following mixture :—

	Vol.	Temp.	Pressure.	Vol. at 32° F. & 1m pressure.
Detonating gas employed	65·7	72° 86′ F.	0m·6321	38·35
After the addition of hydrogen	98·0	73° 04′ „	0m·6645	60·11
After the addition of oxide of carbon	151·9	73° 4′ „	0m·7165	100·38

Composition of the gas in 100 volumes :—

X.

Oxide of carbon	40·12
Hydrogen	47·15
Oxygen	12·73
	100·00

Burned at 73° 4′ F., and at 0m·7200, in diffused light.

	Vol.	Temp.	Pressure.	Vol. at 32° F. & 1m pressure
Before combustion	168·6	73° 4′ F.	0′·7194	111·87
After combustion	112·4	73° 4′ „	0′·7206	74·71

Contraction in 100 volumes of gas = 33·22.
Oxygen burned in 100 volumes of gas = 12·73.

XI.

	Experiment.		Theory.
Oxide of carbon	4·97	1 vol.	5·09
Hydrogen	20·49	4 „	20·37
	25·46		25·46

Another mixture of undetermined composition gave :—

	Vol.	Temp.	Pressure.	Vol. at 32° F. and under 1m pressure
Volume of gas employed	187·9	67° 64′ F.	0m·4389	76·90
After the explosion	155·9	68° 0′ „	0m.4063	59·02
After the absorption of carbonic acid	142·2	32° 18′ „	0m.4082	54.07

XII.

	Experiment.		Theory.
Oxide of carbon	4·95	1 vol.	5·07
Hydrogen	10·27	2 „	10.14
	25·22		25·21

In the mixture (No. VI.) the proportion of the oxygen to the combustible gas was as 10 : 37·3, and the proportion of the hydrogen to the oxide of carbon as 10 : 8·7. The three combustions (Nos. VII., VIII., IX.) which were made of this mixture shew that, for 1 volume of oxide of carbon, 3 volumes of hydrogen were burned, and that in the most various conditions, whether the pressure was 0m.6264 or 0m·3169, and whether the combustion was performed in the dark or in

the solar light. In the mixture (No. X.) the proportion of the oxygen to the combustible gases was as 10 : 68·5, and that of the hydrogen to the oxide of carbon as 10 : 8·5. In this mixture the oxygen was combined with 4 volumes of hydrogen and 1 volume of oxide of carbon (No. XI). Finally, in the latter mixture, there are formed by combustion 2 volumes of steam and 1 volume of carbonic acid.

"*Annales de Chimie et de Physique*," July 1, 1853.

(To be concluded in our next.)

MINERALOGY.

Researches on the Products of decomposition of Rocks under the Influence of Thermal Sulphurous Waters.

By M. Jules Bouis.

The study of the decomposition of rocks under the influence of air or water, which Ebelmen had so happily undertaken, now occupies the attention of many chemists and geologists. Several of them have persuaded me to publish the results which I have obtained, with samples resulting from the decomposition of aquiforous rocks. I do it willingly, in the hope that, added to other more important observations, they will one day contribute to a better knowledge of the interior of our planet, and, perhaps, of the formation of silicious and sulphurous waters; for, as says an illustrious chemist, "Waters are species of sounding lines which bring to us from the bowels of the earth samples of the matter of which it is composed."

All the products which I have analysed, and to which I call the attention of the Academy, come from the soils from which issue the remarkable springs of Olette (Eastern Pyrenees), and particularly the source of the Cascade, the temperature of which amounts to 172° F.

Clearing operations having allowed the removal of the blocks of stone which closed the aperture of the source, we detected in the interstices of the rock the effects of the corrosive action of the waters. The spring takes its rise in a felspathic rock, jutting out into a ravine, at the height of twenty metres, and forms at the lower part a kind of vault covered with efflorescences; from an angle of this vault a portion of the water issues with violence, producing a hissing which indicates its compression, and flows out on a rock inclined at an angle of 45 degrees. To collect the samples, it was necessary, clinging to the fissures of the rock, and with the feet on a burning ground, to remain in a sulphurous atmosphere at a high temperature, always in danger of slipping and rolling to the bottom of the ravine.

The rock is profoundly altered by the corrosive action of the thermal alkaline sulphurous water, and, in order to shew the relation which exists between the first matter and the other products, I will point out in the first place the composition of the rock.

It is grey, veined with white quartz; its fracture is dull; it has a density equal to 2·86; it resembles petro silex. Its composition (the mean of several analyses) is as follows:—

Silica	82·6
Alumina	7·5
Protoxide of iron	1·2
Lime	1·5
Soda	4·2
Potassa	0·7
Water	1·6
	99·3

The rock reduced to powder retains a certain quantity of water which it parts with only at a red heat. This circumstance and its composition cause it to resemble the aquiforous granites and petro silex of China, the analyses of which have been given by Ebelmen and Salvétat. By following the excavations made in the rock, I have been able to seize all the phases of the transformation of the rock into silica: the appearance alone of the samples is sufficient for comprehending the phenomenon. We find that the rock takes at first a very bright red tint, due to peroxide of iron, and then cracks as if it had undergone the action of a very high temperature; afterwards it becomes friable and white, and assumes the appearance of pummice stone, and, in fine, to be composed of pure silica.

The analyses of the decomposed rock gave 95, 97, 98, 99·5, and even 100 per cent of silica, according as the state of alteration was more or less advanced. The silica is ordinarily as white as snow, friable and porous; sometimes it is colored with iron and manganese. In the clefts of the rock we find silica under the form of stalactites, forming concentric layers which indicate the result of a deposit. This variety of silica is often covered with crystals of sulphate of lime; it contains sometimes also very small crystals of quartz, which may be quoted by the geologists who are of opinion that quartz has been formed by the way of aqueous crystallisation. The stalactiform silica often presents on the surface a very beautiful green color, due to cryptogamic vegetation. Finally, silica is found in the gelatinous state, and constitutes transparent masses which are often confounded with glairine, to the composition of which I shall shortly recur; or else, covering the vegetables which grow on the rocks, it gives by spontaneous dessication, a grey felty mass, which the most practised eye would take for cardboard.

The silica arises then from two different sources; in one case, it is due to the action of water on the rock, which removes all the other elements; in the other, it is deposited from water which holds it in solution by favor of a high temperature, as lime in incrusting waters is dissolved by carbonic acid. This view is confirmed by the circumstance that porous silica is insoluble in weak alkaline or acid solutions, whilst stalactiform silica is dissolved with the greatest facility in the same reagents.

The water carries away the soluble matter, and there is deposited a reddish mud, acted on by hydrochloric acid, which removes all the iron; white plastic argil remains.

The mud is formed of—

Silica	74·5
Alumina and oxide of Iron .	17·9
Water	7·5
	99·9

This composition is almost identical with that of the deposits formed in Iceland. Dieffenbach and Hooker, found in the interior of New Zealand, a great number of volcanic springs of high temperature which deposited stony matters resembling chalcedony. These deposits analysed by Thomson, gave :—

Silica	77·35
Alumina	9·70
Peroxide of iron . . .	3·72
Lime	1·54
Water	7·66
	99·97

It is evident, then, that the composition is the same, although obtained from different localities. In comparing the analysis of the mud with that of the rock, we ascertain that the potassa and soda, which are very soluble, have been carried away by the water, that the protoxide of iron converted into peroxide has accumulated, as well as the alumina, in the deposit, whilst the silica has diminished ; results which confirm, moreover, the beautiful experiments of M. Ebelmen.

Among the curious products which I have examined, I collected from a rock bathed by the water of the spring of the cascade a crust of 1 to 2 millimetres thick, and easily detached. This substance was very white internally, and of a red color on the surface. Hydrochloric acid removed the red coloration due to oxide of iron ; it presented a radiated crystallisation ; it was very hard. Its composition classes it with the zeolithes, for it agrees exactly with the formula $(CaO, SiO^2 + Al^2O^3, 3SiO^2) + 6HO$.

In fact.		Calculation.	Experiment.
$4SiO^2$ . . .	2267·2	57·6	57·6
Al^2O^3 . . .	641·8	16·3	16·1
CaO . . .	350·0	8·8	8·6
6HO . . .	675·0	17·1	17·6
	3934·0	99·8	99·9

All chemists know the action which the air and moisture exert on hydrosulphuric acid, and some years ago, Dumas shewed, by precise experiments, the influence of porous bodies in these phenomena. Many examples of natural products formed by the combustion of sulphur have been found, but these compounds have not occurred, perhaps, so beautiful and in as large quantity as in the waters of Olette.

In the clefts of the source of the cascade, or under the vault which I have mentioned, are abundant white or yellowish efflorescences. Certain parts are formed of potash alum; others are composed essentially of sulphate of soda. These salts are almost always mixed, and it is sufficient to dissolve them, to separate them from the silica. The solution, evaporated and exposed to the air, deposits crystals of alum, and the efflorescent sulphate of soda is easily removed.

These productions are evidently due to the action of the air on the hydrosulphuric acid, which is disengaged from the waters. The rock exposed to the steam is much disaggregated, becomes porous, and causes the sulphur to pass to the state of sulphuric acid, which aids, in its turn, in attacking the rock by eliminating the alkalis. I am induced to think that the sulphur which is found in these and analogous localities, only shews itself, inasmuch as the rock is not porous. The water charged with alkaline sulphuret, agitated by any cause whatever in contact with the air, deposits sulphur, and I have collected at Olette sulphur in very pretty crystals.

In comparing all these facts with those observed by M. Ch. St. Claire Deville, in the rocks constituting the sulphur mine of Guadeloupe, remembering also that I have noticed the presence of boracic acid in the sulphurous waters, we cannot help remarking the perfect concordance which exists between the phenomena which are presented near volcanoes and those produced near thermal sulphurous springs.

In the Eastern Pyrenees, where thermal sulphurous waters are so abundant, earthquakes were formerly very frequent, and the masses of water which issue daily from the earth, serve, so to speak, as *safety-valves* for preventing upheavings.—*Comptes Rendus*, August 8, 1853.

MINERALOGICAL NOTICES.

WÖHLER finds that many meteoric irons occur in the native form, in the so-called *passive* state, that is to say, their bright surfaces do not reduce a neutral solution of sulphate of copper. Thus, seven specimens examined from different localities were passive; six active, and four were at first passive, but after a time the reducing action commenced, usually starting from a point, or from the margins of the solution. These states seem to have no connection with the presence of Nickel, or the property of forming Widmannstattean figures. Conclusions as to *the cause* of these variations in the action of meteoric irons cannot be formed till more extensive experiments and observations have been made.—Dr. C. M. Wetherill has determined that the color of the red molybdate of lead is not due to the presence of chromic acid, as stated by Mr. N. T. Blake, as after a careful examination he could not find any trace of it. He suggests that this variety may possibly be an acid molybdate, analagous to some chromates, which when neutral are yellow, and when acid red.—M. Becquerel has obtained rhombohedrons of carbonate of lime (*Calcite*), by slowly acting on laminæ of gypsum with bicarbonate of soda of 2°, and trimetric

crystals (*Arragonite*) by using asolution of 6°. This may help to explain the mode of formation of *Arragonite* in the gypseous and saliferous deposits of Spain, Salzburg, &c. *Calcite* was likewise obtained by the action of a solution of potassa of 10° on gypsum in a lightly closed flask, the carbonic acid being derived from the atmosphere.—Malachite ($CuO,CO_2 \times CuO,HO$) was obtained in small silky globules by the action of a solution of nitrate of copper of 12° or 15° on coarse porous limestone, which becomes coated with subacetate of copper, and then plunging it into a solution of bicarbonate of soda of 5° or 6, it is converted into malachite, or, if the action is prolonged, a double carbonate of copper and soda.—By subjecting glass to the action of hydrofluoric acid, M. Leydolt found that far from being homogeneous; nearly all kinds of glass presented distinct, regular, and transparent crystals imbedded in an amorphous base, the latter being most soluble in the acid. Many natural transparent crystals and agates likewise present this phenomenon.

S. H.

ORGANIC CHEMISTRY.

On the theory of the Amides. By M. ADOLPH WURTZ.

THE idea of chemical types, so happily introduced into science by M. Dumas, has lately received considerable developments. My own researches and those of Professor Hofmann, had already demonstrated that many alkaloïds might be regarded as derived from *ammonia*, when M. Gerhardt shewed, in a remarkable work, that the acids are derived from the type *water*. I now propose to demonstrate that the very numerous combinations which are known under the name of *amides*, and which have long been connected with the acids, are derived, indeed, like them, from the type *water*. The views which I now develop, differ, consequently, from those which MM. Gerhardt and Chiozza have recently expressed on the same subject.

By adopting the equivalents generally used, the monobasic acids must be regarded as derived from two molecules of water; the oxygen of these two molecules of water occupies, so to speak, a distinct place in the molecule of the acid itself, and must not be confounded with the oxygen which the complex group substituted for one of the molecules of hydrogen contains. These relations are perfectly expressed by writing the formula of acetic acid, for example, as follows :—

$$\left.\begin{matrix} C^4H^3O^2 \\ H \end{matrix}\right\} O^2$$

This being put, nothing is more easy than to explain the formation, and to represent the constitution of the amides formed by the monobasic acids. Two molecules of the hydrogen of the ammonia remove the two molecules of oxygen placed without the binary group, and the residue NH is substituted for this oxygen.

In this point of view an amide is nothing but an acid in which the two molecules of oxygen of the primitive type, *water*, have been replaced by the residue NH of a molecule of ammonia, having lost two equiva-

lents of hydrogen. This substitution does not modify the general form or type of the combination, which remains perfectly intact.

It is sufficient to glance at the following formulæ to appreciate the simplicity of these relations :—

$\left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}O^2$,	$\left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}NH$,	$\left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}N(C^4H^5)$.
Acetic acid.	Acetamide.	Ethylacetamide.*
$\left.\begin{matrix}C^4H^3O^2\\C^4H^3O^2\end{matrix}\right\}O^2$,	$\left.\begin{matrix}C^4H^3O^2\\C^4H^3O^2\end{matrix}\right\}NH$,†	$\left.\begin{matrix}C^4H^3O^2\\C^4H^3O^2\end{matrix}\right\}N(C^4H^5)$.
Anhydrous acetic acid.	Diacetamide.	Ethyldiacetamide.

It is evident that nothing prevents the substitution of a complex group for the hydrogen of the residue NH. It is by a similar substitution that the ethylacetamide and ethyldiacetamide are formed.

Let us now examine the constitution of the amides formed by the polybasic acids. The bibasic acids are derived, according to M. Gerhardt, from two molecules of water. In the notation generally used, we are obliged to derive them from two binary groups of molecules of water. It may be supposed that in each of these groups one molecule of hydrogen is replaced by an acidifying group, so that a bibasic acid may be regarded as formed by the union of the two conjugate monobasic groups. Hence the constitution of the oxalic acid would be expressed by the following formula :—

$$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}O^2$$
$$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}O^2$$

The amides of oxalic acid are formed by the intervention of one or two molecules of ammonia, and by the elimination of four or two molecules of water. By applying the principles by which we have just been guided, we may conceive the constitution of these amides in the following manner :—

$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}O^2$,	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}NH$,	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}NH$,	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}NH$,	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}NH$, (C^4H^5)
$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}O^2$	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}NH$,	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}O^2$	$\left.\begin{matrix}C^2O^2\\C^4H^5\end{matrix}\right\}O^2$	$\left.\begin{matrix}C^2O^2\\H\end{matrix}\right\}N\ (C^4H^5)$
Oxalic acid.	Dioxamide.	Oxamic acid.	Oxamethana.	Diethyloxamide.

Succinic acid, another bibasic acid, may be regarded as containing two monobasic groups ($C^4H^3O^3$).

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* The body which is formed by the action of ethylamine on acetic ether is identical with that produced by the action of cyganic ether on acetic acid. I have not established its composition by analysis; but, as caustic potassa divides it into acetic acid and ethylamine, I have thought that it might be regarded as ethylacetamide.

† It will be easy to prepare this compound by passing vapors of hydrated cyanic acid into anhydrous acetic acid.

If we place ourselves in such conditions that two equivalents of ammonia may react on this acid, we obtain, in consequence of the elimination of four molecules of water, the compound which it is convenient to call *disuccinamide*. If, on the contrary, the formation of four equivalents of water is determined by making only one equivalent of ammonia react on succinic acid, the hydrogen of the ammonia is not sufficient for the formation of this water. The molecule of ammonia loses in these conditions only two equivalents of hydrogen, to which will be joined the two equivalents of basic hydrogen of the two succinic groups. In these conditions we obtain the *succinamide* of Laurent and Gerhardt; improperly called *bisuccinamide* by M. E. D'Arcet. The following formulæ express the constitution of these amides :—

$$\left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}O^2 \qquad \left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}NH, \qquad \left.\begin{matrix}C^4H^3O^2\\C^4H^3O^2\end{matrix}\right\}NH$$

Succinimide.

$$\left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}O^2, \qquad \left.\begin{matrix}C^4H^3O^2\\H\end{matrix}\right\}NH$$

Succinic acid. Disuccinamide.

To select a last example, I shall now indicate how I view the constitution of the amides of carbonic acid itself, a bibasic acid which must be derived, consequently, from two groups of molecules of water :—

$$\left.\begin{matrix}CO\\H\end{matrix}\right\}O^2, \quad \left.\begin{matrix}CO\\H\end{matrix}\right\}NH, \quad \left.\begin{matrix}CO\\H\end{matrix}\right\}NH, \quad \left.\begin{matrix}CO\\H\end{matrix}\right\}NH \quad \left.\begin{matrix}CO\\H\end{matrix}\right\}N(C^5H^5)$$

$$\left.\begin{matrix}CO\\H\end{matrix}\right\}O^2, \quad \left.\begin{matrix}CO\\H\end{matrix}\right\}NH, \quad \left.\begin{matrix}CO\\H\end{matrix}\right\}O^2 \quad \left.\begin{matrix}CO\\C^4H^5\end{matrix}\right\}O^2 \quad \left.\begin{matrix}CO\\C^4H^5\end{matrix}\right\}O^2$$

Carbonic acid (Hypothetical Hydrate). Carbamide. Carbamic acid. Uretham. Ethylurethane.

We have obtained only a very small number of amides derived from tribasic acids. It appears to me, consequently, useless to complicate this hasty exposé, by pointing out all the cases which it is possible to foresee relative to the formation of these complex amides. But, in order to shew how easy it is to make them enter into the system which has just been traced, I will point out how I conceive the constitution of the anilides of citric acid obtained by M. Pebal. We will suppose the hydrogen of the residue NH, which figures in the foregoing formulæ, replaced by phenyle $C^{12}H^5$, and we shall have the key to all the following formulæ :—

$$\left.\begin{matrix}C^4H^3O^2\\H\\C^4H^3O^2\\H\\C^4HO^2\\H\end{matrix}\right\}O^2 \qquad \left.\begin{matrix}C^4H^3O^2\\H\\C^4H^3O^2\\H\\C^4HO^2\\H\end{matrix}\right\}N(C^{12}H^5) \qquad \left.\begin{matrix}C^4H^3O^2\\C^4H^3O^2\\C^4HO^2\\H\end{matrix}\right\}N(C^{12}H^5)$$

Citric acid. Citranilide. Citro bianile.

$$\left.\begin{matrix} C^4H^3O^3 \\ H \\ C^4H^3O^3 \\ H \\ C^4HO^3 \\ H \end{matrix}\right\} N\,(C^{12}H^5) \left.\begin{matrix} \\ \\ O^2 \\ \\ O^2 \end{matrix}\right\}$$

Bibasic citro monanilic acid, formed by the elimination of two molecules of water.

$$\left.\begin{matrix} C^4H^3O^3 \\ C^4H^2O^3 \\ C^4HO^3 \\ H \end{matrix}\right\} N(C^7H^5) \left.\begin{matrix} \\ \\ \\ O^2 \end{matrix}\right\}$$

Monobasic citro monanilic acid, formed by the elimination of four molecules of water.

The foregoing developments apply more particularly to the most ordinary mode of formation of the amides, that is to say to the action of which ammonia itself exercises, or, if it be wished, to the action of heat on an ammoniacal salt. I shall now demonstrate that the views just enunciated accord perfectly with the other modes of forming the amide.

1. When ammonia reacts on a compound ether—acetic ether, for example—the following is what takes place. The ammonia removes the two molecules of oxygen, which are found beyond the groups; two molecules of water are formed, and react, by double decomposition, on the two groups of ether, in such a manner as to give rise to an amide and to alcohol. The reaction is divided, consequently, in two phases, shewn in the following formulæ :—

$$\left.\begin{matrix} C^4H^3O^3 \\ C^4H^5 \end{matrix}\right\} O^2 + NH^3 = \left.\begin{matrix} C^4H^3O^3 \\ C^4H^5 \end{matrix}\right\} NH + 2HO$$

Acetic ether.

$$\left.\begin{matrix} C^4H^3O^3 \\ C^4H^5 \end{matrix}\right\} NH + 2HO = \left.\begin{matrix} C^4H^3O^3 \\ H \end{matrix}\right\} NH + \left.\begin{matrix} C^4H^5 \\ H \end{matrix}\right\} YO^2$$

Acetamide. Alcohol.

2. When ammonia reacts on the chloride of an oxygenised radical, two molecules of hydrogen are separated from the ammonia: one of them forms, with chlorine, hydrochloric acid, which is eliminated; the other is substituted for the chlorine; and the residue, NH, is united with the binary group modified by substitution.

$$\left.\begin{matrix} C^{14}H^5O^2 \\ Cl \end{matrix}\right\} + NH^3 = ClH + \left.\begin{matrix} C^{14}H^5O^2 \\ H \end{matrix}\right\} NH.$$

Chloride of Benzoil. Benzamide.

3. When cyanic ether $\left.\begin{matrix} C^2N \\ C^4H^5 \end{matrix}\right\} O^2$ reacts on a hydrated or anhydrous acid, or anhydrous acetic acid, for example, the carbon of the cyanogen unites with the oxygen of the ether and with the oxygen of the acid, carbonic acid is disengaged, and the residue N (C^4H^5) of the ether is conveyed integrally to the two groups of anhydrous acetic acid.

$$\left.\begin{matrix} C^2N \\ C^4H^5 \end{matrix}\right\} O^2 + \left.\begin{matrix} C^4H^3O^3 \\ C^4H^3O^3 \end{matrix}\right\} O^2 = C^2O^4 + \left.\begin{matrix} C^4H^3O^2 \\ C^4H^3O^2 \end{matrix}\right\} N(C^4H^5)$$

Cyanic ether. Anhydrous acetic acid. Ethyldiacetamide.

By adopting the views just enunciated relative to the constitution of the amides, the acid properties of certain amides is explained in a

satisfactory manner. It is evident, indeed, that oxaminc acid must be an acid, and a monobasic acid, since it contains integrally one of the two monobasic groupings of oxalic acid. It may well be conceived, besides, that even the amides, which have hitherto been considered neutral, and which do not contain oxygen beyond the conjugate groupings, may, in certain circumstances, exchange the basic hydrogen of the primitive groups of the acid, or even the hydrogen of the residue NH, not only for an organic group, but even for a nutal.

Comptes Rendus, No. 6, August 8, 1853.

ANALYTICAL CHEMISTRY.

On a quick Approximative Method of Estimating Minute Quantities of Iodine. By THORNTON J. HERAPATH.

THE estimation of minute quantities of iodine is not always accomplished without difficulty. I have, however, recently discovered a simple means of effecting this object, which I think is worthy of being generally known. The method in question is based on the mode of analysing silver coin proposed by Gay Lussac, and on that employed by Mr. Horsford for the determination of lead in potable waters; modifications of which processes have been since successfully applied to the quantitative estimation of many other substances. It is the method of graduated solutions. The reagent I use is a salt of palladium, which, as is well known, produces in solutions of iodine and the soluble iodides a brown or brownish-black precipitate of iodide of palladium. When, however, the quantity of oidine is small, the iodide instead of being immediately precipitated, remains suspended in the solution, to which it communicates a brownish tinge, more or less deep according to the proportion of iodine present. Consequently, then, by ascertaining the depth of tint of such a solution, by comparing it with that of standard solutions properly prepared with known quantities of iodine, the proportion of the latter substance that is contained in the matters tested may be ascertained with the greatest exactness.

In an investigation of this kind, therefore, the first thing to be done is to prepare certain standard solutions. 1·309 grain of perfectly pure iodide of potassium—which is equivalent to one grain of iodine—is accordingly dissolved in 10,000 grains of water. This constitutes solution No. 1, and contains a little less than ·01 gr. per cent. of iodine. By diluting this with water, other solutions—Nos. 2, 3, 4, 5, 6, &c.—are formed. The iodine in the substance to be tested having been converted into hydriodic acid or a soluble iodide, the latter is introduced into a colorimeter and diluted with water to the necessary degree—that is to say, until it occupies exactly 100, 500, 1000, 10,000 or more water grain measures; the precaution being taken to first add the palladium solution drop by drop until no deepening of color ensues. The tint is then compared with that of the standard solutions before mentioned contained in tubes or phials of similar diameter, in

which certain known quantities of iodine are contained in the same bulk of water. Though it might rarely be possible to identify it with either one of two solutions in the scale, there can be no difficulty in deciding between which two it should fall, or nearest to which one of two it should be placed.

These standard solutions, it should be observed, may be sealed up in glass tubes, and thus rendered available in future investigations; the only precaution necessary to be taken in such cases being to well agitate the contents of the tubes, so as to again get the precipitated iodide of palladium in suspension in the fluid. Operating in this way, it is possible to estimate the one twenty-thousandth of a grain of oidine with the greatest readiness. It is sometimes preferable to employ but one standard solution. The proportion of iodine in the liquid analysed is then determined by measuring the volume of water that is required to lighten the tint, so as to render it identical with that of the normal solution, or *vice versâ*.

Mansion House, Old Park, Bristol, July 27, 1853.

INDUSTRIAL CHEMISTRY.

On the Purification of Glycerine, and on its Employment in Manufactures. By M. A. CHEVALLIER.

HAVING been requested to examine a memoir by M. Bruère-Perrin relative to the purification of glycerine and its employment in the arts, and especially in perfumery, I now give an account of this work.

It is well known that the discovery of glycerine dates as far back as 1782 or 1783; that it is due to Scheele, who made known that oils and fats contained a saccharine matter, which could be obtained by treating 2 parts of oil and 1 of litharge, adding a certain quantity of water, heating, then separating and purifying the saccharine matter found in the mother-liquors. He published the result of his researches in a work entitled: "*De Materia sacharina peculiari oleorum expressum et pinguedinum,*" a publication which was printed in the "*Acts of the Royal Academy of Sweden*" in 1783. In this publication Scheele gave the name of *sweet principle of oils* to glycerine, founded on the fact that it was saccharine, and that evaporation produced a syrupy product.

The discovery of Scheele was propagated by the scientific journals, especially by the "*Annales de Crell's*" in 1784, then by the "*Opuscules de Chimie*" of Bergmann, translated by Guyton de Morveau. Subsequently, in consequence of chemical investigations, it was established that the oils were compounds of the fatty acids and glycerine; and that the latter, which acts the part of base, is separated by means of soponification.

Glycerine, although well known to chemists, and produced in great quantities since the development of the industrial arts in France, was useless, and considered as a laboratory product, a very curious product, but of no practical use.

The first uses made of glycerine were not in manufacture, but in medicine; in fact, the first use of the sweet principle of oils was in the treatment of affections of the ears by an English medical man. This employment of glycerine having been published, it attracted the attention of medical men; and shortly after, Dr. Startin, physician to the Infirmary for Diseases of the Skin in London, announced its efficacy in some cutaneous affections. These facts being known, experiments were made: 1st. in Paris, at the Hôpital Saint Louis, by MM. Bazin and Cazenave; 2nd., in London, by Mr. Yearsley, and Mr. Wakley, surgeon to the Royal Free Hospital; 3rd., in Russia, by Dr. Dallas, of Odessa, who, without hesitation, proclaimed glycerine as the best of cosmetics. The experiments made by these different medical men proved that glycerine applied to the cutaneous tissue penetrates and softens it; and, moreover, that it assists the cicatrisation of injuries to the skin.

But, to return to the memoir of M. Bruère-Perrin, and the operations intended for the purification of glycerine. It is known that this product has a disagreeable odor, and it has been proposed to purify it by passing a current of carbonic acid through it to precipitate the lime which it still contains. According to M. Bruère-Perrin, this method only removes the lime which is in excess, and not that which is combined with the fatty acids.

M. Bruère-Perrin, to attain his end, has proposed to make use of the following means: 1st., He determines by means of oxalic acid the quantity of lime existing in the liquor which he wishes to purify; 2nd., having determined the proportion of lime, he adds to the liquid a sufficient quantity of sulphuric acid to convert the lime into insoluble sulphate of lime; 3rd., he concentrates it in a tinned copper vessel, stirring quickly during the concentration, using an agitator furnished with arms, turned round and round by the handle; during the concentration there is a disengagement of vapors having a very disagreeable odor, and a partial discolourisation of the liquor; 4th., when the liquid has attained a density of 10° of the areometre, it is allowed to cool and pass through a canvass filter, to separate the sulphate of lime; the excess of acid, should there be any, is then saturated with sub-carbonate of potassa, it is then concentrated with agitation again; the liquor, when it makes 24°, deposits a certain quantity of sulphate of potassa under the form of a gelatinous mass, it is then allowed to cool, passed through a cloth, and the deposit washed with a small quantity of water slightly alcoholised; 5th., it is evaporated a third time, and the liquid is brought to 28° when hot, 30° when cold; it is allowed to cool; in consequence of this cooling there is again a precipitation of a small quantity of sulphate of potassa, which is to be separated by filtration. The product resulting from these operations has an amber color; it is without any marked odor; its taste is sweetish—it is unctuous to the touch. In this state it is treated cold with animal charcoal, then filtered, and colorless glycerine is obtained without any perceptible odor and of a syrupy consistence. Glycerine, like water, mixes with aqueous liquors,

alcohol and vinegar, it softens bodies without greasing them ; like oil, it is unctuous and does not evaporate on contact with the air ; it is easily charged with the aroma of the volatile oils ; it is not susceptible of becoming rancid or of fermenting.

Such is the product that M. Bruère-Perrin presented to the *Société d'Encouragement.*

M. Bruère-Perrin has introduced glycerine into toilette soaps. He used it for the preparation of a cosmetic vinegar, aromatic spirits, and various other kinds of perfumery. We have ascertained that the glycerine soap preserves its original consistence : it renders the skin soft. We have used pure glycerine for washing hands, and glycerine vinegar in the cases in which cosmetic vinegars are used, and we have thought very well of it. We think from what we have said that the application of glycerine to the art of perfumery is a very happy idea.

M. Bruère-Perrin has been of great use to manufactures : 1st., by perfecting the processes for the purification of glycerine ; 2ndly, by utilising a product hitherto without any important application.

We cannot conclude this report without mentioning that M. Barreswill, in one of the sittings of the Society, made known that he had successfully used glycerine to preserve a proper moisture in modelling clay. (*See* THE CHEMIST, Vol. iv.) We think that glycerine might be introduced into the size which is used in the fabrication of cloth, to preserve to the threads of linen or hemp the suppleness necessary in weaving them ; anyhow, this experiment would be worth trying.

Journal de Chimie Médicale, August, 1853.

Demonstration of the Capabilities of the Fire Annihilator.

IF the study of Chemistry had within itself no interest or attraction whatever, and was not even capable, like Mathematics, of enlarging the mind and developing the ideas, still its claims to public support and consideration, on the score of practical utility must insure it no ordinary share of attention in every manufacturing nation. Confining ourselves therefore to this last and lowest view of the importance of chemical study, let us see what has been done of late years towards the progress of civilisation by this means. We have the Daguerreotype, the Calotype, the Electrotype, the Electric Telegraph, Gun Cotton, Chloroform, and, in fact, a chain of interesting and surprising results too lengthy for us to mention link by link, and yet severally these links have conferred upon mankind within the space of a few months greater and more immediate advantages than most of the other sciences put together have been able to produce in the same number of ages. This is no idle boast, nor is the progress of useful invention arrested or likely to be so in the same direction ; even whilst we write, Chemistry is at work subduing the laws of nature to man's use, and converting the elements of destruction into agents of production, or, by skilful antagonism, rendering that which would be doubly pernicious, simple and harmless. We have been forcibly led to the above remarks by a series of experiments upon the extinction

of flame made a few evenings since at the gardens of the Royal Pavilion Hotel, North Woolwich, by the "Fire Annihilator Company." The words, Fire Annihilator, as applied to the instrument used on that occasion, do not as we think correctly describe the operation, and real use of the machine; the expression Fire Preventor would be better, for this is precisely the effect obtained. It nips conflagration in the bud, and secures an easy victory over an infant enemy against whose terrific force at maturity human efforts are vain and hopeless. But as "a rose by any other name would smell as sweet," so Fire Annihilator, or Preventor, or whatever else it may be called; for this is all one to our present purpose, which is merely to record what we saw, and offer a tribute of applause to the effects produced. This Fire Annihilator, then, is an instrument really capable of producing the most surprising results in the world—results which must be seen to be believed, and comprehended to be fully admired; the fiercest flame, the most terrific conflagration, the most intense heat, are all as easily subdued by the power of the Annihilator, as a farthing rushlight is capable of being put out by an extinguisher. The action looks like that of magic, and fully realises the bombastic despatch of Cæsar, "*veni, vidi, vici.*"

If our readers will picture to themselves a series of seven iron tanks, each 7 feet long, and 5 feet wide, filled respectively with pitch, tar, resin, sugar, oil, tallow, naphtha, turpentine, and other highly inflammable matters, all in the most intense state of ignition, and emitting volumes of blazing sparks and flame many yards in length, breadth, and height, with such a degree of heat as to render all approach both uncomfortable and dangerous, then will a faint notion have been formed of the primary condition of experiment No. 1, as exhibited to a crowded audience at the Royal Pavilion Gardens, on the evening of the 22nd ultimo. We must confess that, in common with a large proportion of the company present, we had given way to an impression that for once the powers of the Annihilator had been over-estimated by its Promoters,—a sharp breeze was blowing at the time, the fire roared and raged like a lion, the pitch, oil, &c., boiled and bubbled in the tanks like lava in the crater of a volcano. The glare of light and radiation of heat were absolutely painful at a distance of several yards; and even the grass sward, for many feet around the tanks, was charred and ignited by the radiant caloric. Just at this juncture two stalwart individuals made their appearance, each bearing an Annihilator, of the size and shape of a small gas stove; the signal was given, and instantly a dense column of vapor issued from each instrument, at the mere touch of which the flames retired, and permitted a nearer approach to be made with ease and safety. The vapor still issued, and being now brought into actual contact with the blazing tanks, these one by one became extinguished, and within two minutes from the first approach of the Annihilator, not a spark or vestige of flame or fire was visible in any of the tanks. In short, the success was complete and perfect, and elicited thunders of applause from the audience, many of whom had felt far from comfortable under the terrifying

effect of the conflagration. This experiment was afterwards repeated with a slight variation, when one Annihilator subdued all the flames in nearly the same time as in the former with the two. The hold of a vessel, of 150 tons burden, was filled with all sorts of combustibles, and, after being allowed thoroughly to ignite, the machines were applied with instant effect; and we are fully convinced the Annihilator must soon be universally adopted by the public at large, and occupy its true position in our social system, as one of the most useful and indispensable instruments of civilised life. We are told on all hands that "prevention is better than cure," and in conformity with this adage, the ancient Egyptians were in the habit of destroying the eggs of the crocodile, as being a safe and effectual mode of retarding the increase of that reptile. On the same principle, we most confidently recommend the Annihilator—it crushes the egg of the fiery enemy, and, as it were, "takes time by the forelock." In all confined localities, such as cellars, warehouses, ships, &c., a paternal government would do wisely to enforce its adoption; and, from the experiments we had an opportunity of beholding on the occasion alluded to, we have no hesitation in declaring that, no matter how open or exposed the situation may be, the Annihilator will certainly and safely extinguish the most serious fire, if applied in reasonable time, and in quantity proportioned to the urgency or extent of the mischief going on.

In conclusion, we recommend all candid inquirers to see for themselves, and form their own opinions concerning the value of this invention.

AGRICULTURAL CHEMISTRY.

On the Guanos of Commerce. By M. J. GIRARDIN, of Rouen, Correspondent of the Institute.

IN consequence of a communication made by me on the 11th of November, to the Central Society of Agriculture of the Seine Inférieure, on a sample of guano coming from Havre, and which presented none of the characteristics proper to the fertilising substance which comes to us under that name from Peru, that Society requested me to make a comparative examination of the various manures sold under the name of *guano* in Havre, so as to warn farmers to be on their guard against a new species of fraud which appears likely to be substituted for that of the so-called *concentrated manures*, to which full justice has already been done.

It is all the more necessary to investigate the trade in manure, as our farmers, at length overcoming their habitual repugnance to novelties, have adopted the use of guano, and consume considerable quantities of it. The following are some statistics which shew the importance to which the importation of this article has attained during the last few years.

In the decennial table of the trade of France with its colonies and Foreign Powers, published by the custom-house authorities of the years from 1837 to 1846, no indication of guano is given before 1845. From

this year the importation of this article has been as follows to 1851 inclusively:—

	Kilog.		Francs.
1845	6,721,110,	representing in value ...	537,689
1846	3,129,990	„ „	250,399
1847	1,505,471	„ „	120,438
1848	5,327,432	„ „	799,115
1849	3,924,712	„ „	784,943
1850	2,699,249	„ „	539,850
1851	3,834,048	„ „	766,810

It has come from the Sardinian States, the Western Coast of Africa, Mauritius, United States, Brazil, Peru, Chili, Rio de la Plata, and other countries.

In England the present consumption of guano from Peru is alone far more considerable, as it amounts to at least from 80,000 to 100,000 tons, that is to from 81,252,000 kil. to 101,565,000 kilog. (The English ton containing 1,015·65 kil.)

When guano was first tried as manure in Europe, we only knew of that from Peru, collected in the Isles of Chinche, near Pisco, then in those of Iza, Ilo and Arica, farther south. In 1841, a society, called *Peruvian*, having been formed in Lima, of French, English, and Peruvian houses, obtained from the governments of Bolivia and Peru the monopoly of the exportation of guano, and introduced it into Europe. Some years after, immense deposits of guano were found on the south-west coast of Africa, in the dependencies of the colony of the Cape of Good Hope, at the Isles of Ichaboë, Angra-Pequeña, Malaga, &c.; and although this African guano was inferior in quality to that of Peru, English vessels went in such numbers to the African islands, that these deposits were very soon exhausted. The desire of obtaining large profits, and competing with the Peruvian Society, induced both English and French merchants to seek deposits of guano in every direction. It has been found at Cape Tenez, in some islets near Algeria, as well as on the coasts of Labrador, the Egg islands, and on the coasts of Patagonia. It now arrives from all these places, and other little-known localities, but, in all these cases, these new guanos are very inferior as manure to those of Peru, the quality of which is always alike, when not altered by long exposure to the air, or designedly adulterated.

The importers of guanos carefully avoid mentioning whence they come, so as to make purchasers believe that what they sell is *Peruvian guano*, whose powerful effects are well known, and one proof that they wish to deceive consumers in this respect is, that in their advertisements and notices, they designate their products as *Peruvian guano*, or as *guano of the first quality*.

We certainly cannot prevent merchants from seeking guano elsewhere than in Peru, but they ought to be obliged to sell their merchandise under specific names which would not enable them to confound them with a superior produce, whose nature is well defined and has been long known for its peculiar characters.

The following are the distinctive characters of the Peruvian guano.

It is in a dry powder; the color is a pale yellow or that of coffee with milk in it, but it becomes of a chocolate tint as it ages, or with exposure to the air; it absorbs, besides, in the latter case, a great deal of moisture, becomes heavier, and sticks to the fingers. It exhales a powerful putrid or ammoniacal odor, which causes sneezing. It has a very marked saltish taste. There are, among the mass, numerous whitish concretions, which are rather hard, but will crush between the fingers, and which, when exposed to the air, soon fall into powder, exhaling a very strong ammoniacal odor.

When thrown into water, Peruvian guano sinks rapidly, leaving no portion floating. When heated, it turns black, burns with a slight flame, producing a powerful ammoniacal vapor; it leaves a porous scoria of a slightly blue white color; the weight of this residue varies only between the following very near numbers, 27·50 to 35 per cent.

When triturated with quick lime in powder, Peruvian guano diffuses a powerful ammoniacal odor. When thrown into a glass containing a concentrated solution of chloride of lime, it causes immediately a disengagement of bubbles, which continues for some time. Put in contact with hydrochloric acid, it produces only a slight effervescence. Moistened with nitric acid and dried in a porcelain capsule, it becomes of a beautiful red color.

Finally, this guano very rarely contains silicious stones, and it contains only from 1 to 1·50 of sand; the maximum is from 2·50 to 3 per cent.

All these qualities render it easy to distinguish the Peruvian guano from the guano from other sources, for the latter shew very marked differences, if not in all, at least in some of their properties, as we shall see.

For the purpose of fulfilling the intentions of the Agricultural Society, I procured from Havre thirteen samples of guano brought by as many different vessels, and intended for sale to farmers.

The following are the qualifications and origin of these various samples:—

No. 1.—Bag of about 2 kilog, marked Borneo, and a seal having on one side these words—*Guano of the Peruvian monopoly*, and on the other, *Houtain and Co.*

This guano, imported in the ship *Bon Père*, is of a dull reddish colour; it contains numerous very large concretions; its odor is strong, its flavor is sharp and saltish.

No. 2.—A bag, having on both sides *Nélie et Mathilde*, and below, *5 kilog.* At the opening of the bag was a seal, having on one side *Guano of the Peruvian monopoly*, and on the other, *Houtain and Co.*

This guano, brought by the ship Nélie and Mathilde, is of a decided brick color; it is soft to the touch, a little moist; it contains a great many concretions; it has a powerful odor, its taste is sharp and salt.

No. 3.—A bag, on which was written *Borneo*, with a seal having on it the same words as the preceding two.

This guano, imported by the ship Borneo, was the color of coffee

with milk in it; it appeared rather moist to the touch, its odor is strong, its taste sharp and very salt.

No. 4.—*White guano of Bolivia*, brought by the ship *Emile.*

It is of a light color, very moist to the touch; it contains many voluminous concretions, whitish internally; remains of feathers are discovered in them, and even entire small feathers; its odor is very ammoniacal; the taste is sharp and salt.

No. 5.—Guano, imported by the ship Bombay, said to be a mixture of white Bolivian guano and guano from Chili.

It has a reddish color, contains numerous whitish concretions with abundant animal and vegetable debris; it has a slightly ammoniacal odor, a salt and sharp taste.

No. 6.—Guano designated *Chili lag;* origin unknown.

It is of a chocolate color; it contains some small concretions, no organic detritus, it is very hygrometric, it has a slightly ammoniacal odor, a salt and sharp taste.

No. 7.—Guano, called *yellow Chili;* origin unknown.

Is of a pale brownish yellow color; it is very moist; it contains no concretions; there are some feathers in it; it has no odor; the taste is salt and very slightly sharp.

No. 8.—Guano, said to be from Patagonia. In a very coarse bag, having on one side a *P* and an I. Ship not mentioned.

It is of a very pale color; contains many concretions, it is but slightly moist to the touch, its odor is powerful, its taste is salt and sharp.

No. 9.—Guano, said to be from Patagonia. Bag made of ticking, having on one side a *P* and a 2. Ship's name not given,

Its color is pale and dull, it contains some concretions; it appears slightly moist, it is harsher to the touch than the preceding; it has scarcely any odor, and the taste is much less decided than the above.

No. 10.—Guano, brought by the ship *Ducouédic*, and considered as a species from Chili.

It is of a yellowish brown, it contains many concretions and pebbles, with various organic debris, such as feathers, small branches, &c. It appears to be very slightly moist. It has neither odor nor taste.

No. 11.—Guano, imported by the ship *Ave Maria.*

Color of coffee and milk, contains numerous concretions and pebbles; the fracture of the concretions presents many brilliant points; it contains considerable vegetable detritus and many feathers. It has no odor, and the taste is slightly salt.

No. 12.—Guano, imported by the ship *Edwige.*

Of a light bay color; few and small concretions, no organic detritus. Neither odor nor taste.

No. 13. Guano, from Watchman's Island, Patagonia, imported by the ship Bayard.

It is of a grey color, contains a great number of small round pebbles, and others not less numerous, which are immediately recognisable as carbonate of lime: there are likewise found in it considerable quantities both of ligneous debris and hairs. No odor, an earthy taste.

Before proceeding to the chemical analysis of these guanos, I subjected them to a mechanical test, to determine the relative proportions of silicious pebbles, friable concretions and powder contained in them. For this purpose they were sifted in a tin sieve, the round holes of which were half a millemetre in diameter. The fine powder passed through alone. What remained in the sieve was bruised in a mortar and again sifted, there only remained the pebbles themselves. It is evident that these would be of no use as manure, and that the fewer of them there are, the better the guano will be.

The following are the results thus obtained:—

	1	2	3	4	5	6
Fine powder . . .	62·8	64·4	68·2	57·0	64·76	89·75
Friable concretions .	30·2	21·6	25·8	27·4	22·39	5·75
Silicious pebbles . .	7·0	14·0	6·0	15·6	12·85	4·50
	100·0	100·0	100·0	100·0	100·00	100·00

	7	8	9	10	11	12	13
Fine powder . . .	100·0	35·4	41·0	83·6	34·1	80·0	60.00
Friable concretions .	—	47·2	33·8	9·4	33·3	14·1	27·35
Silicious pebbles . .	—	17·4	25·2	7·0	32·6	5·9	12·65
	100·0	100·0	100·0	100·0	100·0	100·0	100·00

We shall now briefly shew how the chemical analysis of the preceding guanos was performed.

"*Journal de Pharmacie et de Chimie*," August, 1853.

(To be continued.)

PHOTOGRAPHY.

New Process of obtaining Positive Proofs of all dimensions, and with all the delicacy of which Negative Proofs are susceptible. By M. J. J. Heilmann.

Up to the present time the high price of cameras of large dimensions and the difficulty of transporting them from place to place in the country, have greatly hindered photographers from obtaining large negative proofs of *views;* as for the *portraits*, the difficulty of getting them of large dimensions is produced from the fact, that placing the object near injures the perspective, and that as the time of the sitting is limited by the mobility of the subject, it is impossible to employ lenses with a distant focus. Moreover, these negatives being faulty from their small dimensions, never give in the positive form all their fineness, in consequence of the mode of re-production employed. I believe that I have obviated all these defects, and have produced some improvements in photography, by employing a new process for reproducing the positives.

The author operated by means of an apparatus, the description of which it would be useless for us to give, as it would not be understood

without the figure; it will suffice for us to mention that a lense, placed in the middle of the apparatus performs, in some respects, the part of the refracting media of the eye, in such a manner that the negative image is painted on the sensitive plate on which the positive image should be formed, in the same way that external objects are represented on the retina. If the negative image is at the same distance from the lense as the sensitive plate, the positive will be of the same dimensions as the negative; it will be smaller, on the contrary, if the distance between the sensitive plate is less, and *vice versâ*.

The dimensions of the negative, continues the author, are therefore of no importance for the dimensions of the positive, and a camera may be carried in the pocket which is intended to produce landscapes as large as paper will permit. Setting aside the increase of size, the delicacy will be improved in equal measure, because the rays which traverse the negative being regulated by the lense, preserve their original direction, and give an impression exactly symmetrical to that of the negative, whereas, by the ordinary process, the thickness of the negative, exposed to a diffused light, prevents the perfection of the positive; this thickness is of no importance here, being so small in comparison to the distance of the negative from the lense.

Negatives may, in this instrument, be exposed in either way, so as to make positives either on or under the glass.

The new method which I propose, has the advantage of preserving the negatives quite intact, easily as they injure (especially those on collodion), as it is unnecessary to place them on a piece of paper.

For the same reason I can take positive proofs on moist paper, on collodion, and on surfaces which are neither flexible nor elastic, such as porcelain, ivory, glass, &c.; these last being transparent positives, may be used in the transparent stereoscope, magic lanterns, &c. My process will, perhaps, be an assistance to those who wish to make a negative impression on moist metallic plates which are intended to serve for reproduction, by ordinary pressure.

Mr. John Stewart, an experienced photographist, who has assisted me very much in my experiments, has suggested to me a very advantageous mixed process, for the production of positives in considerable numbers and at a low price; it is, to employ a positive on glass made by my process, and to make a good many *negatives on paper* by the same process; positives might thus be obtained in unlimited numbers, still retaining the original negative unhurt. The two operations being made transparently, through a very thin coat of collodion, the loss of fineness would be imperceptable.

Comptes Rendus, No. 4, *July* 25*th*, 1853.

FORENSIC CHEMISTRY.

The Torbanehill Mineral Case—Summary of the Chemical Evidence.

In this case, Elizabeth Honyman Gillespie, heiress of entail in possession of the lands of Torbanehill, in the parish of Bathgate, and county of Linlithgow, and William Gillespie, her husband, were the

pursuers, and James Russel and Son, coal masters, Blackbraes, near Falkirk, the defenders.

By an agreement for a lease, dated March 20, and April 1, 1850, entered into between the pursuers and defenders, it was arranged that the former should grant to the latter a lease of "the whole coal, iron stone, iron ore, limestone, and fire clay, but not to comprehend copper or any other mineral whatsoever." The plaintiffs alleged that although in the course of their operation, the defenders had come upon iron ore and iron stone, coal and fire clay of workable value, they had neglected these, and had chiefly worked a certain mineral substance, which the plaintiffs contended, was not let to the defenders, not being, as they averred, one of the mineral substances specified in the agreement. This mineral was of much greater value in the market than any of those set forth in the agreement, the lordships or royalties on which were fixed at 6*d.* per ton; whereas, we suppose that the pursuers, had they gained this action, would have demanded a much higher royalty for the mineral in dispute. The pursuers alleged that the mineral was not coal; that its chemical, microscopical, and mineralogical characters were not those of coal. The defenders asserted that it was coal; that it was let by the agreement, and, in fact, that they had been led to enter into this agreement by the knowledge that on the adjoining lands of Boghead this mineral existed, and was worked and sold as coal, being known in the market as the Boghead gas coal, and from a belief that the same mineral would be found on the Torbanehill lands. They contended, likewise, that it was coal, a true Cannel or Parrot coal.

Perhaps on no occasion have so great a number of men of science been arrayed as witnesses; perhaps never was such an antagonism of evidence as to the nature of a substance so familiar as coal. Certainly it is devoutly to be hoped that never again will science be made to appear unworthy of confidence as a guide.

In the course of the trial, which lasted six days, the following Chemists, Geologists and Mineralogists were examined:—

For the pursuers.—Chemists: Mr. Brande, Dr. Anderson, Dr. G. Wilson, Mr. J. T. Cooper, Dr. John Lee. Mineralogists and Geologists: Mr. Ansted, Mr. D. M. Holme, Mr. E. J. Chapman, Mr. Hugh Miller, Mr. Alexander Ross, Rev. Dr. Anderson.

For the defenders.—Chemists: Mr. Graham, Dr. Stenhouse, Mr. Johnston, Dr. Hofmann, Dr. A. Fyfe, Dr. D. Maclagan, Dr. Gregory, Dr. Frankland, Mr. R. Penney. Mineralogists and Geologists: Dr. Fleming, Mr. Jos. Jukes, Mr. Ramsay, Mr. Maclaren, Mr. Harkness.

To give the whole of the conflicting portion of the evidence is impossible; we therefore refer such of our readers as may be desirous of a more perfect acquaintance, to a verbatim report * by Mr. A. W.

* We have received a copy of the Report from the pursuers; the defenders are about to publish a Report, but they have informed us that it will not be ready for some weeks. We have to thank Mr. Gillespie for a sample of the mineral. We have also received samples from other quarters.

Lyell, shorthand reporter, and published by Messrs. Bell and Bradfute, Edinburgh, a perusal whith will not enhance their opinion of those professors of science who had the sad misfortune of appearing as witnesses.

We wish it to be distinctly understood, that in our remarks we are most desirous of wounding the feelings of no one: it is our duty to make some comments on a case which has shaken the confidence of the non-scientific public in the efficiency of science and the honesty of its professors.

The following is a very condensed report of the chemical evidence, to which alone we are confined by want of space.

The first witness called was *Mr. D. T. Ansted*, consulting mineral engineer, and late Professor in King's College, London, who was examined by the Dean of Faculty He said that he considered the mineral in question not to be a coal, and not to possess the essential elements of coal. It had not a sufficient quantity of carbon to give fuel after distillation, and its basis was not carbon, but clay. It was valuable for the great quantity of gases it contained, but he was not aware that it possessed any other value, for when the gases were extracted from it, the virtue was gone out of it.

Professor Brande, had examined specimens of a mineral found at Torbanehill, which he had received from Mr. Rettie and Mr. Forbes. From Rettie's specimens he had got 30 per cent. of carbon and ash; this carbon he denominated residuary, or fixed carbon. Of volatile matter he had obtained 70 per cent. From another of Rettie's he had got 28 per cent. of carbon and ash, and of volatile matter 72. In Forbes's, he had got 33 carbon and ash in one case, and the remainder volatile matter; and in another 31 and 69. The average of the whole, ascertained by taking a portion of each specimen, was of 10 of carbon, 20 of ash, and 70 volatile matter. He had subjected 23 specimens of Newcastle coal to destructive distillation, and the average result was 77 carbon and 2 ash, and 21 volatile matter. The residue of the Torbanehill mineral was materially different in character from the residue of coal. The carbonaceous residue of the Torbanehill mineral was a pulverulent sort of charcoal, while the other left ordinary coke. As regards the earthy residue of the Torbanehill mineral, it was distinguishable from the earthy residue of coal chiefly from its very large quantity, and by the absence, or almost entire absence, of salts of lime. It was also distinguishable in color, the ash of coal being generally brown, from the large quantity of iron it contains, while this contains merely a trace of iron, and is white. He afterwards instituted another series of experiments in conjunction with Mr. Cooper, of London, for the purpose of comparing the Torbanehill mineral with the Leshmahagow and the Marquis of Lothian's coals. In the case of the Leshmahagow coal the result was 41 residuary carbon, 3 ash, and 56 volatile matter. The Marquis of Lothian's gave between 44 and 45 carbon, 2·4 ash, and 53 volatile matter. The Torbanehill mineral gave 55 carbon, 5 ash, and 40 volatile matter. There was no difference between the experiments he had made with Mr. Cooper and those which he had formerly made. They had also subjected the Torbanehill mineral and the coal to the process of digestion in naphtha. With the coal they obtained a brown tincture; with the mineral the naphtha remained pure in color. In the case of coal the watery products were alkaline from the beginning; in regard to the mineral, they were slightly acid in the first instance. The cause of the alkaline character of these watery products in the coal is the formation of ammonia in consequence of the presence of hydrogen and nitrogen. In the other case, there was a slight formation of acid, which he believed to be acetic acid, and which is formed during the process of distillation by the union of carbon, hydrogen, and oxygen. In the latter case there was no nitrogen. The gases produced by the Torbanehill mineral were principally varieties of carburetted hydrogen, which is also produced by the distillation of coal. As the result of all these chemical experiments, he considered that the

Torbanehill mineral was certainly not coal, and, as a chemist, he clearly pronounced it not to be coal.

Cross-examined.—The Newcastle coal was indiscriminately used for household coal and for gas. In this case the *fixed* carbon, according to the statement he had already made, was 10 out of a total of 100. *He could not tell the* TOTAL *amount of carbon in* 100 *parts of the Torbanehill mineral, as he had not made an ultimate analysis of it.* But he knew that other chemists had made it. There was *certainly more carbon* than 10 per cent. Of the 70 of volatile matter, a *considerable proportion* must have been carbon. There was a small quantity of fixed carbon as compared with the acknowledged coals which he had examined. He considered that the small quantity of fixed carbon necessarily implied a small quantity of total carbon. It might, however, arise from there being a greater proportion of hydrogen in the mineral. After all, the hydrogen had combined with the carbon and become exhausted, the unexhausted carbon remained as fixed carbon. The more carbon went off the less remained; and the more gas the less fixed carbon. In the other experiments referred to, there was a very minute quantity of coloring matter taken up from the coal which did not exist in the Torbanehill mineral. He presumed it to be bitumen.

Q. Now, Mr. Brande, you say that you do not consider this is a coal; what do you consider that it is?—*A.* A peculiar mineral.

Q. Anonymous?—*A.* Well, I never saw a specimen of it before, and, therefore, I cannot—being no mineralogist—pretend to say.

Q. It is, then, a peculiar mineral, to which you cannot give a name?—*A.* No.

Q. The other ingredients of coal also vary in different coals?—*A.* Yes.

Q. Are they very various?—*A.* Yes.

Dr. Anderson had examined the Torbanehill mineral, as to its mineralogical and chemical characters. Coal is a black, carbonaceous mineral, with a resinous or waxy lustre, a black and lustrous streak, and a conchoidal fracture. This applies to bituminous coal only. Bituminous coal, however, is only one mineralogical division of coal, which is divided into anthracite, bituminous coal, which are the true coals; and brown coals, a distinct variety, also called lignite. Anthracite is hard and black, with an exceedingly brilliant lustre, and consists principally of carbon; the quantity of carbon in it is very great, and almost entirely fixed. Lignite is exactly like a piece of charred wood, like a piece of imperfectly-made brown charcoal, with carbon much smaller than in anthracite, but still considerable. Lignite is clearly a vegetable formation; hence its name. It is found in the tertiary formation, not in the coal measures. Bituminous coal is divided into cherry coals, splint coals, caking coals, and gas coal or cannel. Mineralogically speaking, cannel coal is black, sometimes with a greyish or brownish shade in it; its streak is black and lustrous; and its fracture, described mineralogically, is large conchoidal; its lustre is resinous or waxy, and it polishes. With regard to the mineralogical character of the Torbane mineral, its color is brown; its streak is pale fawn-color, and perfectly dull, with no appearance of lustre. Its fracture is somewhat slaty, that is, parallel to the stratification, and *not* conchoidal. It does not polish at all. In arriving at the results he had now stated, he had examined the West Wemyss and the Cappledrae cannels, which come nearest in their mineralogical character to the Torbane mineral. There are important differences between them and the Torbanehill. The Wemyss cannel is black, with a resinous lustre; the streak is black and lustrous, and the fracture is conchoidal. He had also examined a mineral which is found at Methill, in Fifeshire, specimens of which approximate in some respects to the brown color of the Torbanehill mineral. Its streak has a somewhat brownish shade, but it is lustrous, as regards individual specimens; but if he was to state the general appearance of the coal, it gave a black streak. He had also examined the Torbanehill mineral with reference to its chemical qualities and composition. An ordinary average specimen, taken from the central part of the seam of Torbanehill, contains 64·02 per cent. of carbon, 8·9 per cent. of hydrogen, 0·55 per cent. of nitrogen, 0·5 of sulphur, 5·66 of oxygen, and 20·32 of ash. The main ingredients were the carbon and the ash; but the hydrogen was important also. In the upper part of the seam the ash is very much higher, there being 32·3, nearly a third part of the whole mineral. In the lower part of the seam he found it to be 38·54. The ash found, on the average, was 30·38 per cent. of the whole mineral. The average

of the carbon of these different parts was about 60 per cent. It varies considerably, but it is not above 60 per cent.

Cross-examined by Mr. Penney.—Anthracite was, in mineralogical language, not strictly a coal, but was so in common language. Lignite was also commonly called coal, but not rightly so. It was a distinct species, and not a true coal. The amount of ash in coals differed very considerably. The ash never exceeded a third of the coke. When the amount of the volatile matter thrown off was great, that made the coke less. Bituminous shale was not a coal. Shale was a clay impregnated with a coaly or bituminous matter which might be extracted from it as from bituminous coals. The component parts of the shale were the same as coal. The chief difference was in its structure as to splitting. Its split was parallel to the stratification. Most coals split with a cross fracture. Shale had no cross fracture. A large percentage of ash was a distinction between coal and shale. The Torbane mineral was not a shale. He could not give it a name.

Re-examined.—He could not give it a name, because it was a new mineral, and had not received one. The nomenclature of mineralogy was receiving additions every year. The chief distinctions of shale from coal was that the specific gravity was different. Shale presented an amorphous appearance under a microscope, and betrayed no symptom of organic structure, and had a clayey and not a carbonaceous base. While in ordinary coals the ash never exceeded a third of the coke, in this mineral the rule was reversed, and the carbon was under one-third.

Dr. George Wilson, considered the Torbane mineral a clay, largely impregnated with bitumen, and did not regard it as a coal. He had made chemical analyses, with Professor Miller, of thirteen Scotch cannels, and was examined upon them. The principal characteristic of the Torbane mineral he considered to be, that it left no available coke. He thought that no substance could be called coal unless it gave a considerable residuum of coke. In cross-examination he stated that some substances which went to compose coal might be found in the Torbane mineral, though in various degrees and arrangements. Coal was a mixture of different compounds, and, with the exception of anthracite, was not a homogeneous mass.

Mr. J. T. Cooper was examined as to some experiments he had made some years ago, on the Kimmeridge coal, or shale, with the view of shewing that the Torbanehill mineral was not coal; but he does not appear to have examined this mineral, which strikes us as curious.

Dr. John Lee, stated that the mineral yielded gas of a highly illuminating power, and of large quantity, 14,000 cubic feet of gas per ton; whereas the best cannel he was acquainted with, the Wigan cannel, produced only 11,500. The mineral yielded much more tar than any coal he knew, but much less ammonia. He did *not* consider the mineral as coal, but supposed it had been so called from also producing gas.

Professor Graham, for the defenders had examined specimens of the Torbanehill mineral; the ash in each of these was 19, 20, 22, 22, 28, and 30 per cent. respectively. This ash did not bear a fixed proportion to the specific gravity of the mineral, one specimen which gave 22 per cent. of ash having a specific gravity of 1·28, and the specimen with 30 per cent. of ash having a specific gravity of 1·22. From this he inferred that the combustible part of the coal was variable in composition; the density seeming to be more affected by the combustible material than by the ash. He concluded also that the substance was not homogeneous. There was a great variation in the amount of ash in coals, that ingredient varying from under half to very nearly 50 per cent. Professor Graham then described several experiments which he had made on the Torbane coal with reference to its coking quality, stating that when subjected to cherry-red heat for an hour, it gave 41·8 per cent., and for five hours 37·9 per cent. of true coke. The first coke contained 47, and the second 41·9 of combustible matter; when turned red-hot into the air, it burned like charcoal, although evidently an inferior fuel. The ash resembled that of other coals. On reducing the mineral to powder he found that it contained the merest trace of bitumen, agreeing in this respect with the acknowledged bituminous coals. The other ingredients were also such as were found in these coals. He therefore pronounced the mineral to be decidedly a coal.

Dr. John Stenhouse, considered that the substance in question contained a great deal of coally matter; it was not more marked with bitumen than any other coal.

It was undoubtedly a coal, very similar to ordinary parrot coals, and he had only been astonished how any question should ever have been made about it.

Professor Johnston, had recently examined the Torbanehill coal field, and found the usual stratification of coal measures. The beds above and below the mineral in question were those usually found in the coal measures. He also observed traces of vegetable remains; and the origin of the mineral he regarded as being the same as that of other coals—decomposed vegetable matter. From its geological appearance, then, he *had no doubt* of its being a coal. Its color was peculiar; some portions of it were lighter in the streak than other varieties of cannel coal, while other parts were darker and resembled the Methill and other coals from Fifeshire. There was, however, a mineral called coal which was still lighter than this mineral; he referred to the "paper coal" from the banks of the Rhine. Cannel coals were generally lighter than other coals, but some of them were quite as dark. In this case some parts were lighter and some darker; still he came to the conclusion that it was coal. He had made an analysis of it, and found 21·3 of ash in the black and 24·3 in the brown specimens. The quantity of ash differed considerably in different coals; the best Durham containing only one-half per cent., while he knew, from his own examination, that at New Brunswick there was a description of coal which contained 19 per cent., and another on the Missouri which contained from 30 to 33 per cent. The other ingredients of coal also differed considerably. On the whole, he had *satisfied* himself from the general appearance of the mineral in question, *that it was a coal;* nothing in his experiments had led him to doubt it, but, on the contrary, everything had tended to confirm his opinion.

Dr. Hofmann had made some experiments on the Torbanehill mineral, and found its components similar to those of coal. Its ash contained the same ingredients, and its combustion agreed in character. He had subjected it to the usual solvents and tests for ascertaining if it were a mixture of bituminous matter, but he only discovered the merest trace of bitumen, and it was not reduced to a fluid state as would have been the case if it was bitumen. Looking at the composition and character of the mineral as a scientific Chemist, he considered it an undoubted coal. The ingredients of coal varied considerably, but carbon was the largest ingredient, and from 100 he extracted 65·66 parts of this substance. He also found that it contained about 20 per cent. of earthy matter, which remained after combustion of the carbon. There was nearly 9 per cent. of hydrogen; and this he, as a scientific man, did not consider as being incompatible with this mineral being a coal. He did not believe there was a bitumen in this body, but it might have produced it. He found no benzol in the body itself, but he found it in the products of the distillation of the mineral—another feature in which it bore a strong resemblance to coal. Coal shales generally contained 60 per cent. of earthy matter; but looking at the proportion of earthy matter here, as compared with the carbonaceous matter, this mineral could not be called a shale or schist, its predominant constituent not being earthy matter, as in shale or schist, but carnaceous. He considered it, to be a true coal.

Dr. D. Fyfe during the last twenty years, ever since gas came into use in this country, had analysed all the cannel coals in Scotland, with a view to ascertain their gas-producing qualities. He had also examined specimens of the disputed mineral, and, in his opinion, it in no respect differed from the ordinary cannel coals, except in being of a very superior quality. Comparing its constituents with the Capeldrae cannel coal, he found them to be as follows:—

	Carbon.	Hydrogen,	Oxygen.	Nitrogen.	Sulphur.	Ash.
Torbanehill mineral . . .	60·25	8·8	3·6	1·5	9·3	25·6
Capeldrae cannel coal . .	56.7	6·8	8·8	1·9	0·25	25.4

The only difference was, that the Torbanehill was the better gas coal of the two. He had subjected Lesmahagow and parrot coal to the process patented by Mr. Young, of Bathgate, for the extraction of oils from the mineral, and he had obtained the very same products. He therefore considered it only a coal.

Dr. Douglas Maclagan had examined the Torbane mineral and several cannels for the purpose of comparison. It burnt exactly like a cannel coal; its products, when heated in a retort, were those of cannel; and on exposing it to the action of

naphtha, he found only an infinitesimal quantity of bitumen. From all the results of an analytical and comparative examination, he came to the conclusion that the mineral *was cannel coal.* Shale was a mineral with a larger quantity of earthy matter than coal; but, so far as he knew, there was no line of demarcation between coal and shale. They run into each other. If the clayey matter predominated, he should call it a shale; if the carbonaceous matter preponderated, he would call it a coal. He analyzed the ash of the Torbane mineral, and he found it to contain the same constituents as the ash of the cannels.

Dr. Gregory had analyzed specimens of the Torbane mineral. The conclusion to which he came was that the mineral was a cannel coal of a certain class. There were variations in the composition of cannel coals, and he considered this mineral as belonging to one division of these coals. He considered it as a coal with a very considerable tendency to slaty structure. In no essential particular did it vary from the general character of coal. Portions of the mineral varied a little, but within the limits of variance he would consider it to be a coal.

Cross-examined.—The material difference between bituminous shale and coal, was in the frequent occurrence of the conchoidal fracture, and the large proportion of combustible matter. In bituminous shale, the amount of combustible matter was much less, and its slaty structure was decided and invariable. In coals the structure was less slaty, even in slaty coals.

Re-examined.—The earthy matter in the specimens of the mineral he examined was 25 to 31 per cent. This was an extreme which was incompatible with its being a shale. A shale would contain a much larger quantity—a preponderating quantity of earthy matter.

Dr. Edward Frankland regarded it as a true coal, similar to other gas-producing coals. It would not be properly designated by the name of shale, because of its small percentage of ash and its large percentage of combustible matter. Carbonaceous matter was certainly the base of the mineral, and not clay. He discovered no bitumen in it. Its gas producing-powers were much greater than those of bituminous coals.

Professor Penny some years ago analysed what was called the Torbanehill mineral. He at that time considered it to be a valuable gas coal; and, from the specimens which he afterwards saw of the mineral, he had no doubt of the correctness of his opinion. The proportions of ash varied in different coals. He had made an analysis of several which he knew. In Capeldrae the proportion was 31·5 per cent.; in Methill it was 29·7; in Stevenson, 19·35; in Hillhead, 27·4; in Bartonshill, 38·7; in Bathvale, 29·2; and in the Torbanehill, 21 per cent. The quantity of ash in the variety made it more or less valuable as a coal, but still it remained a coal.

After very able addresses from counsel and the Lord Justice General, the jury, as most of our readers are aware, found for the defendants.

PUBLIC HEALTH.

On the AMMUNITION BREAD *supplied to the* EUROPEAN FORCES, *and on the* CHEMICAL COMPOSITION *of* BRAN. By M. POGGIALE, Professor to the Ecole de Medecine et de Pharmacie et Militaire.

TOWARDS the end of 1850, a Commission, of which the author was one, having been instituted for the purpose of examining the results obtained by the system of the direct purchase of the ordinary baker's bread, they made many experiments on panification, and analysed, chemically, the ammunition bread provided for the troops of the European powers, the bread of the civil hospitals of Paris, ammunition flower, and common commercial flower. The results of these researches, which have taken two years to complete, form the subject of this memoir.

The analysis of the bread presenting great difficulties, M. Pozziale used his best endeavours to overcome them by extreme care in his

investigations, and by modifying the known methods, which appeared to him defective. The following is the process which he used in his first analyses :—

He determined the weight of the inorganic substances by calcining a known weight of bread in a crucible, and weighing the residue, which are generally formed of carbonates of lime and magnesia, sulphate of lime, carbonate of potassa, silicic acid, oxide of iron, aluminiæ, and chlorides.

The quantity of water was estimated by drying 50 grammes of bread in a stove, with a current of air heated to 248°F. The matter was weighed, until its weight remained unaltered. The estimation of the fatty matters was executed by treating the bread, when perfectly dried, with ether, rectified in a displacement apparatus.

To ascertain the proportion of gluten, or nitrogenous matter, the dried bread was digested, with diastase, in the sand-bath, at the temperature of 140°F., so as to destroy all the starch ; the gluten was collected on a cloth, and, after several washings, it was dried. A substance was thus obtained which was insoluble in water, slightly elastic, translucid, brittle, soluble in potassa and in nitric acid. It was remarked that the gluten produced from wheaten bread was of a greyish white ; whereas, the gluten furnished by rye bread, and mixed rye and wheat, had a brown color and a peculiar odor. The gluten of fibrine and vegetable albumen were sometimes separated by the action of acetic acid ; but this separation, having no practical interest, was not done in all the analyses. In many experiments, the proportion of the nitrogenous matters was calculated according to the quantity of nitrogen which they contained.

As for the starch, it was estimated in the state of sugar, with cupreous tartrate of potassa.

In another experiment, M. Poggiale obtained the quantity of glucose and dextrine, by macerating in water the bread reduced to powder. The liquor, thus obtained, contained only traces of albumen. M. Poggiale estimated the quantity of sugar with tartrate of copper and potassa. The bran was collected on a fine sieve.

He has thus analysed samples of ammunition bread from Belgium, the Netherlands, the Grand Duchy of Baden, Prussia, Frankfort, Bavaria, Stuttgardt, Spain, Austria, and Paris.

On comparing the results of the analyses, we observe that the maximum of nitrogenous matters (gluten and albuminous matter) is 8·95 per cent., and the minimum 4·85. The French bread contains the most gluten ; and, as might be expected, that of Prussia contains the least. Our ammunition bread is, besides, superior to the others in appearance, taste, cooking, and even color. We must, however, remember that the foreign bread had been made a long time, and was, therefore, dry when analysed.

This circumstance induced M. Poggiale to determine the nutritive richness of these different breads by the estimation of the nitrogen.

In many experiments, the bread, having been dried at 248°F., was burned in a tube, and the products of the combustion were received

in a graduated test-glass containing a concentrated solution of potassa, so as to separate the carbonic acid from the nitrogen. Instead of a glass tube, the author made use of a long copper tube, which renders the operation more convenient and certain. A sufficient quantity of bicarbonate of soda was introduced into the tube, to remove the air contained in the apparatus; and, when the combustion was concluded, to carry the whole of the nitrogen into the test-glass.

The volume of nitrogen, brought to the temperature of 32°F., and with a barometric pressure of 760 millimetres, enabled the author to determine its weight.

The following table shews the result of these analyses, and gives the classification of the breads distributed to the soldiers of the European powers, according to the quantities of nitrogenous matters and of nitrogen contained in them :—

	100 Parts of bread dried at 248° F., contain, of nitrogen,	Nitrogenous matters calculated.
Paris	2·26	14·69
Grand Duchy of Baden	2·24	14·56
Piedmont	2·19	14·23
Belgium	2·08	13·52
Holland	2·07	13·45
Stuttgardt	2·06	13·39
Austria	1·58	10·27
Spain	1·57	10·20
Frankfort	1·44	9·36
Bavaria	1·32	8·73
Prussia	1·12	7·28

The proportion of nitrogenous substance was calculated by multiplying the weight of nitrogen obtained by 6·5.

As may be seen, M. Poggiale has endeavoured, in his analyses, to determine particularly the proportion of gluten and nitrogen; in fact, it is now acknowledged by chemists and physiologists, that the quantity of nitrogenous substance makes known the nutritive property of bread and flour. We must, however, with respect to bread, take notice of its manufacture! but we may say decidedly, that the flours which are the richest in gluten are those which are the best for human food. The differences observed between wheat, rye, oats, &c., proceed from the quantity, and, perhaps, from the nature, of the gluten, which exhibits very considerable differences in its composition and in the proportions of the elements composing it.

It became interesting, after the preceding experiments, to learn the proportion of gluten and nitrogen in bread of the first and second quality of the common bakers—that of the Parisian hospitals, and commercial flour. M. Poggiale has investigated this subject. It results, from this second series of analyses, that the ammunition bread and flour contain less nitrogenous matters than bread and flour of the first quality, and more than bread and flour of the second quality.

M. Payen obtained the same when operating on flour only; he came to the conclusion that the ammunition flour possesses superior nutritive qualities to flours of the second quality. Indeed, these, like the ammunition flour, do not contain all the parts of the corn; they are prepared with the inferior parts, after the separation of the gruaux and the finest portion of the flour. This opinion, which rests on incontestible chemical analyses, is, moreover, confirmed by the best practitioners. Nevertheless, it is just to add, that the ammunition bread contains a small quantity of nitrogenous matter, which, according to my experiments, is not assimilable.

Several Commissions have decided that bread, composed of good ammunition flour, has superior nutritive qualities to those of the bread of the second quality of the ordinary baker's. The opponents of the ammunition bread complain—and, in my opinion, without reason—that it is less nutritious than bread of the second quality; and the chemical theory of the composition of aliments, does not admit, as they think, that 630 grammes of white bread should be equivalent to 750 grammes of ammunition bread. This opinion was supported by the Commission named in 1850. Still, M. Poggiale is of opinion, that, to have good bread, the War Office should diminish the proportion of bran 4 or 5 per cent.

PRIVATE ACTS OF PARLIAMENT.

A SHORT time since, in the discharge of our duty as commentators on matters affecting the public health, we felt it necessary to refer to dangers which still exist of granting parliamentary powers to private companies for the execution of works at variance with the now established principles of sanitary science.

Our attention had been particularly directed to a private Act for supplying a certain small watering-place in Somersetshire with water for domestic use. The statement made in our article (vol. 4, p. 479) was sufficiently startling to draw general attention. Since then, the Chairman of the Company has publicly stated that almost all that statement was false. We owe an apology to the Company for making public their extraordinary system of water-supply, without giving our authorities for so doing; and in reparation of this injury we present now to our readers the clause of the Act referring to the six months' supply. Probably, after reading this clause, the public will assume our representation of other matters to be correctly given:—

"Be it enacted, That it shall be lawful for the Company, subject to the provisions and restrictions in this Act, and in the Acts incorporated therewith, contained, to make and maintain the works requisite for such supply in the lines or situation and on the levels, and upon the lands, and within the limits of deviation delineated on the said plan and sections, and described in the said Book of Reference, and to enter upon, take and use, such of the lands mentioned in the said plans and Book of Reference as shall be necessary for that purpose, *and also to divert, impound, and take, for six months in every*

year, commencing on the fifteenth day of December, and terminating on the fifteenth day of June, the waters of the West Moor Rhine, subject to the following regulations and restrictions"—

(Here follow four engineering regulations.) The fifth is :—

"The Company may, from time to time, by and with the consent in writing of the owners of two-thirds in quantity of the land in the parishes (here the parishes and properties are specified), divest, impound, and take the waters of the said Rhine at other periods than those hereinbefore specified, subject, nevertheless, to the beforementioned terms, restrictions, and conditions, *and to such other terms, conditions, and restrictions as may be set forth in any such consent.*"

Can there be any question here as to the limited powers of the Company, and the extent to which they must provide against a dry season? We believe the giving powers to take water for domestic supply only during one half of the year, and that the wettest half, to be perfectly unparalleled, and we hope that in the interests of humanity, of decency, and of morality, such a proceeding may never be repeated. In doing such things we lay ourselves open to a well-deserved infliction of cholera, fever, and other awful plagues.

BIBLIOGRAPHY.

A FULL Report of the trial before the Lord Justice General and a special jury of the issues in the action at the instance of Mr. and Mrs. Gillespie, of Torbanehill, against Messrs. Russel and Son, Coal-masters, Blackbraes, for infringment of lease of coal, iron. stone, &c., commencing Friday, July 29, and ending Thursday, August 4, 1853. Reported verbatim by Mr. ALEXANDER WATSON LYELL, Shorthand Reporter. Edinburgh, Bell and Bradfute.*

This is a report in full of the celebrated Torbanehill mineral trial, a trial that must long exercise a most damaging influence upon the interests of science, and do more than perhaps any circumstance whatever to bring scientific men into public contempt and ridicule in this country. Nor can we deny that such is the legitimate conclusion afforded by the data as these stand presented to us in the evidence put forward to the jury upon this singularly unfortunate occasion. On both sides we find an array of names heretofore held in respectful veneration, not only by the people of Great Britain, but of Europe also—we find, moreover, that the question which they had to decide was one of a particularly plain and simple nature. A certain combustible mineral has been found, and it was demanded whether this mineral was coal or not coal? Charles the Second once put the whole of the Royal Society into a "fix," by asking, why a live salmon was heavier than a dead one? And we are told that some considerable time elapsed before any individual could prevail upon himself so far to doubt the royal word as to try by actual experiment whether the fact was or was not as stated in the query. A reserve of the

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* A summary of the chemical evidence will be found on p. 43.

same kind has evidently hung over the name of the Torbanehill mineral. Not that there was any royal impediment in the way, as in the instance quoted; but we fear the non-scientific public will believe that there was something worse: that there was money, and money, too, on both sides of the question,—that there was money for those who would swear that the mineral was coal, and that there was money for those who would swear that it was not coal—money for geologists, chemists, microscopists, and every other kind of "ists," to say and to unsay, to prove and to disprove, all things and anything advanced or denied on either or both of the points at issue. The trial had quite a Homeric character—hero against hero—now Hector, now Achilles—now Ansted, now Fleming—now Brande, now Graham, and so on, down to mere ordinary working-men, the "pawns" of the legal chess board. The contest was quite exciting, and the chivalrous devotion of the combatants to the interest they served, absolutely heroic; nothing but firm conviction or hard cash could ever have produced such unparalleled enthusiasm. Professor Ansted found that the mineral "smelt like clay, could be made into a brick," and had, moreover, "a clay basis"—it was not coal. Professor Brande was quite clear that it was not coal, because it had so small a quantity of carbon in it, but he could not tell the amount of carbon in it, because he "had never analysed it so as to ascertain its amount of carbon." Professor Anderson thought it was "not coal," because in his opinion nothing could be coal which contained less than 70 per cent. of carbon, and that mineral had only 64·0 per cent. of carbon. Mr. Hugh Miller had been able to make "a ghaist" from the Torbanehill mineral, therefore it was not a coal; and he found himself confirmed in this opinion by reference to Holy Writ. On the opposite side, Professor Fleming declared that the mineral "differed in no essential point from cannel coal, and as such he would designate it," in opposition to Professor Ansted and Mr. Miller. Professor Graham in like manner contradicted Professor Brande. Dr. Hoffman contradicted Professor Anderson, and at length the jury found themselves in the agreeable position of men who had to form an opinion from assertions diametrically opposed to each other. The circumstances of the case required that men should be found like Hudibras, who could

> "Dispute, refute,
> Change hands, and still confute;"

and they were found in the very first ranks of science. How they will preserve their gravity when next they meet in scientific conclave, it is not for us to say. Cicero was astonished that one soothsayer did not laugh whenever he saw another soothsayer, "*Mirabile videtur, quod non rideat haruspex cum haruspicem viderit,*" but how much greater would be his astonishment at witnessing the solemnity of the next meeting of the scientific celebraties of the Torbanehill mineral trial? Cicero's soothsayers would have burst their sides under one tenth of the pleasurable excitement; and as to remuneration, why Gillespie and Russel must have paid more at this trial than the whole

Senate of Rome in its most anxious sittings. Upon the whole, we feel justified in classing the evidence given in that trial as a real and permanent disgrace to the age we live in, and enter our warmest protest against it. It is not, and we here emphatically declare that it is not, a fair criterion of the state of *science* in this country. In point of fact, however, this trial was really a matter for equity and not for a jury, as it is painfully obvious that the Messrs. Russel had taken those means to procure a good bargain, which, on this side the Tweed, are looked upon as "sharp practice." Mr. Gillespie was too confiding or too dilatory in looking after his own interest: this, we are told, arose from illness, but then illness, if sufficient to incapacitate a man from conducting a bargain, should also have prevented him from completing it. The truth is, however, clear enough, as commented on by the Dean of Faculty, in his concluding address, that Messrs. Russel did discover the existence of this valuable mineral in the lands of Mr. Gillespie, and that they did not, as they had agreed, communicate that knowledge to him. On the contrary, they kept it hidden till they had completed their bargain, and they then set about making a fortune from it, and from *it* alone, out of all the other minerals, they leased. Of such conduct, we say, with Master Slender, this is "as it may be."

The following comments of the Lord President are so very just, and so forcibly illustrate and bear out our previous remarks, that we cannot do better than offer them to our readers as a kind of corollary, by way of conclusion.

"When they call in an eminent Geologist, Mr. Ansted, he tells you that it is not coal, and when the other party call in another Geologist, he tells you that it is coal, and so they go on, on each side. I think there were four Geologists on the one side and five on the other. The four declared positively that it was not coal, and the five declared as positively that it was coal, all speaking with perfect sincerity, and no doubt according to their modes of thought, that is, according to what they as Geologists class as coal—according to the description which they have provided for themselves of what they class as coal, for I do not know that any reference was made to any ascertained decided standard of coal to which all Geologists could appeal. If they had so referred, they would not have differed as they did. But, being men of science, they do endeavor to ascertain if the substance depends on a combination of elements and accumulation of results. They endeavor to specify fixed elements as leading to fixed results, and they differ in their opinion.

"And if you go and call in the Chemists, it does not tend to make the matter more clear. We had men of high eminence as Geologists, and in addition we had also Chemists of the highest reputation—all men of acknowledged talents, and in their several departments the best the country can produce—and yet we had five on the one side applying all their energy to find whether this was coal, and coming to a clear conclusion that it was not; and on the other side we had ten witnesses, all coming to a decided opinion that it was. Now, that

clearly argues the want of some reasonable and reputed definition, for it is quite true that it is not the existence or presence of certain component elements that will necessarily decide whether it is coal or not. They all agreed that the same elements will exist in substances in different proportions—in a substance not coal, and in a substance that is coal. I do not think that there was anything said to exist in coal which is not said to exist here, but in different proportions, and also said to exist in substances found in the coal formation."—*Report published by the Pursuers, page* 237.

THE MICROSCOPE IN ITS SPECIAL APPLICATION TO VEGETABLE ANATOMY AND PHYSIOLOGY. By Dr. HERMANN SCHACHT. Translated by F. CURREY, M.A., "Highley's Library of Science and Art." Post 8vo., Samuel Highley, London.

WE are glad to find produced in an English form, the valuable treatise of Dr. Hermann Schacht upon the application of the microscope to vegetable anatomy and physiology. The volume in question forms the first of a series of works projected by the Publisher under the title of "Highley's Library of Science and Art," and the parts of the series already advertised, lead us to think that Mr. Highley's project is calculated to render great service to the subjects to which it is devoted. The high reputation of Dr. Schacht, not only in his own country, but amongst men of science in England, is such, as to render it unnecessary that we should say much in praise of the present treatise, and we think we should be doing our readers the best service by calling attention, necessarily in a concise manner, to the scheme of the work before us. After noticing the importance of microscopical enquiries, and the difficulties and deceptions to which unpractised observers are subject, the author describes the instruments necessary for investigation, and furnishes a very useful list of chemical reagents, stating the particular purposes to which each reagent is particularly applicable. He then proceeds to lay down general rules for the use of the microscope, and for the examination and preparation of objects, referring again to the great importance of a proper use of chemical reagents. The fourth chapter is devoted to a consideration of the vegetable cell, including the different appearances, forms, and arrangements of such cells, and the manner in which they are combined, so as to form parenchymatal, prosenchymatal, woody and other tissues, and rules are given for the examination of the cells and tissues of which the chapter relates. In the next chapter the author describes the subject of the microscopical examination of plants in general, commencing with the methods to be adopted in examining the perfect plant, and proceeding in detail through the examination of the stem, the root, the leaves, the flower and the fruit, taking a passing notice of the motion of the juices of the cell, such as is found in the *Vallisneria spiralis* and other plants, and which the author, in common with some other vegetable physiologists, conjectures may be present in all living vegetable cells. This chapter also contains observations relative to the germination of the spores of certain cryptogams, and to the

investigation of the development of stems, leaves, and flowers. The development of the embryo is dwelt upon at some length, and is a subject to which Dr. Schacht has devoted special attention. He is a warm advocate of Schleiden's theory of the origin of the embryo in the interior of the extremity of the pollen-tube; a theory which created considerable interest, and which has given rise to much discussion. Although the author's views have been disputed by Amici, Mohl, Hofmeister, and others, it is worthy of remark, that when the question was propounded by the Dutch Academy as the subject for a prize essay, the prize was awarded to Dr. Schacht.

The chapter to which we are referring contains also directions for the examination of the development of certain cells, and closes with some remarks relative to the division of the primordial utricle, a point of great physiological importance.

In the sixth chapter, the author traces the development of certain particular flowers, selected with the view of illustrating his previous remarks. The successive phases of the flowers in question are exhibited in a number of beautiful wood-cuts, the whole chapter forming a practical commentary upon the contents of the preceding ones.

The seventh chapter is devoted to the subject of the drawing of microscopical objects, and contains many excellent observations, which cannot be usefully condensed.

The eight, and last, chapter gives rules for the preservation of microscopical objects, the author prefering a solution of chloride of calcuin, Canada balsam, or glycerine, to all other preservative media.

The brief short notice which we have thus given of Dr. Schacht's work, will be sufficient to enable our readers to estimate its importance and interest, and we conclude by cordially recommending the present translation to all botanists, and other parties interested in microscopical enquiries.

A PRACTICAL TREATISE ON SEA-BATHING AND SEA-AIR. By GEORGE HARTWIG, M.D. "Highley's Popular Medical Series," No. 1. Foolscap 8vo. London.

THIS useful little guide to sea-side visitors, is, we perceive, the first of a series which promises to do good service to the public. Too many persons seem to fancy that a plunge into the sea may be taken at any time without prejudice to the health; but, in all cases, the body should be prepared for the shock, the proper period after a meal selected, and not too long a time spent in the water—otherwise, the constitution may lose more than it gains, in the attempt to invigorate it. The work before us, after putting the reader in possession of a sufficient amount of information respecting the functions of the nervous system, and the digestive and respiratory organs, enters upon the mode of action and the effects of the sea-bath and the complaints in which it is most efficacious, and on the rules and regimen to be adopted whilst visiting the sea-side in search of health or recreation. We recommend this little book to every bather, whether he be in or out of health.

ANNUAL REPORT OF THE PROGRESS OF CHEMISTRY AND THE ALLIED SCIENCES, PHYSICS, MINERALOGY, AND GEOLOGY. By JUSTUS LIEBIG, M.D., and H. KOPP, &c. Edited by Drs. HOFMANN and H. BENCE JONES. Vol. 4, for 1850. 8vo., London.

REPORTS of this kind are of great service to those interested in scientific pursuits, as furnishing an index to recent research; but their value is considerably decreased, if, as in the present instance, they are not published till nearly three years after that for which the report is given. This, we believe, is not entirely the fault of the Editors or Publishers, but arises from that want of support which scientific works generally have to struggle against in England. It seems strange that, whilst on the Continent a multiplicity of journals and reports, &c., on natural history and natural philosophy flourish, in England, where so many monied manufacturers exist, and who would naturally be expected to feel an interest in the progress of science, few such publications issue from the press, and success is the exception to the rule. This work should find a place, as a book of reference, on the shelves of every person connected with chemical pursuits, but the matter is too old to give room for review or extract on the present occasion.

NOTES AND QUERIES.

Can any of your readers give their experience as to the *causes* of failure in Photographic Practice?—F. C. S.

What were the points in which the Irish Amelioration Societies' operations for the Manufacture of Peat Charcoal have failed?—A.C.E.

Has any relation been noticed between the symmetrical grouping of arborescent forms of crystallisation, and the Crystallographic system to which they belong?—S. H.

PROCEEDINGS OF SOCIETIES.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE Twenty-third Annual Meeting of this Association was held at Hull, on the 7th ultimo, and the following days. We extract from the *Athenæum* a report of the transactions of the Chemical Section, and such portions of the Report of the Physical Section as are appropriate to this Journal.

[*Chemical Section, Sept.* 8.]

On the Chemical Action of the Solar Radiations.

By Professor R. HUNT.

This was a report to the Section of the continuation of an examination of the chemical action of the rays of the prismatic spectrum, after it had been subjected to the absorptive influences of different colored media. The mode of examination adopted was to obtain well-defined spectra of a beam of light passing through a fine vertical slit in a steel plate, by prisms of flint and crown glass and of quartz. The spectrum, being concentrated by a lens, was received upon a white

tablet and submitted to careful measurement; the colored screen, colored glass, or colored fluid, was then interposed, and the alterations in the chromatic image were carefully noted; the chemical preparation was then placed on the tablet, and the chemical impression obtained. The relation which this image bore to the luminous image was a true representation of the connection between the color of a ray and its power to produce chemical change. In the report made to the Belfast meeting of the Association, the results of experiments made on glass tablets, prepared by the so-called collodion process, alone were given. In the present report, the examination has been extended to the photographic preparation known as the calotype, and to iodide and bromide of silver in their pure states and when excited by gallic acid. M. E. Becquerel, in a paper communicated to the Academy of Sciences, of which an abstract appears in the "*Comptes Rendus*," tom. xvii. p. 883, states that, "when any part of the luminous spectrum is absorbed or destroyed by any substance whatever, the part of the chemical rays of the same refrangibility is equally destroyed." The author's experiments, as recorded in the former report, and those now detailed, would prove that this conclusion has been formed too hastily. Although there are many absorptive media which, at the same time as they obliterate a particular-colored ray, destroy the chemical action of that portion of the spectrum, yet there are many more which prevent the passage of a ray of given refrangibility, and do not, at the same time, obstruct those rays which are chemically active of the same degree of refrangibility. This is particularly exemplified in the case of glasses colored yellow. With some of these the blue rays are obliterated, the chemical action of this part of the spectrum not being interrupted; whereas, in some other examples, those rays permeate the glass, but are almost entirely deprived of chemical power.

A still more curious fact is noticed in this report, for the first time, of some media which have the power, as it were, of developing chemical action in a particular part of the spectrum where the rays did not appear previously to possess this power. Several glasses exhibited this phenomenon to a certain extent, particularly such as were stained yellow by the oxide of silver; but one glass shewed it in a remarkable manner. This glass was yellow when viewed by transmitted light, but it reflected pale blue light from one of its surfaces; it obliterated the more refrangible rays down to the green, and rendered the yellow rays far less luminous than usual. In nearly every case the yellow rays are found to be not merely inactive, chemically, but actively to prevent chemical action. After the spectrum has been submitted to the action of this glass, *all chemical power is confined to this yellow ray.* The author has hitherto supported the view that photographic phenomena and the illuminating power of the sunbeam were distinct principles, united only in their modes of motion. He was led to this from observing that where there was the most light there was the least power of producing chemical change; and that as illuminating power diminished, the chemical phenomena of the solar rays increased. The

results, however, which he has obtained during the brief sunshine of the present Summer, leads him to hold that opinion in suspension. In many of the spectra obtained there appears to be evidence of the conversion of one form of force into another—the change, indeed, of *light* into *actinism* or chemical power; and, again, as in Mr. Stokes's experiments, the exhibition of the ordinarily invisible chemical rays in the form of *light*.

Professor Stokes made some remarks on the different effects produced by the spectrum, dividing them into luminous effect, chemical action, calorific power, phosphorescence, and fluorescence. These were different effects resulting from the same cause, and he did not consider that sufficient evidence had yet been given to warrant the idea that there existed any dissimilar agencies in the solar rays.

Professor Johnston, the Rev. V. Harcourt, Dr. Daubeny, Mr. Claudet, and others, took part in the conversation which followed.

On the Employment of the higher Sulphides of Calcium as a Means of Preventing and Destroying the Oidium Tuckeri, or Grape Disease.

By Dr Astley P. Price.

Of the numerous substances which have been employed to arrest the devastating effects of this disease, none appear to have been so pre-eminently successful as sulphur, whether employed in the state of powder or flowers of sulphur, or by sublimation in houses so affected. Notwithstanding the several methods described for its application to the vines, I am not aware that any had been offered in 1851, when these experiments were instituted, by which sulphur might be uniformly distributed over the branches, and be there deposited in such a manner as to be, to some extent, firmly attached to the vine. Three houses at Margate, in the vicinity of the one in which the disease first made its appearance in England, having been for the space of five years infected with the disease, and notwithstanding the employment of sulphur as powdered and flowers of sulphur, no abatement in its ravages could be discovered,—I was induced to employ a solution of pentasulphide of calcium, a solution of which having been found to act in no way injuriously to the young and delicate shoots of several plants, was applied to the juices in a dilute condition; the object in view being that the compound should be decomposed by carbonic acid, and that the excess of sulphur should be deposited with the carbonate of lime in a uniform and durable covering on the stems and branches of the vines. This was adopted, and although but few applications were made, the stems became coated with a deposit of sulphur, and the disease gradually but effectually diminished, in so much that the houses are now entirely free from any trace of disease or symptoms of infection. The young shoots are in no way injured by its application, and the older wood covered with this deposit of sulphur continues exceedingly healthy. This was, we believe, the first employment of the higher sulphides of calcium as a vehicle for the application of sulphur to the stems and foliage of diseased vines. Specimens were exhibited

from vines which in 1851 were covered with disease, and which since the Autumn of that year have received no further treatment. The vines in the immediate neighbourhood, and adjoining one of the houses, are covered with the disease, but, notwithstanding their close proximity, no indication of the disease has at present been detected in either of the three houses.

On the Effect of Sulphate of Lime on Vegetable Substances.
By the Chevalier CLAUSSEN.

About six weeks since, I was engaged in making various experiments on the effect of sulphate of lime on vegetable substances. A portion of the substances then used by me was carelessly thrown aside, and on returning to my experiments about a fortnight afterwards, I was surprised to find that decomposition had not taken place in those portions of the vegetables which had been subjected to the action of the sulphate, while those which had not been so treated were completely decayed. Among the articles experimented upon were a number of potatoes, each of which was affected by the prevalent disease; some of these remain sound to the present day, the others have some time since completely rotted away. Subsequently, I procured some more potatoes, and also some beet-roots, the former being, as far as I could judge, all diseased. I divided the potatoes into three portions. One lot I placed in a vessel with a weak solution of sulphuric acid, and from thence I placed them in a solution of weak lime-water. In the second lot the process was reversed, that is to say, the potatoes were first placed in the lime-water, and then in the acid. The third lot was left untouched. Ten days afterwards I examined the potatoes, and found, as I expected, that the potatoes which had not been treated with the sulphate were rapidly decaying,—those which had been first placed in the solution of lime and then in the acid were more nearly decomposed,—while those which had been treated in the mode first described remained as sound as when first taken in hand. Upon being cut open the diseased part of the potatoes was not found to have spread internally, and the flavor of the root was in no degree affected by the application of the process, nor do I think that its germinating power was injured by the effect of the sulphate. The effect upon the beet-roots was similar to that produced on the potatoes, and which would seem to be somewhat analogous to that of galvanising metals, viz., protecting the substances from the effect of atmospheric agencies. I may add, that muriatic and other acids have been employed by me on other occasions with equal success, the only agents required, appearing to be those which will most readily produce a sulphate in contact with the substances required to be preserved. I do not think that any insuperable difficulty exists with respect to the application of the process. The acid I employed was very weak,—about one part to two hundred of water; the lime-water was about the consistency of milk.

On Crystals of Carbonate of Lime inclosing Sand from the Sea-Coast of Africa. By M. T. J. PEARSALL.

The crystals here shewn were obtained by Captain Mitchell, of the merchant-ship Frankfield, while searching the coast of Africa, between Saldanha Bay and the island of Ichaboe, for guano deposits. The crystals are of carbonate of lime, inclosing sand; 15 to 20 per cent. of sand is obtained from some specimens. The crystals are very hard, and have sharp cutting edges, so as to make it a painful task to walk upon them. The beach was covered with crystals for miles; it seemed as far as the eye could reach, and was one-half to one mile in breadth. Some of the specimens are from four to five inches in length, and with surfaces shewing a thickness of half an inch, and from two to three inches across the plane. The report given was, that some of the crystals protuded up from the sands so far as to wound the ankles and legs without great care in walking over. Some crystals seem to be opaque, with the sand inclosed except at the edges; 15 to 20 per cent. of sand is obtained from portions of crystals. Carbonate of lime and magnesia, with small quantities of saline matter, common salt principally, can be obtained by breaking them up in distilled water. They are entirely soluble in diluted nitric acid. Mineralogists and chemists are perfectly well aware of the stony substance called Fontainbleau sandstone, where the sandstone is found having forms of crystals of carbonate of lime. The crystals now exhibited shew the grains of sand of the beach inclosed, without altering the general form, and also that the crystal has at its base adapted itself to the sand and other crystals. These specimens shew the great facility on that coast of producing mineralised crystals; and also shew the opportunities that are constantly offered to intelligent merchant-seamen of bringing home specimens of great interest which are uncommon in most parts of the world except in some places where they may visit, and where these may be in abundance.

On the Chemical Constitution of the Humber Deposits. By J. D. SOLLITT.

This morning was devoted to Photography; the Section having requested Professor R. Hunt and Mr. Claudet to arrange the means of exemplifying all the processes at present employed. By the aid of the local photographic artists, this was accomplished in as satisfactory a manner as the suddenness of the occasion would admit of. Mr. Hunt explained all the processes on paper and on glass, while Mr. Claudet exhibited the manipulatory details of the daguerreotype.

The Rev. T. EXLEY read a paper "On the Cause of the Transmission of Electricity along Conductors generally, and particularly as applied to the Electric Telegraph."

Professor Andrews described a simple instrument for graduating glass tubes. The divisions admit of being varied in length to the 1–17,000th of an inch.

On the Origin and Composition of a Mineral called Rotten-Stone.
By Professor JOHNSTONE.

After having stated the district—the Great Fin, Derbyshire—in which the mineral was found, the Professor went on to describe its chemical character and affinity. He observed that its component parts were not of a constant character, as had been asserted in some mineralogical works. Its origin was stated by Phillips to be from the decomposition of the slate rocks of Derbyshire. When examined under the microscope it did not exhibit any organisms, but there were particles or bundles of some substance resembling the bituminous substances found near Castleford. Rotten-stone was found in lumps of all shapes and sizes, at depths varying from two to six feet below the surface of the earth. It was his (the Professor's) opinion that the rotten-stone was not the result of the decomposition of the shale of Derbyshire, but of the veins of black marble of the country, which had undergone a great change. In proof of this assertion, he produced specimens which he found to be black marble, with the merest coating of rotten-stone on them, whilst others were half rotten-stone and half black marble. The decomposition had been effected by dissolving the lime out of the rock and not the rotting of the strata; this substance can be produced by dissolving the lime out of limestone, by bringing weak acids to bear upon it. This proved that there must necessarily exist in the soil some acid which dissolves the lime with which it comes in contact. Farmers would, therefore, see the necessity of adding lime to their land from time to time, because the lime kept continually washing away by the waters of heaven falling on it and extracting from the rotting roots of the earth an acid which had a powerful effect, not only on lime but on other mineralogical structures as well. Professor Johnstone then, at considerable length, entered upon a disquisition of the subject of the recent discovery of the greenstone rocks and the phosphoric acid which abounded in some districts.

Dr. Daubeny thought the Professor's opinion as to the origin of rotten-stone and the cause of the decomposition of the limestone, was highly incorrect. He thought the phosphoric acid was owing to the action of carbonic acid gas, generated in the earth, upon the lime of animal substances.

Mr. C. Varley was of opinion that the various animals feeding in the districts in which the phosphoric acid was found, took it into their bodies in their food, and as they could not exist without phosphorus, they retained a certain portion and evacuated the rest.

Mr. H. S. Blundell said, attempts without end had been made to obtain rotten-stone equal to the natural production, but they had proved entirely abortive. He always understood rotten-stone to be an aluminous substance, and not one of the nature of black marble, but he thought rotten-stone had now been proved to be owing to the decomposition of the limestone.

Professor Johnstone said, thousands of gallons of muriatic acid rolled

yearly down the Tyne, and no doubt it could be turned to good account in manufacturing rotten-stone from marble, which might be procured from Derbyshire.

Mr. Blundell said, that in the event of such an establishment he would support it heartily.

On the Determination of the available Amount of Chlorine contained in the Hypochlorites.

By Dr. ASTLEY PRICE.

On the Spontaneous Decomposition of Xyloidine.

By Dr. GLADSTONE.

This was a description of the changes that had taken place in a specimen of Xyloidine, made by treating arrow-root with nitric acid of specific gravity 1·5. After remaining about six years unaltered, this specimen suddenly began to give gases, and in a few weeks time nothing remained of the original Xyloidine, but, in its place, a light-brown viscid liquid.

On the Decomposition of Water under Pressure by the Galvanic Battery.

By J. P. GASSIOT.

The paper detailed a series of experiments which entirely confirmed the law laid down by Professor Faraday.

A New Method for Determining the Commercial Value of Oxide of Manganese. By Dr. A. PRICE.

On the Conduction of Electricity by Flame and Gases.

By W. R. GROVE.

A somewhat extended series of researches have been recently carried out by M. Edmond Becquerel, with a view to determine the conducting power of flame and of hot air. These investigations have led M. E. Becquerel to conceive that he has proved the conducting power of both for electricity. The apparatus employed—a platinum tube, with the conducting wire passing through it—appearing to offer some sources of error, Mr. Grove has adopted a somewhat different arrangement. This consisted of a glass tube, with two copper wires inserted through corks at either end; from these, within the tube, proceeded a piece of platinum wire, which, by connection with the battery, could be brought to a state of intense ignition. In this state these were adjusted at the distance of 1-50th of an inch apart, and then connected with the powerful voltaic combination of Mr. Gassiot. Notwithstanding the proximity of the wires, no trace of electricity could be detected as passing through the interposed stratum of heated air, thereby proving the non conductibility of the gases while hot. The conducting power of flame has been already satisfactorily proved.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

*On Atmospheric Electricity.**

By Reuben Phillips.

212. Before proceeding to further applications, it appears necessary to shew more fully that the principle on which is based the explanation of the source of atmospheric electricity, is entirely supported by the experiments.

213. On discharging a jet of steam at a low-pressure in the manner previously described ("Phil. Mag." S. 3, vol. 36, p. 104), the issuing steam as it escapes into the air becomes charged with negative electricity, leaving the boiler positive. Supposing that evaporation does, under certain circumstances, generate some electricity, which can only be a very small quantity, it appears impossible to consider evaporation as the cause of this electric disturbance; for the lowness of the pressure at which the effect may be obtained, and, as a consequence, the similarity of temperature which pervades the metal jet and steam, makes any considerable evaporation in the orifice impossible. By surrounding a brass pipe 5·5 inches long and ·7 inch diameter with wet cotton, while its interior was kept filled with high-pressure steam, I have endeavored to develope sufficient electricity to affect the electroscope used in the experiments before referred to; but although the evaporation was very rapid, no electrical action was obtained: yet this evaporation must have been many hundreds of times greater than that which could take place in the orifice, which is only 1–4th inch long and 1–12th inch diameter. The minute quantity of water which may be evaporated in the orifice cannot, therefore, be assumed as the cause of the electricity therein developed.

214. In the discharging orifice we have to consider the action of three substances—the metal of which the jet is made—and the water and steam. The case of the friction of water against the sides of the orifice has been very fully examined by Mr. Armstrong and Dr. Faraday, and it is known to leave the orifice negative, the water particles becoming positive; which is the reverse of what takes place in the experiments with low-pressure steam. Therefore the friction of the water on the metal jet does not generate the electricity of low-pressure steam. This is farther proved by the circumstance, that saline matters put into

* Having been received by the Royal Society, May 19, 1853.

the orifice extinguish the electricity produced in Dr. Faraday's experiments, but have no such action on the electricity of low-pressure steam. I rather think that after Dr. Faraday's investigation, analogy would point out the friction of water on steam as the cause of the phenomenon. Now if the friction of steam on water generates the electricity, then dry steam should generate none of this electricity; which I found to be the case ("Phil. Mag." S. 3, vol. 36, p. 507, § 114). Also, in the generation of frictional electricity, it is necessary, in order to accumulate electricity on the rubber, that the more extensive surface which is generally the moving surface, should be a non-conductor. If, therefore, in the production of the electricity of low-pressure steam, the steam itself is the moving surface and the water the rubber, then, increasing the conducting power of the water should not extinguish the electricity; which agrees with the experiment (116). Again, if the water on which the steam rubs is the moisture which adheres to the interior of the metal jet, then, by sufficiently increasing the velocity of the effluxion, the water should be torn from the sides of the orifice, thus reproducing the ordinary electricity of the hydro-electric machine; this effect is easily obtained.

215. The jet of high-pressure steam and water ("Phil. Mag." S. 3, vol. 35, p. 493, § 55) contains both positive and negative electricity (58). In this instance the electricity can in no way arise from friction against the sides of the orifice, for either positive or negative electricity should in that case be found on the boiler. Nevertheless, friction must be occasioned between the minute particles of water shot out of the orifice and the stream of steam. Let it now be assumed, in accordance with the preceding paragraph, that the electrical disturbance results from this friction of the particles of the water on the steam. Then the particles of water, on account of the readiness with which they conglomerate, should be more easily separated from the cloud; and, as with low-pressure steam, the friction should make the water positive, therefore a screen should collect positive electricity from the cloud—which thing it does. The electricity of this jet is, moreover, extinguished by discharging the steam without water—and is unaffected by increasing the conducting power of the water: these facts have the same significance as in the case of low-pressure steam. In this electrical jet, evaporation must ensue when the particles of water escape from the pressure inside the orifice into the atmospheric pressure; added to which, it seems likely that the friction to which the particles are exposed, also produces heat and evaporation: however, a considerable portion of the water does escape evaporation, and is seen under favorable circumstances to form nebulous cones in the steam-cloud. The quantity of water discharged amounts but to a few drops in a second, of which probably only a very small part is evaporated; but supposing the whole to be evaporated, no one has yet shewn that the evaporation of 1000-times as much water produces any quantity of electricity approaching that given by this jet. Evaporation cannot therefore produce the electricity.

216. When a jet of steam escapes into the air, there is a great

condensation ; and condensation has been supposed to generate electricity. Now this condensation can be avoided by receiving the blast as it escapes from the orifice into a glass tube fitting air-tight to the brass jet. On performing this experiment (47), I found that the electricity was not suppressed ; for the water particles imparted positive electricity to the tube, just as when condensation was allowed (51).

217. The experiments ("Phil. Mag." S. 3, vol. 36, p. 506, § 105—112) lead so directly to the inference that the friction of air on water produces electricity, that, as it appears to me, no reasoning can make it plainer, and it seems necessary either to accept this inference, or to reject the experiments as altogether inexplicable. Finally, admitting that the friction of gaseous matter on water produces electricity, a ready explanation of a great number of complicated experiments, which are otherwise unconnected, is afforded. And friction is an undoubted agent of great power in the production of electrical disturbance, which certainly cannot be said either of evaporation or condensation.

218. *On the Structure of Thunder-clouds.*—The theory that lightning results from the friction of water, requires that the drops of rain should be exposed to the action of a strong wind. But it frequently happens during a thunder-storm, that the air is calm and the clouds have only a slow horizontal motion ; it therefore follows that the direction of the wind which produces the electricity is perpendicular. The accounts obtained by M. Arago, "from the military engineers employed in the great trigonometrical survey, and who being placed at elevated stations on the Pyrenees, were enabled to observe the superior surface of strata of clouds situate below them. From the reports of these officers, and especially those of MM. Peytier and Hossard, it appears that there is no correspondence between the state of the lower and upper surfaces of a stratum of thunder-clouds ; that when the inferior surface is perfectly even and level, the superior surface will be broken into ridges and protuberances, rising upwards to great altitudes, like the surface of the earth in an Alpine district. In times of great heat, such strata were observed suddenly to send upwards lofty vertical cones, which, stretching into the higher regions of the air, established, by their conducting power, an electrical communication between strata of the atmosphere at very different heights. This appearance was generally observed to precede a thunder storm."—("Lardner and Walker's Manual of Electricity," vol. 2, p. 118.) These appearances can only result from ascending columns of air, set in motion owing to the lower parts of the air having become greatly expanded by heat. The current of air ascending in the air-channel of the thunder-cloud will become cooled both by expansion and by drawing in the adjacent air of the more elevated strata ; rain is consequently formed, and the drops are carried upward by the wind until they have acquired such a bulk that the wind can no longer bear them up. During this time, the friction of the wind on the drops of water developes electricity, the rain becoming positive and the air negative, and when the rain falls through the wind, the positive drops become more separated from the negative cloud, and the electric intensity of both are exalted. The light-

ning may then strike upward into the negative cloud, or to the earth, according to their relative distances, and the other circumstances which are known to influence the direction of electrical discharges.*

219. The connection of lightning with hail appears to be this: if the ascending air has power to carry the drops to a sufficient altitude, they will be incorporated with snow, and frozen. This theory of hail is, perhaps, the same as that given by Von Buch.—("Encyclopædia Metropolitana," Meteorology, p. 128, § 625.)

220. Lightning is often an intense red; and, in accounts of aurora, red is also frequently mentioned. However, the light, when first emitted, may have been whiter, as the brilliancy of lightning may be dimmed by the action of clouds and rain, and the more refrangible portion may be lost to the eye by the transmission of the light through mist ("Phil. Mag.," S. 4, vol. 4, p. 129). A mist, dense enough to color the diffused light of an aurora, would be incapable of sensibly coloring the light of a star; for the same reasons that the sun appears white when seen through colored media ("Phil. Mag.," S. 4, vol. 4, p. 414). Perhaps other of the auroral colors may have a similar origin.

221. Shooting-stars may, perhaps, be instances of ball-lightning. The conversion of part of an auroral arch into a multitude of shooting-stars, described by Mr. Pringle ("Phil. Mag.," S. 4, vol. 5, p. 308), is entirely in favor of such a notion.†

222. The connection of whirlwinds with lightning is now, I think, very apparent; because, a whirlwind in which vapor is being condensed is, or resembles, a thunder-cloud resting on the ground. With regard to water-spouts, there appear to be two sorts—a blustering whirlwind, raising water, or spray, in consequence of a difference of

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* In Mesopotamia, the motion of the thunder-cloud is sometimes seen from below. Layard, in his last volume of "Discoveries in Nineveh and Babylon" (p. 294), writes:—"During the day heavy clouds had been hanging on the horizon, foreboding one of those furious storms which at this time of the year (April 6th) occasionally visit the Desert. Late in the afternoon these clouds had gathered into one vast circle, which moved slowly round like an enormous wheel, presenting one of the most extraordinary and awful appearances I ever saw. From its sides leaped, without ceasing, forked flames of lightning. Clouds springing up from all sides of the heavens were dragged hurriedly into the vortex, which advanced gradually towards us, and threatened soon to break over our encampment. Fortunately, however, we only felt the very edge of the storm—a deluge of rain, and of hail of the size of pigeons' eggs. The great rolling cloud, attracted by the Singar-hill, soon passed away, leaving, in undiminished splendor, the setting sun." In this phenomenon, the force of the ascending current is evidently sufficient to draw in the outlying cloud, which ordinarily forms a screen to the observer, and consequently the motion of the thunder-cloud becomes visible.—*R. P., July* 21, 1853.

† It appears very probable that ball-lightning is an instance of that mode of discharge in which the spark passes near to electrified particles, as in the case of those sparks which appear to fly on the glass of the common electric-machine when the electricity is suffered to accumulate. As yet I have not had time to construct apparatus of sufficient delicacy to decide the question.—*R. P., Aug.* 9, 1853.

pressure; the other, the result of electrical attraction, as explained by M. Peltier.

223. Liebig has rendered it probable ("Chemistry of Agriculture and Physiology," chap., *The Sources of Ammonia*) that the quantity of nitric acid at present produced by lightning, is insignificant as a source of ammonia. However, I would suggest the question,—May not much of the nitrogen of ammonia have been combined, primarily with oxygen, by atmospheric electricity, during geological periods, when the temperature of the earth's surface was higher than it is at present? I am disposed, notwithstanding, to look to the direct formation of the cyanides by a high temperature, and their subsequent decomposition by the play of affinities brought about by solution, atmospheric air, and evaporation, and by heat and water ("Chem. Gaz.," May 16, 1853, p. 190), as, perhaps, the chief original source of ammonia.

On Sounds in Electrical Conductors.

224. A great number of observations have been made by different philosophers on the sounds produced in metallic conductors. It has been found that the metals give a sound when a discontinuous current is passing through them within the influence of a magnetic arrangement; and that wires of metals magnetic, like iron, develope sound by the mere transmission of the current through them without any magnet or helix. The following experiments go to prove that all metals give a sound with the electric current; nevertheless, the identity of the force which produces these sounds with that which produces the sounds previously known, seems doubtful. While making some experiments, with a view to ascertain whether any change in the character of the elliptic polarisation produced by the reflection of polarised light from gold-leaf, could be brought about by transmitting an electric current through the gold, I observed, that every time the circuit was completed or broken, a feeble but sharp sound was emitted by the leaf.

225. Two rectangular slips of tin-plate, each $\frac{3}{8}$-inch wide, were supported, so as to have two of their ends in the same plane, and about half-an-inch apart. A slip of gold-leaf, about the same width with the tin-plate, was now attached to both these ends of the tin-plates, by breathing on the tin-plate and then gently pressing the two metals together. The battery used was one on Smee's construction, with six cells, the immersed portion of each plate being 4 inches by 3·5 inches; the battery was generally used as a double series of three repetitions. The wire proceeding from one pole of the battery joined one of the slips of the tin-plate, and the other slip was connected with the other pole, both through an electrepeter, by means of which the circuit could be easily completed or reversed. When the circuit was completed the gold-leaf gave a sound, rather weak, but closely resembling that produced on breaking a circuit, containing an electromagnet, by lifting the end of the wire out of mercury. A similar sound was produced by the gold-leaf when the circuit was broken.

The gold-leaf started every time the circuit was made or broken, which motion appeared to me to be in entire accordance with the magnetic power of the current established by Ampère, and the elasticity of the gold.

226. If these sounds were produced merely by the leaf being agitated by the external magnetic force of the current, then it should follow, that by increasing the violence of these motions, the sound should also increase. The poles of a small, but, for its size, powerful horse-shoe steel magnet capable of sustaining 9 or 10 lbs. by its keeper, were supported at a distance of 3–20th-inch from the gold-leaf. The circuit was completed and broken, and the current was transmitted sometimes in one direction and sometimes in another, but the snapping sound of the gold was unaltered; however, I could perceive a scarcely audible rustling added to it, which could be imitated by merely shaking the gold-leaf near one of my ears. The rustle is thus easily accounted for, by the lively motions which the leaf performs when under the influence of the magnet. On the other hand, the sound not being altered by the proximity of the magnet, goes to shew, that the force which produces this sound differs from that which produces the magnetic sounds hitherto observed. Instead of both poles of the magnet being brought over the leaf, one pole was sometimes employed, but the sound did not vary. The least distance between the poles of the magnet was about 3–16th-inch.

227. A piece of silver-leaf was now introduced into the circuit instead of the gold. It was necessary, to prevent the destruction of the leaf by the current, to make the length of the silver-leaf twice as great as that of the gold-leaf. On completing or breaking the circuit, a sound similar to that given by the gold-leaf, but somewhat stronger, was observed. On bringing the magnet near, the leaf was torn.

228. A piece of brass-leaf (Dutch gold) was now substituted for the silver-leaf, and secured by binding-screws. The plates of the battery were only about one-third immersed, in order to avoid the undue heating of the leaf. The leaf emitted a sound when the circuit was either completed or broken. The sound on completing the circuit was the stronger of the two; this was especially observed the first time the circuit was closed and opened after the leaf had been allowed to rest for some time. On bringing the poles of the magnet near to the leaf, the sounds were much increased, but quite changed in quality, being now a confused rustling, instead of reminding one of the spark; the leaf, was also much agitated, and very nearly the same sound was produced by shaking the leaf. When the leaf was hanging as nearly as could be in a horizontal plane, on completing the circuit, the leaf was observed to fall and become more curved, and on breaking the circuit it took up its former position. These actions, which probably resulted from a heating of the leaf, formed a marked contrast with the lively motions produced by the magnetic force, the leaf taking about one-third of a second thus to fall or rise.

229. To develope these sounds it is as well to have the circuit as

short as possible ; for when a helix of 25 yards of copper wire 1–20th inch diameter was brought into the circuit, the brass-leaf ceased to produce any sound on the transmission or cessation of the current.

230. An individual vibration of the ultimate particles of a metal cannot directly produce sound ; for the minuteness of these particles is such that the period of their vibration must be very much shorter than the period of vibration of the acutest sound. I do not see how these leaves can produce sound except by a bending or buckling of the metal ; which motions, produced by the molecular forces of the current, may be so small as to be invisible. Now, Ampère shewed that two conductors transmitting currents in the same direction attract each other ; and consequently the particles of a conductor mutually attract each other in the same direction, that is, transversely to the direction of the current. Further, Ampère discovered that a conductor could repel its different parts in directions parallel to itself. It therefore follows that the electric current must thus diminish the sectional area and increase the length of a conductor ; and this may cause the buckling of the metal distinct from those other visible motions produced, indeed, by the same magnetic forces, but acting through sensible distances.

231. This contraction of a conductor in a transverse direction should develope a polarising structure. Some experiments were accordingly made with gold-leaf, which it may be as well, perhaps, briefly to describe. A piece of gold-leaf about $\frac{5}{8}$ inch wide was affixed to a plate of glass by breathing on the glass and pressing it on the gold-leaf. A slip of tinfoil about 3–16ths of an inch wide was doubled up two or three times, and this doubled portion was nearly as long as the gold-leaf was wide ; it was then laid upon the gold-leaf, and a piece of tin plate laid upon the foil, which, in its turn, was pressed on by a spring. At a distance of ·5 inch, another similar arrangement of tinfoil pressed upon the gold. Two slits, each about 1–16th of an inch across, and midway between the two tinfoil packings, were now formed in the gold-leaf, which made the width of the gold left between the slits about ·1 inch. The glass plate was now so arranged that a strong pencil of polarised light could be directed on this neck of gold-leaf, and the transmitted light analysed by a Nichol's prism ; while at any moment the current of the battery, arranged either as a series of three or six, could be sent through the gold-leaf. I could not find that the slightest alteration was produced by the current on the light transmitted through the gold. The plane of the polarisation was variously placed with regard to the current ; and two varieties of gold-leaf were employed, one transmitting a blue light, and the other a greenish light. These arrangements gave with the current a sound resembling the beating of a small watch.

232. On account of the thinness of gold-leaf much weight cannot be attached to this experiment as shewing an equality of pressure in every direction in a conductor. I think, however, the following considerations will render it apparent, that, in addition to the magnetic

attraction which is exerted transversely, there is another attractive force acting in lines lying in the direction of the path of the current. In Mr. Armstrong's experiment ("Phil. Mag." S. 3, vol. 23, p. 199), a bridge of water was maintained by the current between two glasses. Without an attraction in lines more or less parallel to the current, this bridge could not have been supported. Many very similar experiments have been given by Dr. Faraday ("Researches in Electricity," vol. 1, p. 507). Again, in the voltaic arc, if such an attraction does not exist, it is difficult to see what maintains the position of the flame, as the repulsive force of the current on itself tends to divide the flame. I conclude from the foregoing, that the sound of the metallic leaves is produced by two attractive forces—one, the transverse magnetic attraction—and the other, the difference between the magnetic repulsion of the current and an attraction acting in the same direction. The action of the magnet seems to shew that this latter force is that most directly concerned in the production of these sounds. What relation this attraction bears to the transverse magnetic attraction does not yet appear; and it would certainly be of much importance to ascertain whether the bridge of water in Mr. Armstrong's experiment has the full magnetic force due to the electric current.

223. The great extension of the metal, which in the foregoing experiments formed a sonorous conductor, enabled it to remain cool. If, instead of the gold-leaf, a piece of copper wire of the same length and 1–20th of an inch in diameter were to be used, and if at the same time the number of square inches of the plates of the battery were multiplied by

$$\frac{\text{sectional area of copper wire}}{\text{sectional area of gold-leaf}},$$

giving a battery of proportional power for the copper wire, doubtless on completing the circuit the copper would be dissipated. By employing an instantaneous current, such as the discharge of a Leyden battery, this dispersion might be avoided; and by using a wire of sufficient length, and a battery of sufficient power, a contraction might be visible on passing the current; while, if the wire became sufficiently softened by the heat, it might be permanently shortened. Now, instances are known in which lightning has made conductors shorter, and also broken metal, apparently, without having fused it. (Lardner and Walker's "Manual of Electricity," vol. 2, p. 168.)

7, *Prospect Place, Ball's Pond Road*, May 19th, 1853.

On the Action of Bromine on the Oils of the Series C^nH^n.

By C. Greville Williams.

I have shewn in a former memoir,* by a considerable number of synthetical experiments on various oily hydrocarbons of the terebric class,

* "On a process for ascertaining the equivalents of some fluid hydrocarbons, by means of bromine."—*Chemical Gazette*, Oct. 1st, 1853.

that, on adding them gradually to bromine in presence of water, a point is at last reached when the smallest further addition determines the formation of a colorless oily compound, which appears to have hitherto escaped examination.

I have also shewn that, by acting upon weighed quantities, and stopping the experiment immediately on the color of the bromine being destroyed, the expression C^5H^4 is invariably indicated as the formula for the molecule which is capable of decolorising one equivalent of bromine.

In effect, we find that thirty-four parts of any of the oils of the terebric class, are capable of saturating eighty parts of bromine, under the conditions mentioned in my former paper.

But, although the values arrived at by this process shew that single equivalents only are necessary to the reaction, yet the formation of hydrobromic acid, which invariably takes place, indicates either that a simple combination is not effected, or else that the compound of bromine and the oil is decomposed immediately after its formation.

The analytical results obtained will not allow me to admit that so much as one-fourth of the hydrogen is removed, under the circumstances indicated.

The subject is not entirely without difficulty, owing partly to the impossibility of purifying the compounds to be examined from their having no tendency to crystallise, and still more from the circumstance that the products of the reaction have an extreme liability to decomposition.

Since we have seen that the liquid above the oily matter, which latter we will for the present call bromide of turpentine, is invariably acid, it became important to determine the ratio borne by the hydrobromic acid to the other products. Many experiments made with this view, shew that, as asserted in the former paper, the amount is variable, owing, it is believed, to the formation in small quantity of a terebric bromide, partly soluble in water, but not precipitated by nitrate of silver. The amount of hydrobromic acid obtained as a maximum result, was one equivalent for every four of bromine present. We will examine these products in succession, in the endeavor to eliminate an equation which shall explain their formation.

Bromide of turpentine . . . $\left.\begin{matrix} C^5H^4 \\ C^5H^4 \\ C^5H^4 \end{matrix}\right\} Br^2 + C^5\left.\begin{matrix} H^3 \\ Br \end{matrix}\right\} HO.$

This substance, which is the more prominent feature in the reaction, is obtained in the form of a colorless viscid fluid, opaque until dried by chloride of calcium, or a surface of sulphuric acid, yet even then retaining an equivalent of water from which I have not yet found a method of freeing it without decomposition. A very slight elevation of temperature gives it considerable fluidity. It is decomposed rapidly below 212° with evolution of hydrobromic acid, and at the same time becomes gradually darker in color, and at last almost black.

It is dissolved by hot nitric acid and converted into a yellow resin,

which may be obtained by throwing the nitric solution into water, when a viscid lemon yellow mass is precipitated, which dissolves readily in alkaline liquids, with a brownish red coloration of considerable intensity.

Analogous compounds are formed by the action of nitric acid on most oily and resinous matters.

Its analysis is attended with considerable difficulty owing to the fact of its evolving bromine in the state of hydrobromic acid, even at common temperatures, during exposure to dessicating agents.

The following three analyses were made by passing the vapor over pure lime at a red heat, and analysing the bromide of calcium formed in the usual manner.

I.	7·470 grs.	gave	10·700	Bromide of silver	= 60·95	per cent.
II.	5·615 „	„	8·170	„ „	= 61·91	„
III.	9·925 „	„	14·350	„ „	= 61·52	„

Mean of three analyses
61·46.

Different preparations were used in each of the analyses—the first two were made with turpentine, carefully rectified, until its boiling point was constant at 312°, the third with oil of juniper rectified below 310°.

It was not possible to introduce the oily bromide into the little glass bulbs in which it was weighed, in the usual manner, owing to its decomposing at a temperature much less than that at which it is converted into vapor; it was therefore necessary first to introduce pyroxylic spirit into the bulbs, and after expelling it by heat to immerse the point of the bulb in the fluid to be analysed until it was filled.

Two combustions were made with chromate of lead, a few inches of oxidized copper turnings being placed in the anterior portion of the combustion tube:—

7·565 gave 8·677 carbonic acid, and 3·08 water.
10·430 „ 11·575 „ „ 4·185 „

We therefore obtain as the composition of this substance:—

	I.	II.	III.	Mean.		Theory.
Carbon	31·27	30·13	...	30·70	20	31·25
Hydrogen	4·52	4·45	...	4·48	16	4·15
Bromine	...	...	61·46	...	3	62·50
Oxygen	...	...	...	...	1	2·10

The impossibility of thoroughly drying the bromide of turpentine without a still greater loss of bromine, is the cause of the considerable difference between the experimental and theoretical numbers. The equation $4(C^5H^4) + Br^4 = C^{20}H^{16}Br^3 + HBr$, is necessary to explain these results.

On the supposition of the correctness of the above formula, 17·7 of hydrobromic acid ought to be formed for every hundred parts of bromine and turpentine in their equivalent proportions present,—thus 33·2 bromine had 14·1 turpentine added to it in presence of water, and with the precautions indicated in my former memoir, now 32·2 + 14·1 = 47·3; but if instead of 47·3, 100 had been used, 17·7 hydro-

bromic acid ought to have been obtained, and, in effect, on precipitating the acid liquid floating above the bromide of turpentine by nitrate of silver 19·10 of bromide of silver was obtained = 8·22 hydrobromic acid :—

Theory.	Experiment.
17·7	17·3

Laboratory, Oxford Court, October 19th, 1853.

THE POTATO DISEASE.

To the Editors of The Chemist.

GENTLEMEN,—Since I last addressed you on the potato disease, I have received numerous communications on the subject from different parts of England and the Continent, and have been repeatedly applied to for a full description of the changes I have proposed to introduce in the mode of cultivation now adopted, with the view of effecting the eradication of the disease. As my time will not allow me to reply separately to each of these applications, I will, with your permission, communicate the desired information through the medium of your journal. I adopt this course the more readily, inasmuch as I am satisfied that the directions I am about to convey will prove interesting, not only to the parties in question, but also to the majority of your readers.

It will be doubtless remembered that the changes just alluded to consist :—1st, in carefully drying the seed potatoes ; 2nd, in steeping them in a dilute solution of the sulphate of copper ; 3rd, in planting them in poor, well-drained land ; 4th, and lastly, in substituting for the farm-yard manure, &c., now employed, some mineral or inorganic compost.

DESICCATION OF THE TUBERS.

The apparatus employed to effect this object should consist of a large heated chamber, similar in character to the so-called "*stoving-room*" of a sugar-refinery, or of a long room fitted up with shelves for the reception of the roots, and heated by means of steam pipes, or stoves placed at intervals, and so arranged that a current of air can be made to pass over the tubers, which can be thus rapidly and effectually dried. The same end may be attained on a small scale by exposing the potatoes in layers on the floor of a warm room, or on a maltster's kiln ; precautions being taken to turn them over occasionally until they have become sufficiently desiccated, and thus to promote a free circulation of the air ; but in practice it will be doubtless found preferable for some enterprising parties to undertake the drying of the roots, which may be afterwards retailed to the agriculturists, &c. Great care, however, I find, must be taken in performing the operation, otherwise the vitality of the tubers is destroyed. A long continued exposure in a dry atmosphere, at a moderate temperature, appears to afford the best results. The latter, under any circumstances, should never much exceed 110° or 112°F. If the process has been well carried out, the dried

roots, when rolled up in a damp cloth, or buried in the ground for a few days, will again become plump and fresh in appearance; whereas, on the other hand, if too high a temperature has been employed, they will, when thus treated, still remain comparatively hard and dry.

STEEPING OR PICKLING PROCESS.

Into a gallon of boiling water put a quarter of a pound of blue vitriol or blue-stone (sulphate of copper), and stir the solution well, from time to time, with a piece of wood, until the salt is completely dissolved. When the temperature of the mixture has been so lowered by evaporation and exposure that the hand of the operator can be immersed without any inconvenience, the dried tubers should be thrown into the vessel containing the "*pickle*," in which they should be kept for one or two hours, care being taken to stir them well two or three times during that interval. After they have been removed from the cupreous solution, and well drained, they should be dusted over with a little air-slaked or mild lime, and planted in the usual way. When, however, the drying process before described has not been resorted to, the tubers should be allowed to remain in the copper solution for thirty or thirty-six hours, and the pickle should be made of double strength.

PREPARATION OF THE MINERAL MANURES.

Mix intimately—

30 lbs. of wood-ashes,
15 lbs. of calcined bones, in fine powder,
10 lbs. of gypsum,
20 lbs. of common salt,
30 lbs. of air-slaked lime, and
7 lbs. of nitrate of soda.

Whilst planting the potatoes, into every hole put about half an ounce of the above compost; cover the latter over with some earth, and then plant the tubers in the ordinary way. This manure may be easily prepared by any one at a very trifling cost, and may be measured out by means of a small tin cup, which, for convenience' sake, should be suspended to the waist of the dibbler. On large farms, where the roots are set in drill-furrows, the compost may be more readily distributed by the manure-drill, or by hand in the usual manner. On most soils, however, a simple top-dressing of lime and salt, in the proportion of two bushels of lime to one of salt, will be doubtless found sufficient; the manure being employed at the rate of fifty or sixty bushels per acre. Where the land is rich, the admixture of cinders, coal-ashes, or shell-sand with the soil will be found decidedly beneficial.

Further observations I shall reserve for a future occasion, when I hope to be enabled to communicate the results of some extensive experiments, as I am informed that several well-known agriculturists—amongst whom is the celebrated Mr. Pusey, of Farringdon—are fully determined to carry my suggestions into practice.—I remain, sir, your obedient servant, THORNTON J. HERAPATH.

Mansion-house, Old-park, Bristol, Oct. 1st, 1853.

CHOLERA IN 1853.

THERE is unhappily no question that epidemic Cholera is again generally prevalent in this country. It has at present only appeared in a virulent form at Newcastle; where, however, there had occurred 1,500 deaths up to the 17th instant, in a population of 90,000—shewing a mortality from this cause only, of one in sixty in forty-six days. When the disease visited Newcastle in 1831–32, the number of deaths in the same time was only 289. It will probably not be until the next Spring that the Cholera will appear with violence over the country generally, and thus time is given to take some of the most obvious precautions against its attacks, or at least to remove some of the most obvious incentives to its propagation. The vexed question of contagion is by no means settled in the minds and thoughts of the unlearned. And although the General Board of Health, and the College of Physicians, have published many distinct assertions that cholera is not contagious, people will judge for themselves upon what seems to them plain and obvious deductions from facts. It is most desirable that the world should be persuaded that there is no contagion in cholera, for fear is an undoubted predisposer to its attacks, and desertion in all kinds of plague is a fearful and aggravating evil.

In the year 1851 a passenger landed from a ship which had just arrived at one of the Canary Islands from the West Indies. The cholera had been very prevalent in the parts whence this passenger had come. He took with him his bed, which had moreover been rolled up some weeks before he left the West Indies. After landing at Palmas, the chief city in the island of Grand Canary, he took up his quarters in one of the caverns dug out of the hills, in which the poor population dwell. These chambers are quite unventilated, and altogether very nauseous places. The bed was unrolled, and the man slept upon it. In a few hours he was attacked with violent cholera, and died; and from that cavern the disease spread with frightful rapidity and mortality amongst the whole population. The inhabitants of Teneriffe and the other islands, placed Grand Canary in the most rigid quarantine; and the result was, that no cases of cholera occurred in them.

On the present occasion cholera has broken out in this country in a part whence the intercourse is most frequent with countries where it had previously prevailed. On the other hand, it will be said that cholera spread from Denmark to Sweden in spite of a strict quarantine. One circumstance may, it seems, be taken for granted as to cholera, viz., that the predisposing causes, and the favorite localities of fever, are also the incentives and haunts of cholera. The law of elevations, deduced by Mr. Farr from the mortality in London during the last visit of cholera, is very remarkable, and is applicable, if not in the same proportions, still relatively it is so, to fever. It has been pointed out that the mortality in districts under a level of 20 feet above high water mark was 102 in 10,000; taking that rate as 1, the mortality in districts from 20 to 40 feet of elevation was ½; from 40 to 60 feet was 1--3rd; from 60 to 80 feet was 1-4th; from 80 to 100 feet

was 1–5th ; and above 100 feet was 1–6th. It is believed that generally something approaching to this relative law may be observed, but of course there are exceptions to every rule.

We do not wish, at the present time, to do or say anything that could discourage or check the active endeavors which are in progress throughout the country, to check the approach of cholera ; we wish rather, if possible, to assist them; but there is one point in which we fear much evil may be done. There is very rightly a prevalent opinion that cholera is preceded by diarrhœa, and that if the latter is remedied immediately on its appearance, the case will most probably *not* pass into cholera. Having arrived at that conclusion, local Boards of Health and other parties have in many place arranged that "diarrhœa medicines" shall be kept made up, and shall be given out to any one applying for them. If this is done indiscriminately, and without proper medical instruction at the time, much mischief may accrue. We would strongly urge on all parties, that in their charitable arrangements they should carefully provide, that early assistance should be easily attained, but always under some medical responsibility.

We could wish that the operations of the "Nuisances Removal and Diseases Prevention Act" were under the direction of some more competent authority than our general Board of Health. It is impossible to have confidence in a Board which, in a Regal Proclamation, orders certain parties to fulfil certain duties, and immediately afterwards orders that if those parties neglect those duties, then other parties shall execute the same. It is giving an *option* to the first-named parties, when the Act distinctly lays a *responsibility* on them. We hesitate, too, in giving confidence to a Board under whose authority drainage works may be so disgracefully executed, as has been the case at Croydon. (*Vide* the report of their own officers on the subject, published recently in the daily papers.) There is strong good sense in all English communities. Let them help themselves, and not rely upon any public Board whatever.

TRANSLATIONS AND ABSTRACTS.

CHEMICAL PHILOSOPHY.

Researches on Chemical Affinity. By RUDOLPH BUNSEN, Professor of Chemistry in the University of Marburg.

(Concluded from page 25.)

ON summing up the results of all these experiments, we find that, in various mixtures of hydrogen gas and oxide of carbon incompletely burned by the oxygen, the products of the combustion correspond to 5 hydrates of carbonic acid, expressed by the following formulæ :—

$$HO\ 2CO^2,\ HO\ CO^2,\ 2HO\ CO^2,\ 3HO\ CO^2,\ 4HO\ CO^2.$$

Is the simplicity of the proportions in which the water and carbonic acid are formed in these combustions due precisely to the formation of these hydrates? This is an hypothesis which naturally presents itself

to the mind, but which some still unpublished experiments on the absorption of gases by liquids induce me, however, to regard as inadmissible. It results, indeed, from these experiments, that the co-efficient of absorption of the carbonic acid in relation to the water, which at 66° 2′ F. is 0·8545, and at 39° 92′ F., 1·4698, is, within the limits observed, exactly proportional to the density of the gases submitted to absorption. The following are certainly conditions incompatible with the formation of a hydrate, and which militate, with much more reason, against the existence of five different hydrates. Moreover, if it were desired to recur to this hypothesis, it would be impossible to explain how the combination HO $2CO^2$ would be formed at the lowest temperature, whilst the combination 3HO CO^2 would be produced at a higher temperature, and the combination 4HO CO^2 at the highest temperature produced by the combustion. These facts, I say, would be contrary to all that has been observed in chemistry relative to the stability of the hydrates; for every one knows that the inferior hydrates support, without being decomposed, higher temperatures than the hydrates containing a greater number of molecules of water. It is, consequently, impossible to admit that the combination 4HO CO^2 which is formed at the highest temperature of combustion, could be formed at a lower temperature, and that in its place are produced the combinations 3HO CO^2, 2HO CO^2, or HO CO^2, containing less water of hydratation.

There is also another circumstance which comes, in the most positive manner, in opposition to the hypothesis above enunciated. We know that none of the numerous combinations which the acids form with several equivalents of water, is capable of resisting high temperatures. In the experiments which we discuss, on the contrary, this singular fact would be observed, that whilst the hydrates of carbonic acid are formed at the highest temperatures, they would be decomposed, *de novo*, after cooling, into carbonic acid and water; and if it be desired to admit that, under the influence of the high temperatures developed by gaseous combustions, phenomena may occur the reverse of those which are produced at lower temperatures, this new hypothesis would only lead us into new difficulties and contradictions; for these are facts of the same order as those which we discuss, which it would be impossible to explain, even by having recourse to this supposition.

The following is one of these facts:—

When we direct a current of oxygen gas over burning charcoal, carbonic acid is formed, which, by the prolonged action of the charcoal, is entirely converted into oxide of carbon gas. If we replace the oxygen gas by steam, we likewise observe an oxidation of the charcoal, accompanied by an elimination of hydrogen. But this reaction does not give use to the complete transformation of the charcoal into carbonic acid; the oxidation stops at the point at which, by the side of four volumes of hydrogen, there are formed exactly one volume of carbonic acid and two volumes of oxide of carbon gas. The following analysis, which I have already communicated on another occasion, shews these relations in the most evident manner:—

	Experiment.		Theory.
Hydrogen . . .	56·52	4 vol.	57·14
Oxide of carbon . .	28·71	2 „	28·57
Carbonic acid . .	14·77	1 „	14·29
	100·00		100·00

This analysis agrees perfectly with that which Clement and Desormes made of gases obtained by the action of steam on charcoal, and which was published more than fifty years ago. The following is this old analysis :—

Hydrogen	56·22
Oxide of carbon	28·96
Carbonic acid	14·63
Marsh gas	0·19 (?)

If, in this case, it be likewise admitted that the products of the combustion are formed according to simple atomic relations, precisely because they combine together according to these proportions, such an interpretation of the facts would lead us into a strange contradiction. Indeed as the products of oxidation contain to three atoms of charcoal four atoms of oxygen, we should be led to make one or other of the following equally inadmissible hypotheses ; either that there is formed at red heat a body presenting the composition of mesoxalic acid, and which, after cooling, is divided, *de novo*, into carbonic acid and oxide of carbon ; or else that an organic combination $C^3H^4O^4$, having been formed at a red heat, is decomposed, at the ordinary temperature, into hydrogen, carbonic acid, and oxide of carbon.

I now point out the very remarkable manner in which cyanogen behaves when it is submitted to incomplete combustion. Indeed, there are formed, in these circumstances, nitrogen, carbonic acid, and oxide of carbon, and that according to very simple proportions, as the law enunciated requires. The experiment besides presents some difficulties ; for, in order that it may succeed, it is necessary to operate at temperatures sufficiently low to prevent the partial oxidation of the nitrogen, which would naturally constitute a cause of error. We are enabled to avoid it when after having determined, by aid of preliminary experiments, the limit of the inflammability of cyanogen, a mixture is compressed which does not completely reach this limit, until it is scarcely exceeded, and it is afterwards inflamed. The temperature of combustion should be sufficiently low not to sublime the mercury in the eudiometer. This experiment gave :—

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m pressure.
Dry cyanogen .	160·0	73°·94′F.	0m 1789	26·39
After the addition of dry air . .	413·2	73°·76′ „	0m 4292	163·47
After the addition of dry oxygen . .	451·5	73°·76′ „	0m 4654	193·68
After the addition of dry cyanogen .	466·0	72°· 5′ „	0m 4741	204·18

The mixture, not yet inflammable by the electric spark, was compressed sufficiently for the vol. 466·0 to be reduced to 314·0. After the combustion of the compressed gas, which was easily effected, we obtained the following residue :—

Vol.	Temperature.	Pressure.	Vol at 32°F. and under 1m pressure.
477·9	39°·42'F.	0·4890	216·36

If we call C′ the dilatation observed after the combustion, and O the volume of oxygen absorbed, we find the quantities of carbonic acid c and oxide of carbon c' formed in the reaction, by means of the following equations :—

$$c = O—C', \; c' = 2C'.$$

In the experiment, O = 28·87 and C′ = 5·965 ; we deduce from these data the composition of the products of combustion :—

	Experiment.		Theory.
Nitrogen . . .	17·42	3 vol.	17·42
Oxide of carbon . . .	11·93	2 „	11·61
Carbonic acid . . .	22·90	4 „	23·22
	52·25		52·25

The gas which was submitted to combustion contained moreover :

Cyanogen	18·05
Oxygen	28.87
Nitrogen	53·08
	100·00

We see, from these experiments, that the oxygen formed, with the carbon of the cyanogen, not pure carbonic acid or oxide of carbon, but a mixture of the two gases in a simple atomic proportion, as if the combination $2CO, CO^2$, or C^3O^4 were formed. By modifying the proportions of the mixture employed, we very soon arrive at a limit at which this atomic proportion is suddenly changed into another, but, as this change is not made without causing, at the same time, a perturbation of the phenomena due to the partial oxidation of the nitrogen, I have renounced the pursuit of the study of these incomplete combustions of cyanogen.

Some experiments on a mixture of carbonic acid, hydrogen, and oxygen, present a less restricted field of observation. When such a mixture is burned, the carbonic acid is found exposed at once to the reductive influence of hydrogen and to the action of the oxygen, which are combated in some measure. Well, we observe also, in these reductions, this remarkable fact, that the portion of the carbonic acid gas which is reduced, is found also in a simple atomic proportion with the portion not reduced.

The contraction which a mixture of carbonic acid and hydrogen undergoes, when it is burned with detonating gas, is equal to the volume of oxide of carbon gas formed. Besides, the volume of this oxide of carbon is equal to that of the carbonic acid reduced. In order to

determine the volume of the carbonic acid which has not been reduced, it is sufficient, consequently, to subtract the contraction observed from the volume of the carbonic acid originally contained in the mixture. Such are the considerations which have served as the basis of interpretation and for the calculation of the following experiments :—

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m pressure.
Carbonic acid employed	184·6	73° 4′F.	0m 1826	31·09
After the addition of hydrogen	378 4	73°·94′ „	0m 3832	133·61
After the addition of detonating gas	639·4	74°·12′ „	0m 6201	365·23
After combustion	356·4	74°·12′ „	0m 3503	115·00

The following, according to these elements, is the composition of the detonating gas submitted to combustion :—

Carbonic acid	8·52
Hydrogen	70·33
Oxygen	21·15
	100·00

Contraction observed after combustion = 18·61.

The part of the carbonic acid which was reduced, was found, consequently, in the following ratio with the part which was not reduced :

	Experiment.	Calculation.
Oxide of carbon	18·61 · 3 vol.	18·65
Carbonic acid	12·48 2 „	12·44

It is evident that things occurred as if five atoms of carbonic acid had been reduced in order to form the compound C^3O^7, or as if the combination $3CO, 2CO^3$ had been formed.

The gas which formed the residue of the last combustion (115·00 vol.) was mixed, *de novo*, with detonating gas, in the proportions hereafter indicated, then submitted to combustion.

	Vol.	Temperature.	Pressure.	Vol. at 32°F. and under 1m pressure.
After the addition of detonating gas	689·5	74°12′ F.	0m 6651	422·43
	362·0	71°96′ „	0m 3566	119·59

It results from these numbers that the combustion of the gas, before the experiment, was the following :—

Oxide of carbon	4·41
Carbonic acid	2·96
Hydrogen	68·37
Oxygen	24·26
	100·00

After the combustion, a dilatation equal to 4·57. We have, consequently :—

	Experiment.		Theory.
Oxide of carbon transformed into carbonic acid . .	4·59	1 vol.	4·65
Oxide of carbon remaining .	14·02	3 „	13·96
	18·61		18·61

The oxidised part and the unoxidised part of the oxide of carbon would correspond to the combination C^4O^4 or $3CO, CO^2$.

To sum up, if we compare the results of all these experiments, it may be said that the reductive action of the hydrogen or the oxidising action of the oxygen occur as if these combinations were formed :—

$2CO\ CO^2$, $3CO\ CO^2$, $CO\ 2CO^2$, $3CO\ 2CO^2$, $HO\ 2CO^2$, $HO\ CO^2$, $2HO\ CO^2$, $3HO\ CO^2$, $4HO\ CO^2$.

Almost all these formulæ correspond to unknown combinations. If we were permitted to regard them as the expression of matters truly existing, it is clear that the experiments whose results they interpret would have given the means of determining previously and by way of experiment, the atomic relations of substances still unknown.

Moreover, whatever may be the signification attributed to these remarkable formulæ, it is evident that we may, henceforth, regard them as expressing a very peculiar *modus agendi* of affinity, and that consequently the numerical relations which are discovered in them, and which may be designated by the name of *coëfficients of affinity*, should be distinguished from the *coëfficients of combination* or atomic numbers (equivalents).

The results at which I have arrived in these researches, are perhaps of a nature to modify the ideas which we form of the mode in which affinity acts. Nevertheless, I abstain at present from developing all the consequences which seem to flow from the law which I have enunciated. I shall wait for that until I have been able to complete my researches, by extending them to liquids themselves, with the view of verifying whether this law is equally applicable to them, and by fixing the conditions which determine the variations of the coëfficients of affinity. In studying the limits within which these coëfficients pass suddenly from one rational relation to another, and in which, consequently, affinity is found near a state of instable equilibrium, we shall doubtless some day throw some light on the influence of other forces, such as light, heat, mass, and contact, which we might, perhaps, arrive at in their most feeble manifestations. I reserve the development of all these considerations for a future work.

"*Annales de Chimie et de Physique.*"

METALLURGY.

On the Chemical Combinations of COPPER *and* TIN, *and their* MIXTURES, *constituting the* NON-CHEMICAL ALLOYS *of these two* METALS.

By M. RIEFFEL.

The principal results contained in this memoir may be arranged as follows:

1st. There exist, at least, seven definite chemical combinations of copper with tin, and the simple law which they follow in their atomic composition leads us to imagine that the number is even greater, as may be judged from the following table:

Atomic Composition.	Composition per Cent.		Color.
	Copper.	Tin.	
"	"	"	White more or less approaching that of tin
$Cu^1 Sn\phi$ "	"	"	
$Cu^1 Sn^{46}$	1·11	98·89	
$Cu^1 Sn^{24}$	2·19	97·81	
$Cu^1 Sn^1$	34·98	65·02	Iron grey
$Sn^1 Cu^{24}$	92·81	7·19	Golden yellow
$Sn^1 Cu^{46}$	96·27	3·73	Orange yellow
$Sn^1 Cu\phi$ $Sn^1 Cu^{72}$	97·48	2·52	Yellowish rose
$Sn^1 Cu^{96}$	98·10	1·90	True rose
"	"	"	"

2nd. The combination equivalent for equivalent, $Sn^1 Cu^1$, is remarkable in this, that the properties of the two metals are almost entirely neutralised, to such an extent that we might say that they have almost disappeared, if it were not that the color is a species of greyish white nearly resembling tin, and the property of being more dense in the liquid state than in the solid state in which it resembles copper.

This compound crystallises in large lamellæ disposed in a very clearly characterised manner, melts at about 552° F., is hard, not tenacious, &c.

3rd. The group of compounds placed in the table under the common designation $Sn^1 Cu\phi$, forms what may be termed *cuprides of tin*. In all, it is the copper which appears to perform the active part, and its properties to predominate over those of the tin, and this increases in proportion as it predominates in quantity. The compound $Sn^1 Cu^{24}$, melts at about the ordinary cherry red heat, or from 1652° F. to 1832° F. The others have their points of fusion nearer and nearer to that of copper. Their crystallisation and texture are very similar to those of this metal. All are more dense in the liquid than in the solid state. All are endued with great tenacity, greater even than that of

copper, and are also harder than it, and this in proportion as the value of ϕ is less; its malleability follows the inverse order to its hardness and $Sn^1 Cu^{24}$ has often appeared to me harder than pure copper.

4th. The entire group of compounds designated by the general formula $Cu^1 Sn\phi$ (*stannides of copper*) are very similar to tin in their properties: color, fusing point, crackling of tin, crystallisation, softness, ductility, all these properties differ only in slight degree from those of tin. $Cu^1 Sn^{24}$ and $Cu^1 Sn^{48}$ both crystallise in needles radiating in all directions from a great number of different centres; the needles of $Cu^1 Sn^{24}$ are larger than those of $Cu^1 Sn^{48}$. We think, but are not positive, that they are fewer in number in proportion to their increased size, and that analogous differences continue in the other $Cu^1 Sn\phi$ in proportion as ϕ augments to $\phi = \infty$, or to pure tin. Likewise, if we are not mistaken (for in this case, also, we have been unable to verify it experimentally), the crystallisation takes place in needles, but of such extreme tenuity, that it is very difficult to prevent them from being carried away during the decantation of the metal remaining in the geodes. What has given rise to this conjecture is, not only analogy, but likewise the similarity of the appearance of the geodes of tin and those of $Cu^1 Sn\phi$, when in the latter the needles could no longer be made to subsist.

5th. In all the definite compounds of copper and tin, the density is superior to that deduced by calculation from the densities of the two constituent metals, when we suppose the volume of the compound to be equal to the sum of the volumes of the two components; and this result takes place, whether we consider the bodies compared in the solid or liquid state, at their maximum of condensation; so that it is independent of the mode of crystallisation. Whence we may conclude that there is an approximation of the molecules in the reciprocal action of copper and tin.

6th. All these same definite compounds are difficult to prepare directly in a state of perfect purity, so that it is necessary for obtaining them thus, not to pass far beyond the degree of heat in which they are produced. The reason of this is, that the various consecutive compounds require for their formation, degrees of heat very nearly approaching each other, so that there is always formed a certain quantity of one of them when the necessary degree of heat is attained, even when the relative proportions of the copper and tin are not such as that the whole of the mass may be transformed into this product.

7th. The non-chemical alloys of copper and tin are simple mechanical mixtures of the two chemical compounds between which their composition is included, at any rate when not over heated; for, in this case, they contain besides (but always in the state of simple mixture), a portion of some compound of a superior order in respect to the copper contained in it. The properties of these alloys or mixtures are always very clearly deducible from those of their immediate components; and this is especially remarkable with regard to the alloys comprised between $Sn^1 Cu^1$ and $Sn^1 Cu^{24}$, which embrace almost all those now in use in the arts, and which, as is well known, are

possessed of properties which have been hitherto inexplicable. Such are among others:

1st. That of becoming harder and less malleable after reheating than after tempering, contrary to what takes place with steel.

2nd. That of having in the solid state only a density which is superior to the mean of the densities of their immediate components (Sn^1 Cu^1 and Sn^1 Cu^{24}), which is here not the effect of the approximation of the molecules due to a chemical action, but that of a mechanical obstacle which prevents the dilatation of the Sn^1 Cu^1, according to its natural tendency, at the moment of crystallising in the pores of Sn^1 Cu^{24} which was solidified a long time before it.

3rd. That of presenting the maximum of this increase of density in the solid state at about the composition of 35 to 36 per cent. of tin; and the maximum of density itself at a little lower point, a maximum which exceeds the maximum of density of copper, and with more reason, that of the group Sn^1 $Cu\phi$.

Comptes Rendus, No. 12, September 19th, 1853.

MINERALOGY.

MINERALOGICAL NOTICES.

Dr. MANROSS has obtained, by fusion, the following artificial minerals in crystals, viz:—*Heavy Spar* BaO, SO^3,—*Celestine*, SrO SO^3,—*Anhydrite* CaO, SO^3,—*Apatite* $3CaO^3$, PO^5 + 2Ca Cl,—*Pyromorphite* 3PbO3, PO^5 + PbCl,—*Wolfram* (Fe, Mn, O) WO^3,—*Tungsten* CaO WO^3,—*Scheeletine* PbO, WO^3,—*Wulfenite* PbO, MO^3,—*Crocoisite* PbO, CrO^3,—*Anglesite* PbO, SO^3,—*Anglesite* was likewise obtained with more satisfactory results by the moist way; the details of the experiments are given in "*Silliman's Journal*," No. 47.

Sir David Brewster has read before the Royal Society of Edinburgh, two interesting papers bearing on the Crystallogenic properties of matter, which are entitled, "On the Production of Crystalline Structure in Crystallised Powders by Compression and Traction," and, "On the Optical Phenomena and Crystallisation of Tourmaline, Titanium and Quartz, within Mica, Amethyst. and Topaz." These are given in detail with a plate, in the *Phil. Mag.* for October.

Dr. Andrews, by microscopical examination and the sulphate of copper test, has detected *Native Iron* in fine grains, in the Giant's Causeway, and Slievenish Basalt, associated with minute crystals of *Augite, Magnetic Iron*, and *Pyrites*.

M. Rœmer has discovered in the Charlotte Mine, Clausthal, a Selenide of Mercury. It is massive, fine granular, blackish-grey mixed with quartz and red oxide of iron, this has been examined by Professor Rammelsberg.

Analysis:—	Selenium.	Mercury.
	25·5	74·5

Formula, probably Hg^6 Se^1.

A mineral described by Professor C. U. Shepard under the name of

Warwickite, and regarded as a Hydrated Silico-titanate of magnesia, iron and alumina, and likewise described by Mr. T. S. Hunt, under the name of *Enceladite*, and regarded by him as a Tri-titanate of magnesia, proves by the results of Professor J. L. Smith's analyses to be a Boro-titanate of magnesia and iron; and is estimated to contain upwards of twenty per cent. of boracic acid, the smallest fragment with the sulphuric acid test, strongly indicates the presence of boracic acid. This mineral is of great interest as being the first Boro-titanate known.

Glauberite differing slightly in its form and angles from *Brongniartine*, and attaining a size of from 1 to 1½ inch, has been examined by M. Ulex. The crystals occur sometimes perfect and transparent, sometimes white and laminated, the fissures being occupied by the substance of the matrix, Sp. Gr. = 2·64 H. 2·5—3—BB, like the Spanish brogniartine.

Analysis:—	CaO	NaO	SO^3	BoO^3
	19·6	21·9	55·0	3·5

Formula.—NaO, $SO^3 + SO^3 + CaO\ SO^3$. The matrix called "Tizza," is a boracic acid compound, from which the mineral derives this acid, probably by mechanical mixture. S.H.

ORGANIC CHEMISTRY.

On the combinations of GLYCERINE *with the* ACIDS, *and on the* SYNTHESIS *of the* PROXIMATE PRINCIPLES *of the* FATS *of* ANIMALS.

By M. BERTHELOT.

GLYCERINE, discovered by Scheele in 1779, was first regarded as a gummy matter peculiar to certain oils, and, so to speak, accidental.* The present notions concerning the nature of, and the part performed by, this body are due entirely to M. Chevreul. In his researches on the natural fatty bodies, he shews that every class of these bodies is susceptible of being divided by the action of the alkalies into two distinct parts, with fixation of water,—a fatty acid on one hand, which remains combined with the alkali (soap), and glycerine, on the other. Whence two hypotheses: either stearine, oleïne, butyrine, &c., "are formed of oxygen, carbon, and hydrogen in such proportions, that one portion of their elements represents a fixed fatty, or volatile acid; whilst the other portion, plus water, represents glycerine;" or else stearine, oleïne, &c., "are species of salts formed of an anhydrous fatty acid, fixed or volatile, and anhydrous glycerine," a constitution analogous to that of the ethers. Another group of fatty bodies "contains cetine, which is characterised by the property of being transformed into margaric and oleic acids and ethal." The constitution of cetine gives rise to the same hypotheses. The latter point of view has since been developed and verified further. On one hand, MM. Dumas and Peligot, have confirmed the analogies of ethal and alcohol, first noticed by M. Chevreul. They formed with this body combinations similar either to the ethers or to cetine. On the other

* Fourcroy, "*Système des Connaissances Chimiques*," t. vii., pp. 142 and 334.

hand, M. Pelouze produced, with glycerine, sulpho-glyceric and phospho-glyceric acids, resembling the vinic acids; he obtained, with M. Géhs, butyrine, "the first artificial neutral fatty matter."

I have succeeded in generalising these results, and in combining glycerine as well with the acids properly so called, as with other acids, either organic or mineral. The bodies thus produced are neutral and incapable of being united with the alkalies. Some are crystallised and others liquid.

They belong to several different series of combinations. One of these series is identical with the natural neutral fatty bodies; another, the most numerous, is analogous to the ethers. All these bodies, moreover, may be represented by acid, plus glycerine, minus water.

All, treated by the alkalies, slowly reproduce the primitive acids and the glycerine. Treated with concentrated hydrochloric acid, they are divided in the same manner. Treated with alcohol and hydrochloric acid, they give rise to a double exchange, and produce glycerine and an ether of the acid originally combined with the glycerine. These two reactions are common to them with the natural fatty bodies. Finally, ammonia converts them into amides.

Submitted to the action of heat, they are decomposed and furnish acroleine, with the exception of those which are volatile. However, stearine, oleïne, margarine, and palmitine, taken in very small quantity, may be distilled in the barometric vacuum without furnishing acroleine and without being acidified. This fact had already been noticed by M. Chevreul, as regards natural combinations.

These various bodies were obtained by the direct union of their two proximate principles, acid and glycerine. This union was effected under the influence of prolonged contact in close vessels with the aid of a more or less elevated temperature. Almost all were already formed at the ordinary temperature, but in very small quantity. In certain cases, they were obtained by the way of double exchange between the ethers and glycerine.

Finally, they are also produced by making hydrochloric acid act on the mixture of glycerine and acid. The two opposite parts performed by hydrochloric acid will be noticed; dissolved in water, and employed in large mass, it causes the separation of the neutral fatty bodies into glycerine and fatty acids; gaseous, and acting on a mixture of syrupy glycerine and acid, it causes the combination of these two principles.

I shall divide this body into two groups:—Combinations of glycerine with the fatty acids properly so called (these combinations reproduce the fatty bodies which are formed in the organic kingdom); combinations with various organic acids, identical with which have none been found in the organic kingdom; combinations with the mineral acids, which are in the same situation as those of the preceding group.

I. *Combinations of* Glycerine *with the* Acids *properly so called.*

A. *Fixed Fatty Acids.*

1. **Stearines.**—Stearic acids (fusible at 158° F., prepared by M.

Chevreul's method) forms, with glycerine, three combinations :—monostearine, distearine, and tetrastearine; the latter is identical with natural stearine.

Monostearine $\left\{\begin{array}{l} C^{38}H^{42}O^{5} = C^{36}H^{36}O^{4} + C^{6}H^{8}O^{6} - 2HO \\ C^{38}H^{168}O^{15} = C^{70}H^{138}O^{7} + 2C^{6}H^{16}O^{6} - 4H^{2}O \end{array}\right\}$ is

obtained by heating to 392° F., for twenty-six hours, equal parts of glycerine and stearic acid. The glycerine and acid remain superposed, without there appearing to have been at any moment a reciprocal solution. The neutral matter produced is likewise insoluble in glycerine. After cooling, the solid layer which floats is separated from the excess of glycerine; it is fused, a little ether is added to it, and then slaked lime, in order to separate the uncombined acid, and it is maintained at 212° F. for a quarter of an hour. This done, it is exhausted with boiling ether (I have proved that this body does not separate the stearic acid from lime in these conditions). There is then obtained a white neutral matter, very sparingly soluble in cold ether, and crystallising in fine birefringent needles. These microscopic needles are ordinarily grouped in rounded grains.

Monostearine fuses at 142° F., and solidifies at 140° F. It volatilises without alteration in the barometric vacuum. Heated in the air, it begins to evaporate, and then is decomposed with formation of acroleine. It burns with a very brilliant white flame.

Treated with oxide of lead at 212° F., it saponifies in a few hours, and re-forms glycerine and stearic acid fusible at 158° F. The weight of glycerine thus regenerated amounts to nearly one-fourth of the weight of the monostearine.

Maintained for 106 hours in contact, at 212° F., with concentrated hydrochloric acid, it is divided almost entirely into glycerine and stearic acid. Acetic acid mixed with alcohol does not decompose it in the same conditions. These various reactions are common to it with natural stearine.

We obtain a similar compound by saturating the mixture of syrupy glycerine and stearic acid at 212° F. with gaseous hydrochloric acid; but the compound thus produced fuses at 112° F., and is combined with hydrochloric acid, which cannot be separated from it.

The mixture of glycerine and stearic acid, left for three months at the ordinary temperature, furnished traces of crystallised neutral fatty matter.

Distearine $\left\{\begin{array}{l} C^{76}H^{78}O^{12} = 2C^{36}H^{36}O^{4} + C^{6}H^{6}O^{6} - 2HO \\ C^{76}H^{154}O^{11} = C^{70}H^{138}O^{7} + C^{6}H^{16}O^{6} - 2H^{2}O \end{array}\right\}$ is ob-

tained by maintaining a mixture of equal parts of glycerine and stearic acid at 212° F., for 114 hours. It is purified with lime and ether, as above. It is a granular, white, neutral fatty matter, crystallising under the microscope, in birefringent, flattened, oblique lamellæ. It fuses at 136°, and solidifies at 131° F. When heated, it gives acroleine. Treated with oxide of lead at 212°, it re-forms glycerine and stearic acid fusible at 158° F.

The same body is obtained, either by heating the stearo-glyceric

mixture to 527° F. for seven hours, or by heating one part of monostearine and three parts of stearic acid to 518° F., or by heating natural stearine with an excess of glycerine for twenty-two hours to 392° F.

Tetrastearine.— $\left\{\begin{array}{l} C^{156}H^{146}O^{16} = 4\,C^{36}H^{36}\,O^4 + C^6H^8O^6 - 6\,H\,O \\ C^{146}H^{282}O^{14} = 2\,C^{70}H^{139}O^7 + C^6H^{16}O^6 - 6\,H^2O \end{array}\right\}$ is

obtained by heating monostearine to 518° F. for several hours, with from fifteen to twenty times its weight of stearic acid. Water is eliminated and is condensed at the upper part of the tube. The combination is not produced by simple fusion; it requires the aid of time. This body is purified with lime and ether as above.

Tetrastearine is neutral, and possesses the composition of natural stearine.

II. MARGARINE.—Margaric acid (of human fat) forms, with glycerine, two neutral combinations, monomargarine and tetramargarine.

Monomargarine ($C^{40}H^{40}O^6 = C^{34}H^{34}O^4 + C^6H^8O^6 - 2\,HO$) is obtained, either at 392° F., or at 212° F. It is formed even at the ordinary temperature, but in very small quantity. Its formation is more easy than that of any other solid fatty matter. It fuses at 133° F., and solidifies at 120° F. Treated with oxide of lead, it regenerates glycerine and margaric acid, fusible at 140° F. Its reactions are similar to those of stearine. Only heated to 212° F. for 106 hours with alcohol mixed with acetic acid, it is partially decomposed, and forms margaric ether and glycerine, which the stearines do not.

Tetramargarine appears to be formed by heating monomargarine to 518° F. with an excess of margaric acid. It cannot be obtained perfectly pure. When saponified, it produces margaric acid, fusible at 140° F. and glycerine.

It will be observed that the stearines, prepared with an acid fusible at 158° F., reproduce an acid fusible at 158° F., whilst the margarines, prepared with an acid fusible at 140° F., reproduce by saponification an acid fusible at 140° F. This confirms the existence of stearic and margaric acids as distinct and permanent bodies.

III. PALMITINES.—Palmitic acid forms with glycerine three neutral combinations analogous to those which stearic acid produces, and obtained in the same conditions :—

Monopalmitine ($C^{38}H^{36}O^6 = C^{32}H^{32}O^4 + C^6H^8O^6 - 2HO$), fusible at 136° F., and solidifiable at 113° F.

Dipalmitine ($C^{70}H^{70}O^{12} = 2\,C^{32}H^{32}O^4 + C^6H^8O^6 - 2HO$), fusible at 138° F., and solidifiable at 124° F.

Tetrapalmitine ($C^{134}H^{130}O^{16} = 4C^{32}H^{32}O^4 + C^6H^8O^6 - 6HO$), fusible at 140° F., and solidifiable at 115° F. The latter is identical with natural palmitine.

All these regenerate with oxide of lead, glycerine, and palmitic acid, fusible at 142° F. Monopalmitine, treated at 212° with alcohol and acetic acid for 102 hours, is decomposed, liberating glycerine, a property which also belongs to natural palmitine.

IV. OLEINE.—Oleic acid (purified by filtering the acid of commerce twice, at about 32° F., converting it into oleate of baryta, and crystal-

lising that salt in alcohol) forms at 392°F. a neutral monoleine ($C^{42}H^{40}O^8 = C^{36}H^{34}O^4 + C^6H^6O^6 - 2HO$.) It is an oily liquid which congeals at about 59°F. Its density is 0·947. Oxide of lead saponifies it very slowly and with difficulty. Alcohol and acetic acid do not decompose it at 212°F.; it has this property in common with natural oleine.

This body is also formed by double exchange by maintaining at 212°F. a mixture of oleic ether, glycerine, and hydrochloric acid. The reaction commences even without the assistance of hydrochloric acid.

Dioleine ($C^{78}H^{74}O^{12} = 2C^{36}H^{34}O^4 + C^6H^6O^6 - 2HO$) is obtained by heating natural oleine with glycerine to 392°F. for twenty-two hours. Its density is equal to 0·921 at 70°F. It begins to crystallise at about 59°F.

It is also obtained by heating monoleine with oleic acid.

B. *Volatile Fatty Acids.*

Such are the combinations which I have obtained between glycerine and the fixed fatty acids. The volatile fatty acids unite likewise with glycerine in the same conditions, giving, by the direct way, various neutral and odorous liquid combinations. These liquids are susceptible of being divided, by the alkalies, by aqueous hydrochloric acid and even by acetic acid or water, into acids and glycerine. Alcohol alone, in great quantity, suffices for commencing this double decomposition, either at 212°F. in forty-eight hours, or at the ordinary temperature with the concurrence of air.

They are prepared by heating the mixture of acid and glycerine, saturating, after cooling, with an excess of carbonate of potassa, agitating with ether, evaporating on a sand-bath, and drying *in vacuo* with heat.

The simple contact of glycerine and a volatile fatty acid, for three months is sufficient, at the ordinary temperature, for determining the combination in very considerable proportion, at least as regards butyric acid.

These bodies are formed also by the action of gaseous hydrochloric acid on a mixture of glycerine and acid, or even of glycerine and of the ether of the fatty acid.

I. **VALERINES.**—I have obtained:—

Monovalerine ($C^{14}H^{14}O^8 = C^{10}H^{10}O^4 + C^6H^6O^6 - 2HO$) at 392° F. an odorous, oily, neutral liquid, of a density equal to 1·100, slowly changing into valeramide by the action of ammonia.

Divalerine ($C^{28}H^{26}O^{12} = 2C^{10}H^{10}O^4 + C^6H^6O^6 - 2HO$), an oily, neutral liquid, of a disagreeable odor, of a bitter and aromatic taste, of the density of 1·059, formed at 527° F. by the action of the aqueous acid on glycerine.

II. **BUTYRINES.**—Butyric acid produces three combinations:—

Monobutyrine, ($C^{14}H^{14}O^8 = C^8H^8O^4 + C^6H^6O^6 - 2HO$), an odorous, oily neutral liquid, of a bitter and aromatic taste, of the density of 1·088, formed either at the ordinary temperature, or at 392° F. in

presence of an excess of glycerine. Saponified with baryta, it furnishes half its weight of butyric acid.

Dibutyrine ($C^{22}H^{24}O^{13} = 2C^8H^8O^4 + C^6H^4O^6 - 2HO$), an oily, neutral liquid, of the density of 1·081, volatile without sensible decomposition towards 572° F., mixing with alcohol and ether, slightly soluble in water, formed either at 527° F., or at 392° F., with the aqueous acid. Saponified with baryta it produces two-thirds of its weight of butyric acid.

Butyridine ($C^{14}H^{12}O^7 = C^8H^8O^4 + C^6H^4O^6 - 3\,HO$), an oily liquid of disagreeable odor, of moderate fluidity, of the density of 1·084, soluble especially in a solution of carbonate of soda, formed by heating to 392° F., a mixture of one part of glycerine and four parts of butyric acid. Treated with ammonia, butyridine is converted in five days into butyramide.

III. ACETINES.—Acetic acid, which I think should be here compared with the fatty acids, produces two combinations :—

Acetine ($C^{10}H^{10}O^8 = C^4H^4O^4 + C^6H^6O^6 - 2HO$), a neutral liquid, of a slightly ethereal odor, of the density of 1·20, obtained at 212° F. ; and **acetidine** ($C^{10}H^8O^7 = C^4H^4O^4 + C^6H^6O^6 - 3HO$), an odorous neutral liquid of a pungent taste, mixing with water, volatile at 536° F., of the density of 1·184. Acetidine is formed in the most varied circumstances, either at 527° or 392° F., in presence of an excess of glycerine or of acid, aqueous or not. Saponified with baryta, it furnishes glycerine and half of its weight of acetic acid.

II. *Combination of glycerine with various organic acids.*

I have combined glycerine with benzoic, sebacic, and camphoric acids by the sole action of time and heat.

Benzoycine ($C^{20}H^{12}O^8 = C^{14}H^6O^4 + C^6H^6O^6 - 2HO$), a very viscid light colored oil, inoxidisable or nearly so, of the density of 1·228, is obtained either at 392°F., or at 527°F.; it is purified, like butyrine, at 392°F., and even at the ordinary temperature it begins to be formed ; it is also produced by the action of hydrochloric acid on a mixture of glycerine and benzoic ether, or even by the direct action, at 212°F., of glycerine in large quantity on benzoic ether. The alkalies change it into benzoic acid and glycerine ; alcohol and hydrochloric acid, into benzoic ether and glycerine, an action which alcohol alone in very large quantity suffices to produce, either at 212°F., or at the ordinary temperature and with the concurrence of air. Finally, ammonia converts it into benzamide.

Sebine ($C^{52}H^{50}O^{16} = C^{20}H^{12}O^8 + 2C^6H^6O^6 - 4HO$) is obtained at 392°F. It is a crystallised, neutral body, resolvable by oxide of lead into sebacic acid and glycerine.

Camphorine is neutral, viscid like thickened turpentine, soluble in ether, and resolvable by oxide of lead into acid and glycerine.

III. *Combinations of glycerine with the Mineral acids.*

I prepared the combination of glycerine with hydrochloric acid or

chlorhydrine ($C^6H^7ClO^4 = C^6H^8O^4 + HCl - 2HO$). This body is obtained by saturating with gaseous hydrochloric acid glycerine, slightly heated and maintaining the solution at 212°F. for thirty-six hours. Without this precaution only traces of product are obtained. The solution is then saturated with carbonate of soda, and agitated with ether, and then the ether is evaporated. The residue of this operation submitted to distillation, furnishes, at 440°F., chlorhydrine. It should also be treated once with lime and ether. It is a neutral oil, of a fresh and ethereal odor, of a sweet and pungent taste, miscible with water and ether, and of the density of 1·31.

It does not precipitate nitrate of silver, at least immediately. It burns with a white flame edged with green, liberating hydrochloric acid. Oxide of lead saponifies it slowly and with difficulty, and furnishes quantities of glycerine and hydrochloric acid nearly proportionate to the equivalents of these bodies.

All the compounds of glycerine, prepared by the action of hydrochloric acid on the mixture of glycerine and acid, contain chlorhydrine united, and, so to speak, alloyed; it could not be separated completely from them. It is produced likewise, but in the state of traces, by the action of concentrated hydrochloric acid on neutral fatty matters.

Comptes Rendus, No. 10, July 5th, 1853.

Contributions to the HISTORY *of the* FATTY BODIES. By M. LEFORT, Pharmaceutist at Paris.

I ENDEAVORED, in a former memoir, to make known the elementary composition of the vegetable fat oils, and the compounds which they produce with chlorine and bromine. I shewed, by numerous analyses, that many oils, although proceeding from vegetables which differ very much as to botanical classification, possess an identical composition, and, moreover, that they all contain four equivalents of oxygen—the quantity found in most of the fat acids.

After examining the reactions produced when chlorine, bromine, and iodine are put in contact with the vegetable fatty oils, it became interesting to investigate whether these metalloïds would not react more particularly on one of their components; on oleïne, for example, more than on stearine or margarine, which, united to glycerine, constitute, as is generally admitted, the greater number of the fatty substances.

The following are the results at which I have arrived:—

Iodine—which, as I have shewn, only reacts on the fat oils by removing from them a very small portion of their hydrogen—does not appear to me to remove a greater quantity from oleïne than from stearine or margarine. The action which this metalloïd exerts on the oils or their component parts, is not to be compared to those exhibited by chlorine and bromine. It is known, from the experiments of M. Pelouze, that glycerine furnishes no peculiar compound with iodine, whereas, with chlorine and bromine, it gives chloruretted and bromuretted glycerine. Under the influence of the nascent state, would not this substance and iodine form iodised glycerine, or an iodide of

glycerine? M. Berthé has already proved that the iodised product obtained with or without the aid of water, differs considerably both in composition and characters.

Chlorine and bromine combine very well with oleïne, margarine, and stearine.

These new substances, to the preparation of which it is unnecessary for me to recur, being all obtained in the same manner as the chloruretted and bromuretted oils, described in my first memoir, are denser than water.

Their consistency presents very remarkable differences. Thus, whereas chloruretted oleïne and bromuretted oleïne are thicker than pure oleïne, the consistency of chloruretted and bromuretted stearine and margarine is not so thick as that of pure stearine and margarine, and, moreover, they melt at a much lower temperature.

This fact is very evident with the margarine of olive oil. In fact, from being solid—as it is, when carefully prepared—it gives, when combined with chlorine and bromine, two products, which have a great resemblance to the corresponding oleïne—to such an extent, that we might ask, whether the solid principle of olive-oil (which melts at 68°F.—whereas animal margarine melts at 116°F.—considered, by some chemists, as margarine in a peculiar isomeric state, and by MM. Pelouze and Boudet as a combination of margarine and oleïne) is not rather oleïne, under a peculiar modification in combination with the margarine which exists normally in all vegetable oils, and of solid oleïne, which would pass to the liquid state, under the influence of the haloïd bodies. It is well known that oleïc acid exists in the solid and the liquid states.

No reagent has shewn that the metalloïds exist in these combinations in a free state.

Stearine, margarine, and oleïne lose, under the influence of chlorine and bromine, a quantity of hydrogen corresponding to four equivalents in the first,—the only one whose exact composition we know.

I have, in fact, obtained,—

With chloruretted stearine	21·17 and 21·43 Cl.
With bromuretted stearine	35·97 and 35·99 Br.

The formula $C^{76}\ H^{66}\ \left\{ \begin{matrix} Cl^4 \\ Br^4 \end{matrix} \right\}\ O^6$ requires

Cl	20·35
Br	36·52

Chloruretted margarine gave	.....	19·12 Cl.
Bromuretted margarine „	.....	35·12 Br.
Chloruretted oleïne „	.....	22·08 Cl.
Bromuretted oleïne „	.....	36·69 Br.

Admitting that margarine and oleïne possess compositions differing but little from that of stearine, the chlorine and bromine would likewise be substituted for four equivalents of hydrogen.

To complete this series of researches, I undertook some experiments on the action which the haloïd bodies exert on the fat acids.

Iodine does not react on the fatty acids produced by the saponifica-

tion of the vegetable fat oils and of the fatty bodies of animal origin.

Chlorine and bromine form no peculiar compounds with stearic and margaric acids completely deprived of oleïc acid; a small quantity of these metalloïds is dissolved by the fatty acids, but they do not produce hydrochloric and hydrobromic acids.

Oleïc acid, on the contrary, is very subject to the law of substitutions.

The acid used in my experiments proceeded from the treatment of oil of sweet almonds by potassa, and it was purified by the process indicated by M. Gottlieb. It belonged to the liquid variety.

Chloruretted Oleic Acid ($C^{36}H^{32}Cl^{2}O^{4}$).

This compound and the following were obtained in the same manner as all the chloruretted and bromuretted combinations which I have made known. It is liquid at the ordinary temperature, denser than water, and possesses, no doubt in consequence of the prolonged heat to which it is necessary to subject it for the removal of its water of interposition, a very decided brown tint. It has an acid reaction with litmus paper. Its consistency is rather greater than that of the pure acid; its density at the temperature of 48°F. is 1·082. It boils at 374°F.

The following are the numbers given by its analysis:—

I. 1·374 of matter gave . . . 1·173 Cl Ag. or 0·289 Cl.
II. 1·516 „ . . . 1·239 Cl Ag. or 0·305 Cl.

Or, in hundredths:

		I.	II.	Calculated.
C^{36}	2700	„	„	„
H^{32}	400	„	„	„
Cl^{2}	888	21·06	20·17	20·23
O^{4}	400	„	„	„
	4388			

Bromuretted Oleic Acid ($C^{36}H^{32}Br^{2}O^{4}$).

Bromuretted oleïc acid possesses very nearly the same consistence as the chloruretted acid, but it has a more marked brown tint. Its density at the temperature of 47°F. is 1·272. It boils at 392° F., and has an acid reaction on litmus paper.

Its analysis gave:

I. 1·802 of matter gave . . . 1·551 Br Ag. or 0·651 Br.
II. 1·486 „ . . . 1·301 Br Ag. or 0·546 Br.

which represents in hundredths:

		I.	II.	Calculated.
C^{36}	2700	„	„	„
H^{32}	400	„	„	„
Br^{2}	2000	36·13	36·76	36·36
O^{4}	400	„	„	„
	550			

Before assigning to the chloruretted and bromuretted oils formulæ which would enable them to be considered as definite compounds, I thought it necessary, knowing the action exerted by sulphuric acid on the fatty bodies, to examine whether chlorine and bromine would not act as oxidising bodies, furnishing chlorides and bromides of glycerine, and chloruretted and bromuretted oleïc and margaric acids. All the experiments which I have tried for this purpose, have shewed me that glycerine did not separate, and that chlorine and bromine reacted on the fat oils and their components, as on simple bodies.

These experiments come anew, to demonstrate that if, in certain reactions, iodine behaves in the same manner as chlorine and bromine, in others, on the contrary, it differs completely.

Notwithstanding the remarkable works of MM. Braconnot, Chevreul, and Le Canu, on fatty bodies, the peculiar constitution of a great number of them still leaves much to be desired.

Before M. Braconnot, the neutral fatty bodies were considered as formed of one and the same substance. He shewed that by subjecting them to pressure at a temperature below 32° F., two matters were obtained, which he called suet and oil. The variable quantities of these two bodies which he obtained with the different oils subjected to examination, led him to imagine that they were not definite compounds. The later researches of MM. Chevreul and Le Canu have put beyond doubt, in most vegetable fatty oils, and in many fatty bodies of animal origin, the presence of three different principles, to which have been given the names of stearine, margarine, and oleïne.

The numerous difficulties encountered in trying to isolate perfectly these bodies one from another, have hitherto prevented chemists from making an elementary analysis of them to determine their equivalents.

On this subject M. Le Canu asked himself whether stearine and margarine, although possessing fusing points which differ a little, had not the same composition: MM. Pelouze and Boudet think, on the contrary, that stearine and oleïne should be placed in the class of isomeric bodies.

If, availing myself of the facts already known, and of those which I have mentioned in this memoir, I compare the reactions which produce stearine, margarine, and oleïne, and their derivatives, stearic, margaric and oleïc acids, above all the conversion of stearic acid into margaric acid by distillation; the negative results produced by chlorine and bromine with stearic and margaric acids, whereas the oleïc acid formed a definite compound; I am more disposed to think that the opinion of M. Le Canu should be preferred, and that oleïne is a peculiar principle, which is to the vegetable fat oils what stearine is to fatty matters of animal origin.

It is not only possible that stearine and margarine are isomeric bodies, but it is probable that each of them and oleïne possess isomeric modifications which then permit of explanation:—the physical differences observed when they proceed from the animal or the vegetable kingdom; the ease with which certain fatty oils absorb oxygen from the air and become drying oils: the different manner in which they

behave with the alkalies and metallic oxides; finally, the identity of composition found between certain vegetable fatty oils, although solidifying at different degrees and proceeding from very different sources.

Stearine, margarine, and oleïne made, whether from the animal or the vegetable kingdom, may be well compared with fibrine, albumen, legumine and caseïne, and all substances of similar origin, which, with the most different properties, still possess an identical composition.

I think, that by means of the considerations which I have just made public, it is possible to consider the vegetable fatty oils, as compounds in constant definite proportions, of oleïne and margarine, susceptible of taking various isomeric modifications; or rather as oleö-margarates of glycerine, which may be separated under the influence of the most feeble forces. They would be to the seeds what the essences are to the flowers, salicine and populine to the barks, finally analogous to many other proximate principles which chemists are daily learning to separate into the most opposite principles.

Journal de Pharmacie, August, 1853.

On the COMPOSITION *of* ESSENCE *of* THYME. By M. A. LALLEMAND.

THE separation of the proximate principles of essences presents difficulties which ordinarily arise from the want of clearness of their reactions. We are obliged to have recourse to fractioned distillation, and to turn to account their various degrees of volatility. This process, which gives good results when the mixed substances have very different boiling points, isolates them only very imperfectly in the contrary case. The essence of thyme presents an example of this: it deposits in time a small quantity of stearoptene, which constitutes half of its composition, and whose presence in such large proportion has nevertheless hitherto escaped observation.

This stearoptene, or, in brief, thymol, presents very distinct composition and properties. It is easily obtained in the crystallised state by evaporating its alcoholic solution. It occurs, in this case, under the form of transparent rhomboïdal tables. Thymol is deposited sometimes from the essence in oblique prisms with a rhombic base, with supplementary facettes on the lateral angles, corresponding to the obtuse dihedral angles. The measure of the angles indicates that the crystalline form of this body is derived from an oblique prism with a rectangular base, and belongs, consequently, to the fifth crystalline system. Thymol has a sweet odor of thyme, and a very pungent and peppery taste. It enters into fusion at 108° F., and distils without alteration at the temperature of 446° F. It may remain liquid for a very long time at the ordinary temperature; but its solidification is again determined by throwing into it small solid fragments of the same substance. This phenomenon of superfusion is one of the causes which have prevented the presence of thymol from being recognised in the products of the distillation of essence of thyme. It is very soluble in alcohol and ether, and very sparingly so in water,

which, moreover, does not precipitate it from its alcoholic solution. It does not possess any rotatory power; but in the solid state it acts on polarised light in the manner of birefringent media, which is a consequence of its crystalline form in this state. Its chemical formula, deduced from five concordant analyses and referred to four volumes of vapor, is $C^{20}H^{14}O^2$. It differs from that of the camphor of the *Laurinaceæ* only by two equivalents of hydrogen less.

Considered in a chemical point of view, thymol is neutral to litmus paper; it however combines with caustic soda and potassa. It dissolves at a moderate temperature in concentrated sulphuric acid. By cooling, the mixture assumes the form of a crystalline mass, very soluble in water. The solution, saturated with carbonate of lead or baryta, gives, by evaporation, saline crusts, which crystallise in absolute alcohol. By means of one of these two salts, we easily obtain all the other sulphothymates, and sulphothymic acid itself. They have the formula $C^{20}(H^{13}S^2O^5)O^2$, MO; that is to say, that sulphothymic acid, like all the analogous vinic acids, results from the combination of one equivalent of thymol, with two equivalents of anhydrous sulphuric acid.

Chlorine powerfully attacks thymol in diffused light. Hydrochloric acid is abundantly disengaged, and when the reaction is over, a yellowish viscid liquid, of a very persistant camphory odor, which is represented by $C^{20}H^8Cl^6O^2$ is obtained.

Nitric acid also exerts a very vivid action on thymol, and resinifies it. By prolonging the oxidation until the resinous matter has almost completely disappeared, an abundant deposit of crystallised oxalic acid is formed.

The property which this stearoptene possesses of combining with caustic soda and potassa, enables us to detect its presence in essence of thyme, and to isolate it from the other principles of which it is composed. M. Doveri has ascertained that this essence furnishes by distillation two liquids, one of which enters into ebullition between 347° and 356°F., and the other between 437° and 455°F. The latter is almost entirely composed of thymol. It is sufficient, indeed, to throw into it a few solid pieces of this substance for the liquid to form a mass in a few days. The portion of the essence which distils between 365° and 437°F., contains more than a third of its weight of it. It is extracted by agitating the product with a concentrated solution of caustic soda. The stearoptene is dissolved, and, after having decanted the supernatant oil, the combination is diluted with water, and saturated with hydrochloric acid; the liquid thymol which is separated soon congeals.

The most volatile portion of the essence contains also a considerable proportion of thymol, which is extracted from it in the same manner. However, the purification is complete only after it has been distilled several times over caustic potassa. A colorless hydrocarbon is collected of an agreeable odor of thyme, which enters into ebullition at 329°F.

Thymene is an isomer of essence of turpentine. It has the same

vapor-density and combines with hydrochloric acid, giving rise to a liquid camphor which possesses a composition identical with that of solid hydrochlorate of camphene. Thymene, as well as its combination, does not possess the rotatory power, at least after purification.

It may therefore be concluded from the researches which I have just described, that essence of thyme is in great part formed of two principles; thymene, a hydrocarburet isomeric with essence of turpentine, and thymol, which may be supposed to be derived by substitution from the former. Admitting, indeed, that thymene fixes four equivalents of oxygen, and that two equivalents of water are eliminated, we have :—

$$C^{20}H^{16} + O^4 = C^{20}H^{14}O^2 + 2HO.$$

Whence we comprehend in the same molecular type the following bodies :—

$C^{20}H^{16}$, thymene ;
C^{20} $(H^{14}O^2)$, thymol ;
C^{20} $(H^3Cl^4O^2)$, chloruretted thymol ;
C^{20} $[H^{18}$ (S^2O^5) $O^2]$, anhydrous sulpho-thymic acid.

The formula of the liquid oxygenous principle of essence of carraway, corrected by M. Gerhardt, is identical with that of thymol. The latter would therefore be an isomer of carvacrol.

Comptes Rendus, September, 26th, 1853.

On Esculine. By MM. Rochleder and Schwarz.

Esculine, the crystallisable principle extracted from chesnuts by boiling water, occurs in a state of purity under the form of brilliantly white prismatic crystals, often united in tufts. It is inodorous, and possesses a bitter taste. Its solution is precipitated by sub-acetate of lead, and the yellowish precipitate is partially decomposed by washing. MM. Rochleder and Schwarz express its composition by the formula $C^{42}H^{24}O^{26}$.

When heated in the sand bath with a sufficient quantity of water to dissolve it hot, to which has been previously added 1-8th of its volume of sulphuric acid, the liquor becomes yellow, and, after some time, it is deposited on the edges of the capsule in needles, the quantity of which augments by degrees. This crystallised body is a new principle, which the authors call *esculetine:* there is formed at the same time a certain quantity of glucose, which remains in solution. The esculetine forms, in a state of purity, lamellæ or needles, similar to benzoic acid. Sparingly soluble in cold water, it dissolves better in boiling water, and very easily in boiling alcohol. It possesses the characters of a weak acid, for it dissolves readily in slightly alkaline water. This solution, of a golden yellow color, when an acid is added, precipitates esculetine in the form of fine silky needles. Esculetine forms, with ammonia, a compound crystallisable in yellow lamellæ but which loses, by exposure to the air, all the ammonia contained in it

The authors have found in esculetine, when dried at 212° F. :—

	Mean of Experiments.		Theory.
Carbon	60·68	C^{18}	60·67
Hydrogen	3·53	H^{6}	3·37
Oxygen	„	O^{8}	35·96
			100·00

These numbers lead to the formula $C^{18}H^{6}O^{8}$.

They explain the decomposition of esculine into esculetine and glucose by the following equation :—

$$\underbrace{C^{42}H^{34}O^{26}}_{\text{Esculine.}} = \underbrace{C^{18}H^{6}O^{8}}_{\text{Esculetine.}} + 2\,(C^{12}H^{10}O^{10}).*$$

Annales de Chimie et de Physique, July, 1853.

Chemical Composition of BRAN. By. M. POGGIALE.

FOR some years scientific and practical men have been much occupied with the composition and nutritive value of bran, and with the part it takes in panification. Some consider this product as an essentially alimentary substance, richer in gluten than wheat, and others as a very hurtful element. These say particularly that it absorbs and retains a considerable proportion of water, requiring very powerful yeast, giving a brown color and an acid taste to the bread; of preventing it from keeping, of favoring the formation of sporules of various fungi, and finally of being worthless as nourishment for human beings.

Having frequently been consulted on those important questions, M. Poggiale thought it would be better to subject to experiment the contradictory assertions which chemists have advanced on this point.

Is the quantity of gluten and starch contained in bran so great as has been latterly supposed? Should we consider as alimentary substances all which is removed from it by the acids, alkalies, and solvents used to obtain pure cellulose? May we, without inconvenience, leave in bread all the bran contained in flour? Such are the questions which he had to decide, before being able to give the opinion which had been asked from him.

The analysis of the bran proceeding from ammunition flour bolted to 15 per cent., gave the following results :—

Water	12·65
Substances soluble in boiling water . . .	30·82
Substances soluble in hydrochloric acid diluted with 20 times its weight of water . .	34·37
Substances soluble in a solution of potassa containing 10 per cent. of alkali . . .	12·74
Resisting cellulose	9·42
	100·00

By subjecting bran to several treatments with acids and alkalies,

the proportion of resisting cellulose became only 5·73 per cent., instead of 9·42, and, by using concentrated solutions, the residue came only to 4·53 ; but then the cellulose appeared to be attacked.

As bran only leaves after the action of these solvents 5·73 per cent. of cellulose, it is generally admitted that it is very rich in nutritive and panifiable substances, and that the loss which it sustains represents the proportion of alimentary matter.

This consequence, says M. Poggiale, does not appear to me so certain, because the less aggregated bran found inside the grain, is dissolved, as I have proved, by the alkalies and acids, and even water easily disaggregates it when its organization is not complete. The ligneous part of bran likewise contains other substances which are not alimentary, such as coloring, extractive, resinous, gummy matter, &c., and which nevertheless are dissolved in the separation of the cellulose.

The investigations which I have made proved that the proportion of non-assimilable matter contained in bran is very considerable.

Journal de Chimie Médicale, Sept., 1853.

ANALYTICAL CHEMISTRY.

On a Method of VOLUMETRICAL ANALYSIS *of very general applicability.*

By R. BUNSEN.

AN objection may very fairly be urged against the cultivators of analytical chemistry, that they have recently directed their attention too exclusively to analytical determinations by weight, and have at the same time almost neglected the volumetrical method, with solutions of infinite value. The reason of this would appear to consist in the circumstances that most of the processes adopted in volumetrical analysis are but little adapted to satisfy the more rigid demands of scientific precision, and that almost every individual determination, by this method, involves peculiar preparatory operations, which in many instances are far from simple, and the trouble of which is commensurate with the result only in those few cases where it is required to execute a numerous series of repetitions of one and the same determination: consequently any considerable scientific advantage from the volumetrical method, can only be anticipated from the establishment of some general process, which will serve as a substitute for all those now employed, admit of a higher degree of accuracy than any of them, and at the same time be of such simplicity that in a few minutes the same results may be obtained, which, by the method of weight determination, require many hours or even days.

The process which is described in the present memoir, comprehends only those analytical determinations which are attended with reduction or oxidation, and it may be regarded as a contribution towards the solution of the above problem. The principle upon which it is based consists simply in liberating a quantity of iodine equivalent to the substance to be determined, and then making a volumetrical determination of this quantity of iodine, by means of sulphurous acid.

When for this purpose the method proposed by Dupasquier is adopted, great care must be taken to avoid a source of error which, although it renders the whole process fallacious, has nevertheless been overlooked. It is well known that iodine and sulphurous acid share between them the elements of water in such a manner that sulphuric acid and hydriodic acid are formed.

1. $I, HO, SO^3 = HI, SO^3$

But, on the other hand, sulphuric acid and hydriodic acid are mutually decomposed with production of iodine, sulphurous acid and water.

2. $HI, SO^3 = I, HO, SO^3$

because the value of the quantity which I have termed cöefficient of affinity is variable according to circumstances. Consequently Dupasquier's process is altogether delusive when those conditions are not fulfilled under which the reaction represented by the equation 2 disappears, or becomes unappreciable. Experiments which I have instituted with reference to this point, shew that a solution of sulphurous acid is perfectly oxidised to sulphuric acid by means of iodine, only when its percentage of anhydrous acid does not exceed 0·04 or 0·05 by weight. When the percentage of acid is greater than this, the process no longer furnishes corresponding results, as may be seen from the following observations.

A mixture, containing 0·18 per cent. of the anhydrous acid by weight, gave, by the iodometric test, according as the addition of the liquid was more or less rapid:—

Number of degrees of the burette.	Percentage of acid indicated.
97·3	0·1859
94·5	0·1805
96·0	0·1834
96·5	0·1844
98·4	0·1880

The same acid, diluted with an equal volume of water, gave results which are somewhat more uniform:—

Number of degree of the burette.	Percentage of acid indicated.
46·0	0·08788
46·4	0·08865
46·8	0·08941
46·3	0·08846
46·6	0·08903

It was not until the dilution of the liquid reduced the percentage of acid to about 0·04 by weight that the iodometric test ceased to give results presenting greater variations than are attributable to the unavoidable errors of observation.

Number of degrees of the burette.	Percentage of acid indicated.
47·4	0·04528

47·6	0·04547
47·4	0·04528
47·6	0·04547
47·5	0·04537

With a solution containing between 0·04 and 0·02 per cent. by weight of anhydrous sulphurous acid, a still more close correspondence —to within 1–1000th—between the several results can easily be obtained, especially when a considerable volume of the acid liquid is employed for the experiment. However, too great dilution is attended with other inconveniences.

After these general remarks, I may proceed to the consideration of the process itself. It requires only three test-solutions—a solution of iodine, another of sulphurous acid, and a third of iodide of potassium. In order to prepare the first, pure iodine is dried at the ordinary temperature over chloride of calcium, g grammes weighed between watch-glasses, and dissolved in a litre measure of concentrated solution of iodide of potassium,which has previously been ascertained not to present any brown color either alone or immediately after the addition of hydrochloric acid. When one degree of the burette is equal, as usual, to 0·5 cubic centimeters, the solution obtained is diluted with so much water that its volume amounts to $\frac{g}{0{\cdot}005}$ cub. cent. Each degree of the burette then contains 0·0025 grm. of dissolved iodine. As commercial iodine, even the purest, generally contains traces of chlorine, it is necessary while making a determination to take this impurity into account. For this purpose, a weighed quantity of the dried iodine is dissolved in cold sulphurous acid, the liquid precipitated by a silver solution, and the precipitate digested with nitric acid before filtration, in order to remove any possible admixture of sulphate of silver. If the quantity of iodine used is represented by A, the quantities of iodine and chlorine present in it, by x and γ, and the quantity of mixed chloride and iodide of silver obtained, by B, then:—

$$x + \gamma = A \text{ and } \frac{Ag + I}{I}\, x + \frac{Ag + Cl}{Cl}\, \gamma = B$$

or, if making $\frac{Ag + I}{I} = \alpha$ and $\frac{Ag + Cl}{Cl} = \beta$, then $\gamma = \frac{B - \alpha A}{\beta - \alpha}$

From this quantity γ, it is easy to calculate the quantity of pure iodine which is equivalent to the impure substance containing chlorine. The quantity of chlorine γ must likewise, in accordance with stochiometric laws, exercise the same oxidising influence as the quantity of iodine $\frac{I}{Cl}\gamma$; consequently the iodine containing chlorine A, produces the same oxidising effect as the quantity of pure iodine $A - \gamma + \frac{I}{Cl}\gamma$.

But hence may be ascertained the weight of pure iodine a′ which corresponds to the impure iodine—a—contained in one burette degree:—

$$a' = a + \frac{a}{A}\left(\frac{B - \alpha A}{\beta - \alpha}\right)\left(\frac{I}{Cl} - 1\right).$$

1·4739 grm. of the iodine employed in the following experiments gave 2·7498 grm. of iodide and chloride of silver. In another determination, there was obtained from 1·7456 grm. of iodine, 3·2521 grm. of iodide and chloride of silver. If these values are substituted for the values of a = 0·0025 in the equation, there is obtained as the quantity of pure iodine present in one degree of the burette according to the first experiment 0·0025426 grm., and according to the second 0·0025348 grm., consequently the mean value of a = 0·0025387.

A much more simple mode of ascertaining the quantity of pure iodine in a burette degree when the iodine employed is not pure, will be pointed out subsequently when treating of the volumetrical analysis of chromate of potassa.

When the iodine solution thus prepared is employed at a temperature different from that at which it is prepared, an error is incurred in consequence of the change of volume of the liquid to be measured. Since, however, this change for a variation of temperature of more than 10° C. does not produce a difference in the determination of the iodine of so much as 2–1000ths of the quantity of iodine to be determined, amounting at the utmost to 0·2 or 0·3 grm., it may, on this account, be disregarded within this range of temperature, and still more so from the circumstance that the atomic weight of iodine is in most instances so much greater than that of the substances to be determined.

The volumetric measurement of this perfectly unchangeable solution of iodine is best effected in a burette, the degrees of which contain 0·5 cub. cent. of liquid. In order to avoid parallax in reading off the quantity, the instrument should be held loosely by the upper end between the thumb and index finger, so as to hang perpendicularly, and the level should be read off from the lower curve of the liquid meniscus as soon as it coincides with a horizontal line. In this manner 1–10th of a degree may be read off with certainty, especially when a few minutes are allowed to elapse, until the liquid adhering to the sides of the burette has run down and the level become constant.

It is advantageous to prepare about twenty or thirty litres of the test-solution of sulphurous acid at once, so that the alteration in the percentage of acid effected by the access of air during an experiment is inappreciably small. In such a quantity of liquid the diminution in the sulphurous acid, in a vessel half filled with air, amounts within the space of twenty-four hours to about 1° or 2° of the burette, which, during the three or four minutes occupied by an experiment, is not equal to more than 8–1000ths of a burette degree, or 0·00002 milligrammes of iodine, a quantity which is altogether inappreciable. In order to bring the acid to the proper degree of dilution, twenty or thirty litres of water are mixed with a small volume of concentrated sulphurous acid, 100 burette degrees measured, mixed with a solution of starch, and tested with the test-solution of iodine. If τ degrees of this solution are required to effect the perfect oxidation of the acid

there must be added $\frac{0{\cdot}03\ I}{a\,SO^2\,\tau}$, or when a is about 0·0025 grm., $\frac{48}{\tau}$ of the former volume of sulphurous acid, in order to bring the whole of the liquid to the desired degree of strength, namely, about 0·03 sulphurous acid in 100 water.

The third test liquid is a solution of iodide of potassium, containing about 1 grm. of the salt in 10 cub. cent. of water. The purity of the iodide may easily be ascertained by the absence of any brown coloration of the solution when exposed to the air, or immediately after the addition of hydrochloric acid.

I. *Iodine Determination.*

The weighed quantity of iodine is dissolved in a capacious beaker-glass in a solution of iodide of potassium in the proportion of about four or five cub.-cent. for every 0·1 grm. of iodine. To the brown solution thus obtained is added so much of the normal solution of sulphurous acid, measured off in a stoppered cylinder, as is sufficient to destroy the brown color completely, in which operation the acid adhering to the sides of the cylinder is washed into the beaker by distilled water, and the measure again rinsed with the acid solution before each filling; it then remains to determine the quantity of iodine x which has served for the partial oxidation of the sulphurous acid added in excess. For this purpose it must in the first place be determined what quantity of iodine is requisite to effect the oxidation of the excess of sulphurous acid present. This is done by mixing it with three or four cub. centimetres of very dilute, clear solution of starch, and then adding, drop by drop, the normal solution of iodine. If before the appearance of the blue color t′ burette degrees have been added, and if one burette degree contains a quantity of iodine a, then the quantity of iodine necessary to effect the oxidation of the n measures of sulphurous acid added will amount to $x + a\,t'$. When further the quantity of iodine a t which is necessary for the oxidation of one measure of sulphurous acid solution is determined by means of the burette, the equation $x + a\,t' = n\,a\,t$ is obtained, and hence $x = n\,(n\,t - t')$. Consequently, if the quantity by weight of iodine taken is represented by A, the percentage of iodine which it contains is expressed by $x = \frac{100\,a}{A}\,(n\,t - t')$. If $\frac{100\,a}{A}$ is taken as $= 1$, that is, if the weight of iodine taken is exactly 100 a, then the difference between the two volumetrical determinations, $n\,t - t'$, indicates in a very simple manner the percentage of iodine, in the substance examined.

0·7979 grm. of chemically pure iodine mixed with 1·0400 grm. of iodide of potassium gave, when tested in this manner,—

$n = 5$; $t = 73{\cdot}2$; $t' = 52{\cdot}0$; $a = 0{\cdot}0025387$.

	Employed.	Volumetrical Analysis
Iodine	43·41	43·37
Iodide of Potassium	56·59	56·63
	100·00	100·00

II. *Chlorine determination.*

Chlorine decomposes a solution of iodide of potassium, even in the cold, instantly and completely, an equivalent of iodine being liberated; when this is determined volumetrically in the manner described, the quantity of chlorine sought, x may be ascertained by means of the equation :—

$$x = \frac{Cl}{I} a(n\,t - t').$$

or, the percentage when the quantity A, used for the experiment is weighed :—

$$x = \frac{100\,Cl}{A\,I} a(n\,t - t').$$

If again for A, the weight $\frac{100\ Cl}{I}$ a is taken, the difference of the volumetrical determinations gives directly the percentage of chlorine.

"*Annalen der Chemie und Physique*," July 1, 1853.

(To be continued.)

INDUSTRIAL CHEMISTRY.

Monthly Review of PATENTS.

UNDER this head, it is our intention to give abstracts of the specifications of all patents, which have been enrolled within the period of publication, with critical remarks on the processes patented. In order that we may be able to form our opinion exactly, we shall apply, by letter, in every case to the patentee, to shew us his process in operation, and to explain its points clearly to us. We have no doubt that in most cases this appeal will be responded to ; but in the instances in which it is not, we must content ourselves with forming an opinion on the specification. Our object is to protect the public from valueless patents, and to exhibit the merits of good ones,—to aid in the encouragement of real improvements, and in the exposure of patents which are got up for sale, and are totally destitute of practical value.

The carrying out of this plan will add greatly to the labor already bestowed on the Chemist, and will make very heavy demands on our time. We hope, however, that patentees will aid us as much as they can.

AGRICULTURAL CHEMISTRY.

On the GUANOS *of Commerce.* By M. J. GIRARDIN, of Rouen, Correspondent of the Institute.

(Concluded from page 42.)

THE proportion of water was determined by slightly moistening a sample of guano with a few drops of hydrochloric acid, taking a fixed

weight of this guano, and drying it in a porcelain capsule, at 212°. In this manner, the powder lost all its water without a trace of ammonia.

The rough quantity of organic matters and ammoniacal salts was obtained by gently incinerating in a platinum capsule, ten grammes of guano. The loss, substracted from the water contained in each sample, gave the proportion of the matters destroyed by the heat.

The ash, the weight of which was known, was lixiviated with boiling water in order to obtain the respective weights of the soluble and insoluble salts. The insoluble residue was boiled with hydrochloric acid, and all the phosphate of lime was precipitated from the acid liquor by means of a slight excess of ammonia.

The portion of the ash which resisted the successive action of boiling water and hydrochloric acid, consisted of sand and silicious pebbles.

The potassa was determined by exhausting a known weight of guano with boiling water, filtering, neutralising the liquor with hydrochloric acid, adding alcohol, concentrating to two-thirds, filtering to separate the sulphate of lime, which exists in very large proportion in some guanos, and precipitating the potassa by means of chloride of platinum. The precipitate, collected on a filter, and well washed in alcohol, was afterwards dried at 212° F., and weighed.

The total nitrogen of each guano, that is to say the nitrogen arising as well from the ammoniacal salts as from the nitrogenous organic matters, was obtained by the combustion of one gramme of powder with soda-lime, according to M. Peligot's method.

To distinguish the quantity of nitrogen resulting from the ammoniacal salts, and to determine, in consequence, the proportion of ammonia contained in each guano, I followed the method proposed by M. Melsens, which I found sufficiently accurate, that is to say that one gramme of matter for good guanos, five and even ten grammes for bad guano, were rapidly introduced into a phial containing a concentrated solution of chloride of lime. The gas (nitrogen) arising from the reaction, which was effected at the ordinary temperature, was received into a tube graduated in cubic centimetres; its volume, measured after a contact of one hour, gave that of the nitrogen contained in the ammoniacal salts.

The following are the results obtained from the analysis of thirteen samples :—

	1.	2.	3.	4.	5.	6.	7.
Water	8·9900	20·054	17·160	20·300	11·100	17·520	18·800
Sand and pebbles .	1·2000	1·250	1·000	1·190	10·400	15·400	4·300
Phosphate of lime .	24·0000	24·000	24·500	28·000	25·500	37·000	40·000
Other insoluble salts	2·6000	3·000	0·500	2·700	20·700	11·238	5·800
Potassa	0·9648	2·319	2·894	1·061	2·180	2·162	2·026
Other soluble salts .	5·0352	2·981	4·306	0·239	0·920	1·380	10·974
Organic matters, and ammoniacal salts .	57·2100	46·396	49·640	45·510	29·200	15·300	18·100
	100·0000	100·000	100·000	100·000	100·000	100·000	100·000
Nitrogen per cent. .	11·30	12·18	13·47	14·58	11·30	2·66	4· 48
Ammonia per cent.	4·90	8·23	7·04	4·90	2·29	2·30	1·416

	8.	9.	10.	11.	12.	13.
Water	12·740	15·025	19·740	21·500	15·300	18·0000
Sand and pebbles . .	3·710	2·245	2·280	17·700	20·000	16·0000
Phosphate of lime . .	18·000	31·800	34·800	35·600	11·500	33·8000
Other insoluble salts .	38·200	25·200	23·200	1·100	18·350	12·3000
Potassa	0·771	0·578	1·824	2·500	0·676	0·4824
Other soluble salts .	14·329	13·622	8·576	0·300	2·874	8·8176
Organic matters and ammoniacal salts.	12·250	11·530	9·580	21·300	31·300	10·6000
	100·000	100·000	100·000	100·000	100·000	100·0000
Nitrogen per cent. .	1 ·82	1 ·82	1 ·09	4·82	4·12	1·250
Ammonia per cent. .	0·183	0·183	0·176	0·76	Traces.	Traces.

These tables shew what a diversity there is in the matters sold in commerce under the name of *guano*, and what disappointment must await those agriculturists who substitute indifferently one of these matters for another for manuring their lands.

It is evident that he who should employ the last seven guanos of the table, in the same proportions as the guanos of Peru, would obtain none of the energetic effects which the latter produce; and that, in reality, he will have introduced into his land only almost inert matters, for in guanos, it is especially the ammoniacal salts and potassa which constitute the value of the manure. The phosphate of lime, the non-nitrogenous organic matters, and the soluble and insoluble salts are only of secondary importance, the water, sand and pebbles, of none.

The agriculturist of these countries who wishes to buy guano, because he knows its wonderful activity, is therefore now in the same position as the Breton agriculturists, as regards the animal charcoal of the sugar refineries; for the first of these manures now presents as much variation in its value as the latter. This was not the case some years ago when none but Peruvian guano was known. This guano presents scarcely any variation in its composition; it is, so to speak, a well defined substance; it is known that in 100 kilogs of genuine Peruvian guano, there are, on the average:—

2·5 to 3 kilogs of potassa;
24 kilogs of phosphate of lime;
12 kilogs of nitrogen, nearly half of which is in the state of ammoniacal salts.

With good Peruvian guano, we are certain beforehand of the results which will be obtained by employing it in the proportion of 400 kilogs (8 cwt.) to the hectare (2½ acres).

But with these pretended guanos which are found in large masses in all the islands of the New World, and which are, for the most part, only earth mixed with some little excrements of birds, we can reckon on nothing, since their chemical composition is exceedingly variable, and since they very frequently contain only insignificant traces of nitrogen and ammoniacal salts.

It is very true that these pretended guanos are commonly sold at lower prices than Peruvian guano; but the difference of price is not proportionate to their relative inferiority as manure, and there is

always a disadvantage in buying them, for, as they operate only very feebly on the plants and on the soil, not only do we lose great part of the purchase-money, but moreover, the time in which we might have had a good crop; now, as time is money to the agriculturist, he must not be deceived.

The following are a few very simple calculations, which clearly demonstrate the losses to which the agriculturists who buy guanos other than those of Peru are exposed.

We have said above, that good Peruvian guano operates well in the proportion of 400 kilogrammes to the hectare. Its nitrogen-manure (12 kilogrammes on the average in 100 kilogrammes) is worth twenty-five francs (£1). This is the starting point for all the comparisons to be made between the various guanos. Admitting these bases, which are adopted by all competent persons, the following are the proportions to be employed of each of the various guanos mentioned above, with the actual value of each of them expressed in francs compared with their present market prices.

Name of the guano.	Quantity necessary for 1 hectare.	Actual value of 100 kil.		Market price of 100 kil.	
	kil.	Franc.	Cent.	Fr.	Fr.
Good guano from Peru (monopoly) . .	400 „	25	„	25 to 30	
White Bolivian guano .	329·21	30	37	„	
Bombay „ .	414·77	23	54	20	
Chililag „ .	1804 „	5	54	„	
Yellow Chili „ .	1071 „	9	35	„	
Patagonian, Nos. 1 & 2 .	2626 „	3	79	„	
Of the Ducouédic .	4403 „	2	27	25 to 27	
Of the Ave Maria .	996 „	10	04	18	
Of the Edwige . .	1165 „	8	58	16	
Of the Bayard . .	3840 „	2	60	20	

We shall now see to what the manuring of a hectare (2½ acres), with the various guanos, amounts, when they are employed in such a manner as to produce the same effects as good Peruvian guano.

Cost of Manuring 1 *hectare* (2½ *acres*) *at the present market price.*

	Francs.	
Good Peruvian Guano .	112 to 120	(£4 10*s* to £4 16*s*)
	Francs. C.	
Bombay Guano . .	84·75	(£3 8*s*)
Guano of the Ducouédic .	1100·75	(£44)
Ave Maria . . .	179·28	(£7 3*s* 6*d*)
Edwige	186·40	(£7 10*s*)
Bayard	768	(£30 15*s*).

These cyphers say more than the best arguments.

If agriculturists made it a rule before buying a manure of commerce to have it tested by a competent chemist, there would be no great evil in this importation of bad guanos, for the latter would remain in the hands of their owners, and Peruvian guano alone would

be employed; but unfortunately this is not the case. Our farmers look generally at the ticket on the bag; and however trifling the reduction in the price offered to them, they buy in preference guano at sixteen to twenty francs the 100 kilogrammes, not reflecting that there is always more economy in paying a good price for a good manure, than in paying a low price for a bad one.

It is necessary, therefore, owing to this improvidence on the part of agriculturists, and of this invasion of bad guanos, that the agricultural societies should bestir themselves and seek for means of preventing fraudulent dealers from imposing on the good faith and ignorance of the country people.

As the most efficacious means of attaining this end, M. Girardin proposes to place the sale under government *surveillance* as has already been done with beneficial effect, in the department of the Loire-Inférieure, as regards the waste animal charcoal of sugar refineries, and other manures. The following regulation has been proposed as regards guano. The name of the place whence it comes, and its qualitative chemical composition, must be inscribed on a ticket fixed to the bag.

Journal de Pharmacie, August, 1853.

PATHOLOGICAL CHEMISTRY.

Chemical Examination of the PULMONARY TUBERCLES *of the* PULMONARY MATTER, *and of the* LYMPHATIC GANGLIONS *of the* BRONCHIÆ *of a* BULL *which died of* PERIPNEUMONIA. By M. J. L. LASSAIGNE.

Pulmonary Tubercles.

Moisture	27
Albuminoid matter	62
Sub-phosphate of lime	11
Traces of carbonate of lime	
	100

Pulmonary Matter.

Moisture	65·4
Soluble albumen	2·6
Concrete albumen	25·0
Fatty matter	1·7
Sub-phosphate of lime	5·3
Traces of carbonate of lime	
	100·0

Lymphatic Ganglions

Moisture	59·2
Soluble albumen	3·5
Albumen and organised matter	31·0
Fatty matter	0·2

Sub-phosphate of lime	6·1
Traces of carbonate of lime . . .	
	100·0

Journal de Chimie Médicale, October, 1853.

PHOTOGRAPHY.

On some CAUSES *of* FAILURE *in* PHOTOGRAPHIC OPERATIONS, *and on the* PRECAUTIONS *necessary for avoiding them.* By M. BERTSCH.

SINCE the employment of very rapid processes, all of which require the use of very unstable combinations, many operators have complained frequently of obtaining incomplete or even negative results. These failures have been attributed to various causes, none of which give a satisfactory account of the facts, and only empirical means have been used to overcome them. The experiments which I have undertaken on this subject with the use of rapid collodion for the reproduction of microscopic objects, have led me to discover that the salts of silver applied on glass frequently undergo a slight reduction even in laboratories which are the most carefully preserved from light. They then give in the camera only weak, misty and unequal proofs, incapable of furnishing a good positive. The evident cause of these partial reductions is the frequently fortuitous presence of very minute quantities of free, phosphuretted, sulphuretted, or carburetted hydrogen, that of the vapors of any essential oil, in a word, of any body which easily abandons its hydrogen.

Many of these bodies reduce, even in complete darkness, and at the ordinary temperature, the bromides, chlorides, and iodides of silver in the nascent state. Their action is not absolutely the same as that of light. It only acts, so to speak, on the surface. The lower part is apparently protected, remains intact, and may still give a feeble proof in the camera; but this image being insufficient, we cannot too carefully avoid the agents which have so long been the cause of so many mishaps.

In a laboratory freshly painted with oil and essence of turpentine, in the neighborhood of sulphurous springs, of decomposing organic substances, and wherever hydrogen is easily set at liberty, these phenomena are invariably produced. It is sometimes manifested with so much violence, that in the capsules into which is poured the solution of gallic acid with the addition of some thousandths of nitrate of silver, to develope the image, the metal is instantly reduced, whereas in the ordinary circumstances, at the boiling heat, the reduction does not take place under a quarter of an hour.

The effects of which I speak can be produced at will by dropping a little essence of turpentine or of lavender about the laboratory, or even by leaving open a flask of sulphuretted hydrogen. In submitting to the Academy the results of my experiments on this subject, it is quite unnecessary for me to add, for every one will know it, that it is suffi-

cient to disengage, in the place in which we operate, a little chlorine gas, to cause the disappearance of these spontaneous reductions, and thus to avoid the inconveniences which result from them.

Comptes Rendus, No. 4, July 25th, 1853.

PHYSICS.

Observations on CHARCOAL *and on the* DIFFERENCE *of* TEMPERATURE *of the* LUMINOUS POLES *of* INDUCTION.

By M. DESPRETZ.

THE results of the experiments which we had the honor of presenting to the Academy some years ago on the fusion and volatilisation of bodies, and, in particular of charcoal, shew that it is neither by fusion nor by the sudden volatilisation of charcoal, that we may hope to obtain this body crystallised.

We proved, at that time, that pure fused charcoal, and the fused diamond are only amorphous graphite; that charcoal suddenly volatilised on the sides of a flask is only a black dust without crystalline appearance.

These experiments on the fusion and volatilisation of charcoal are now repeated every year in the chemical and physical lectures at the Sorbonne. Many crystallised bodies are obtained by the fire of the battery. I shall return, I hope, to this point. The same result would, probably, be arrived at for charcoal, if we had crucibles less fusible than this substance, which we have not. I have, therefore, had recourse to other processes. The process which succeeded best is founded on the slow volatilisation produced in the induced current.

I took a flask with two tubulures arranged like the electric egg; to the inferior stem I attached a cylinder of pure charcoal several millimetres in length, and one centimetre in diameter; I fixed to the superior stem a dozen fine platinum wires. I made the vacuum in the flask, and then the distance of the wires from the charcoal being from five to six centimetres, I passed in the induced current from the apparatus constructed by M. Ruhmkorff, and which has been mentioned in various notes presented to the Academy. The arc was reddish from the charcoal to a short distance from the platinum; the part which enveloped the extremity of the platinum wires was of a violet blue.

The apparatus was always kept thus arranged. We put the bundle of platinum wire above, in order that the small splinters of charcoal might not be confounded with the crystals which might be formed.

The battery was composed of four Daniell's elements, united two and two.

The experiment continued for more than a month without interruption, with the exception of the time necessary for recharging the battery. A thin black layer of charcoal was deposited on the wires. This layer, observed with the microscope, presented nothing very dis-

tinct; with the compound microscope, magnifying about thirty times, it presented several very interesting points.

I have seen on these wires, and especially at the extremities, parts separated from each other, and which appeared to belong to octohedra; I have likewise seen on the black layer, and not on the extremities, some small octohedra resting on a summit.

I have examined these wires several times, and have always seen the same things.

A skilful and experienced crystallographer has likewise recognised the black truncated octohedra of the extremities, and the small white octohedra resting on a summit.

I had not previously communicated to my colleague, M. Delafosse, what I had observed.

I substituted for the wires a polished platinum plate of 1½ centimetre in diameter; although this experiment continued in action for nearly six weeks, no crystals were deposited on the plate. The plate was covered for half of its surface with almost circular curves, of longer radius than the plate; each of these curves was tinged with one of the colors of the thin layers. Here and there were seen small stains of a whitish grey, which appeared to be the result of the momentary adherence of isolated deposits.

In another experiment, I fixed a cylinder of pure charcoal to the positive pole of a feeble Daniell's battery, and to the other pole a platinum wire. I immersed the two poles in slightly acidulated water. This experiment lasted more than two months; the wire of the negative pole was covered with a black layer.

The microscopical examination shewed nothing in this layer.

I solicited M. Gaudin, who is known to the Academy by various researches, to test both products on hard stones.

He proved in my presence that the small quantity of matter enveloping one of the twelve platinum wires, mixed with a little oil, sufficed for polishing in a very short time several rubies.

The black powder deposited by the humid body, although in much larger quantity, required more time for giving the same polish.

It is known that *the diamond* is the only body which polishes rubies; thus M. Gaudin regarded both matters as diamond powder.

I have made a great number of experiments during the last two years; I mention the two which have presented the most interesting results.

On the Differences *in* Temperature *of the two* Luminous Poles *of the induced current.*

I wished to know whether the two luminous poles of the induced current are of the same or of different temperatures; for this purpose, I placed the reservoir of a delicate mercury thermometer near the upper ball of the electrical egg. When the vacuum was made, and the luminous arc produced, the thermometer enveloped with violet light, rose; the current was reversed, and the thermometer enveloped with red

light, fell; the direction of the current was thus changed a great many times, and the result was always the same.

The elevation of temperature, when the reservoir was struck with the violet light, was about 5° 4′F.; with other arrangements the difference would probably have varied, but always in the same direction.

When we attach to each extremity of the induced wire a fine iron wire, and bring the two ends into the air, we see one extremity redden, and very soon present a small fused ball; this portion of the induced wire, at the extremity of which the ball is formed, is that which is covered with violet light in the electric egg.

This is not an effect of transport; for, if we replace the iron wire whose extremity is rounded and enlarged by a platinum wire sufficiently strong not to fuse, the latter does not grow larger at the extremity, and receives nothing material in the short duration of an experiment.

These facts lead to the same conclusion, namely, that the violet pole in the induced current is hotter than the red pole.

I am well aware that this subject requires developments which I do not now give to it. I shall do so on another occasion.

Comptes Rendus, *No.* 10, Sept. 5th, 1853.

TOXICOLOGY.

On the Toxicology *of* Bichromate *of* Potassa.

By M. Jaillard.

To many pharmaceutists and medical men this subject may probably appear of but little scientific value, because they may think that bichromate of potassa is a poison of small energy, a medicament but little used, a substance little known, and a manufacturing product but seldom formed.

We find in the "*Archives générales de Médecine*" (1834), a memoir by Ducatel, in which he tells us that a workman having attempted to poison a comrade with bichromate of potassa, was acquitted in consequence of the report of the scientific men, who pronounced that this salt had no poisonous properties. This error still continues among us; and, with the exception of a few individuals, the medical men and pharmaceutists of the present day continue of the same opinion as those mentioned by Ducatel.

To remove this opinion, and to arouse the attention of scientific men and of the authorities, we have determined on making this investigation, and thus to prove that this salt, which is daily employed by dyers and paper-stainers, very often used in medicine as a substitute for mercurial salts in the secondary stage of syphilis, and sold without hesitation to all comers by colormen, druggists, and even pharmaceutists, possesses, even in very small doses, a poisonous action, which has hitherto been but little suspected.

The study of this poison has been consequently neglected; in works

on toxicology we find it but slightly mentioned. M. Orfila himself gives only a few lines to its action on the animal economy, and even those are merely a quotation of what was published by Ducatel in the "*Archives*" of 1834. Nothing is given anywhere on the symptoms of the poisoning, the anatomical lesions, the means of discovering the poison either before or after death. The toxicological history of bichromate of potassa was still to be made; we have undertaken the sketch, and shall be satisfied if we can be, if only of little use to science —of some use to humanity.

In therapeutics, bichromate of potassa is known in external use as a resolutive, and as a caustic when in concentrated solution. Cumming, in the "*Edinburgh Journal*" (1827), says that he has used it with success for warts and syphilitic growths. Internally it is administered as an emetic in the dose of 0·04 to 0·05 gr.

It may be used in diseases of the chest, and in certain spasmodic affections. Lately it has been given in the place of mercurial salts, for the secondary symptoms of syphilis.

This antisyphilitic property was first mentioned by M. Robin, in the "*Gazette des Hôpitaux*" of the 8th November, 1850; he there explains the action of this medicament on the venereal virus. It acts, he says, exactly like the mercurial salts, combining with the virus, and forming with it an inert compound, but it has a great advantage over them in not producing salivation and the other mischievous consequences of mercurial medication.

Its principal employment is in the manufactory of printed fabrics at Rouen, Mulhausen, and Roubaix; dyers use it to obtain colors by double decomposition, and it has of late been employed as an oxidiser in the preparation of certain vegetable colors. Finally, a great quantity is used in the manufacture of chrome yellow (chromate of lead), which is very generally in use in painting.

To determine the dose necessary to cause death, M. Jaillard tried many experiments on animals, preferably on dogs and rabbits, varying the circumstances, causing the product to act sometimes in the stomach, sometimes in the sub-cutaneous cellular tissue, and sometimes in the circulating current.

From these experiments M. Jaillard concludes that 0·25 gr. of bichromate of potassa, introduced into the stomach of a moderate sized dog, or deposited in its cellular tissue, or injected into one of its veins, will produce death in from two to six days.

The symptoms manifested after the administration of bichromate of potassa are the following:—

This salt, administered in small doses, for instance in the dose of 0·05 or 0·10 gr., is an immediate irritant to the alimentary canal. It produces vomiting, sometimes diarrhœa, loss of appetite, and diminishes the rapidity of the circulation. This last phenomenon was always observed in the individuals subjected to the above-mentioned treatment by M. Robin. When the dose is larger, all the phenomena of severe gastritis are manifested; great thirst and spontaneous vomiting accompanied with great retching, the introduction into the stomach of

even the softest drink sufficing to produce it. The matters vomited consist of mucous, bilious matters, yellowish, and sometimes sanguineous. At the same time the extremities become cold, there is dyspnœa, great anxiety, loss of appetite, numerous vomitings, then the respiration becomes stertorous, and the patient sinks into death in the most complete state of prostration.

There have been attributed to it inflammation of the conjunctiva, the formation in the bronchial system of a coagulated mucus colored with blood, exanthematous affections, convulsions and paralysis of the limbs,—phenomena which M. Jaillard has not observed.

It may be observed that bichromate of potassa always has an action on the alimentary canal, causes softening of the mucous membrane, produces redness, ecchymoses, and sometimes ulceration followed by partial gangrene on the gastro-intestinal walls. The lungs are often the seat of mischief; sometimes they exhibit splenisation and hepatisation in some parts. The blood appears to have undergone some modifications; it is black, diffluent, and but sparingly coagulated. In the nervous cerebro-rachidian system, no lesions have been found; on one occasion a slight injection of the pia-mater could be distinguished.

Bichromate of potassa is essentially an emetic, and for this reason it is an antidote for itself.

But it may happen that there are no vomitings; what then must the medical man do? Should he endeavor by every means to cause the expulsion of the matters contained in the stomach? M. Jaillard does not think so; for the bichromate of potassa introduced into the stomach, acting as a caustic from its excess of acid, should be at once neutralised, and consequently the first step should consist in the administration of alkaline salts, so as to transform this acid salt into a neutral chromate. Then the patient should be induced to vomit, by tickling the uvula, or throat, or by administering large quantities of oil or tepid water.

It may be objected that the neutral chromate of potassa is in itself a poison; the numerous experiments which Gmelin published in the "*Journal de Chimie Médicale*" (1825), on the poisonous action of this salt, permit no doubt on this point; but it results also from these same experiments that the yellow chromate has a much less energetic action than the red chromate, for 0·30 gr., 0·60 and 1·50 of the first salt might be given to a dog without any consequences but retching and vomiting. Twelve grains administered to a rabbit produced no effect.

On comparing these results with those observed by M. Jaillard, it becomes evident that the bichromate of potassa is a much more powerful poison than the neutral chromate, whence the direction given above to neutralise the chromic acid in excess by an alkali or alkaline carbonate.

Bicarbonate of soda must, therefore, be prescribed, and then vomiting should be produced by the means which have been already mentioned.

In cases where bicarbonate of soda is not at hand, magnesia or even a solution of soap, may be administered.

Study of the processes to be employed to prove its presence either before or after death.

First Case :—The individual is living, we can act on the remains of the poison.

If the substance examined in the solid state is of an orange yellow color ; if, by putting a small quantity on a piece of boracic acid and exposing it to the flame of the blow-pipe, a green color is obtained ; if, dissolved in distilled water and treated by the salts of barium, a straw colored precipitate is produced ; by the salts of bismuth, a canary yellow precipitate ; by those of lead, a yellow precipitate ; by the protosalts of mercury, a brick red precipitate ; by the salts of silver, a precipitate the color of venous blood ; if, finally, dissolved in the presence of an alcoholised mineral acid, and treated by the alkalies or alkaline carbonates, a dirty green precipitate is obtained, we may with certainty conclude that bichromate of potassa is present.

It is supposed that the substance was unmixed ; it is possible that it may be mixed with mineral acids or with organic matters.

In the first case, it would be easy to determine the presence of chrome by following the ordinary processes of qualitative analysis ; it would take too long to pass in review the manual operations which would lead to a positive result under all possible circumstances.

In the second case the poison must be discovered by following the process which we shall give in the fourth case.

Second Case :—The individual is living ; all the poison has been swallowed ; the matters vomited can be acted upon.

Of all poisonous substances none colors the matters vomited in so striking a manner as bichromate of potassa.

Nevertheless, the characters furnished by the coloration should not be considered as of the first value in recognising the presence of this salt, for simple vomitings of bile might have the same aspect, and thus cause error unless we are on our guard.

If the matters vomited are both solid and liquid they must be strained through a fine sieve, and the filtered portion must be treated as follows : it is only the liquid portion which must be thus treated.

It must be treated first by the mineral salts which give such very distinct precipitates.

But when the quantity of bichromate is small, the reactions are hidden, and this direct means only gives uncertain results.

We must then seek only for the chrome, which is easily done by the process which we shall give in the fourth case.

Third Case :—The individual is living ; all the poison has been swallowed and the matters vomited cannot be procured.

Chemistry can in no way aid in this difficult and embarrassing case.

Fourth Case :—The individual is dead.

All the matters found in the digestive tube are carefully removed ; the physical characters are observed ; their action on colored reagents is determined ; then they are filtered through a fine sieve ; the solid part left on the cloth is washed, and the liquid obtained is tested with

the salts of lead, bismuth, silver and mercury; it is then evaporated to dryness; the residue is mixed with an excess of nitrate of potassa; the mixture is thrown in separate portions into a platinum crucible heated to redness; the organic substance is likewise burnt, and a mass of inorganic salts remains.

The product thus obtained is taken up again with distilled water, which holds in solution alkaline salts, carbonate, nitrite, nitrate, chromate, and sulphate. By adding alcoholised hydrochloric acid, by degrees, so as to avoid a too considerable disengagement of carbonic acid and nitrous acid, and by operating at a temperature of 122 to 140° F., this liquor, which at first was yellowish from the presence of chromate of potassa, becomes of a light green color; if treated with ammonia, a dirty green precipitate is produced, the formation of which will be assisted by boiling.

We have now to ascertain whether this precipitate is really the oxide of chrome of which we are in search. For this purpose it is thrown on a filter, carefully washed and dried in a stove, then it is tried with the blow-pipe on fused boracic acid, which it likewise colors green; then it is dissolved in hydrochloric acid; a solution of sesquichloride of chrome is thus obtained, which, when treated with the alkalies and the alkaline carbonates, gives a dirty green precipitate (Cr^2O^3), soluble in an excess of reagent; with phosphate of soda, a green precipitate soluble in an excess of reagent; with hydrosulphate of ammonia, a precipitate of hydrate of sesquioxide of chrome.

Conclusions—If the matters present a yellow coloration; if, after filtration, the liquid obtained gives a yellow precipitate with the salts of lead and bismuth, red with the salts of silver, brick red with the protosalts of mercury; if, finally, the dirty green precipitate obtained with ammonia gives a green pearl with boracic acid, and if dissolved in hydrochloric acid it causes the same reactions as the salts of sesquioxide of chrome; the certain conclusion is, that among the vomited matters there existed a salt of chromic acid.

If it is desired to find the poison in the viscera, in the liver for instance, it must be cut into thin slices; it is dried and mixed with an excess of nitrate of potassa; the organic matter must be burnt as in the preceding case, the saline residue must be taken up by distilled water, and the whole operation must be exactly the same as has been described for the vomited matters.

By following this process, M. Jaillard has been able, in the vomited matters of the first and second case, to prove the presence of a salt of chromic acid; and, in the liver of the dog of the seventh experiment, to discover traces of chrome.

M. Jaillard has procured from M. Ricord, at the *Hôpital du Midi*, urine from venereal patients under treatment with bichromate of potassa; and, in all the experiments which he made, by following this process he made manifest the presence of chrome, even when the patients were only taking 0·02 gr. to 0·05 gr. of bichromate of potassa per day, and he only operated on 800 gr. of urine, about half of that which was passed in the twenty-hours.

M. Jaillard, therefore, concludes that the bichromate of potassa being a very energetic poison, producing the death of a moderate sized dog in the dose of 0·25 gr. in from two to six days, it should not be sold to all comers without any caution, but that the sale should be under the same restrictions as that of the arsenical and mercurial salts, which is far from being the case, since very considerable quantities of this poison may be procured without any difficulty from colormen, druggists, and even from pharmaceutists.

Journal de Pharmacie et de Chimie, July, 1853.

NOTES AND QUERIES.

Mineralogical Platinum Forceps. — The platinum forceps in general use for blowpipe examinations are made with *two* pins (*a a* fig. 1), by which the points are separated; this entails the employment of both hands, or awkward manipulation with one hand, when a splinter is to be taken between them. The forceps which I use I have had made with one pin only (*a* fig. 2), with this simple construction, by pressing the pin with the tip of the forefinger whilst the forceps are grasped firmly between the fingers and thumb of the same hand, the manipulation is considerably facilitated. The longer steel blades of these forceps may be kept in contact by means of a screw and nut (*b* fig 2), fig. 3 shews the shape and size of the blades.

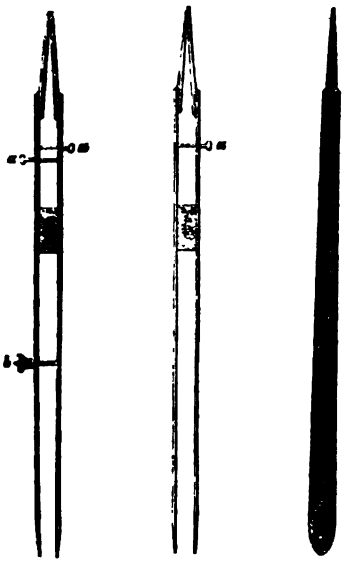


Fig. 1. Fig. 2. Fig. 3.

SAMUEL HIGHLEY, JUN.,
FLEET STREET.

Being desirous of carrying out some experiments on the effects of colored rays, I shall feel much obliged to any of your readers who will furnish me with formulæ for making colored fires of pure red, yellow, green, and blue tints. The works on chemistry and pyrotechny, with which I am acquainted, do not give formulæ for obtaining these colors of sufficient purity, J. W. M., *Debenham.*

It is our object, in giving a portion of our space to Notes and Queries on all subjects connected with the sciences to which THE CHEMIST is devoted, to lead all who may desire information on particular points, to lay their wants before the numerous scientific men who constitute the readers of this work, and to elicit from the latter replies to the same, and we cannot too earnestly urge on the former to avail them-

selves of the inestimably valuable opportunity thus afforded them, and on the latter to come forward, willingly, in the interest of science, to impart the knowledge sought. We shall give every facility and all the space requisite, to both. We shall insert very frequently questions which may appear somewhat trivial, in the hope that by so doing all the practical knowledge on each subject may be brought to light. Let those who are able to answer any particular question remember, that they who now generously give to others out of the treasury of their knowledge, will, in their turn, and in their need, not only receive it, but have earned it.

We shall give in this department occasionally such editorial notices as do not come within any of the special departments of the work.

J. B. wishes to be informed how to make a storm-glass, such as are seen in opticians' windows.

The Irish Amelioration Society.

We have received from Mr. Jasper W. Rogers, who is well known as the inventor and patentee of a process for preparing Peat Charcoal, a reply to the following query contained in our last No., "What were the points in which the Irish Amelioration Society's operations for the Manufacture of Peat Charcoal have failed?" We are glad to have a reply from one who must be considered as fully entitled to be listened to on the subject. We have found ourselves compelled to take from Mr. Rogers' communication, only that portion which relates strictly to the success of the process, and to the merits of the peat charcoal. In the remainder of his letter, which we are precluded from giving, partly from the want of space, and partly from the circumstance that it relates very much to the position of matters between the Society and himself—and shews that he has been by no means fairly treated—he charges the failure of the Society on its managers, and we must observe, that some of his statements are borne out by documents which we have seen. Certainly, until the contrary shall have been proved, the public is warranted in believing that Mr. Rogers' process was as successful as he represents: we shall be happy to entertain any reply the Society may think proper to make.

Those of our readers who are not directly interested in this subject, must bear in mind that it is one of immense importance to a great number of their countrymen, not only in Ireland, but in this portion of the United Kingdom. Here, as in Ireland, there are immense tracts of peat-bog, which are totally worthless except for the production of that valuable commodity, peat-charcoal, and it is well known that this substance forms with fecal matter, a most excellent and economical manure. But for the unhappy disputes and litigation which have paralysed the operations of the Irish Amelioration Society, we have no doubt but that the sister country would have been very considerably relieved of the maintenance of a vast number of starving

peasantry, who would have found employment under the Society, at moderate, but sufficient, wages. Sincerely do we wish that all these differences could be amicably adjusted; that a spirit of conciliation would put to flight the demon of discord and litigation; and that all parties could be brought to unite in carrying out the excellent objects for which Her Majesty's Royal Charter was granted to the Irish Amelioration Society. We fear, however, that there is no hope of our desire being realized

The following is the principal portion of Mr. Rogers' communication:

To the Editors of THE CHEMIST.

GENTLEMEN,

Observing the above query in your publication of this month, it becomes my duty as the patentee and founder of the Society to reply.

Permit me to say, that neither in theory, as regards the properties of peat charcoal, nor in practise, as to its manufacture, has there been *any failure*. On the contrary, that which I published in my writings and lectures, commencing in 1845–6, as to the then discredited chemical and mechanical powers of that most singularly valuable product, was much within the reality. I knew that even one-half of the truth was then too much for the public mind—and that I should, in any event, raise a host of opponents, in those who had not made the discoveries which had fallen to my lot; especially, those who assumed to themselves "all knowledge" upon the productions of my unhappy country, Ireland! They had totally passed over the chemical properties of peat charcoal—its powers of instantaneously and entirely deodorizing night soil, and of converting it into an almost dry and perfectly portable manure—also the fact, that it was itself a most valuable fertiliser, and as I now deliberately aver, the only means by which the millions of acres of peat moss in Ireland can be economically converted into arable land. I knew, in addition, that the publications upon the resources of Ireland had laid it down dogmatically, that "all plants depend upon the atmosphere for the carbon they contain,"—which I have proved to be an utter fallacy,—and that whilst there were above 3,000,000 of acres of valuable land covered with a material, some forty feet in depth, capable of producing a fuel for all manufacturing purposes infinitely superior to coal, and admirably adapted to the development of the mineral riches of Ireland,—that it was sought to be demonstrated that coal taken from the bowels of the earth in England, could be more cheaply and advantageously used in Ireland, than a better fuel, resting upon the surface of the soil there, and *kissing*, it may be said, the minerals and fluxes outcropping on every side. I set forth facts to shew that these were fallacies, but they were sneered at by the *savans*. I have lived, however, to see the following admitted:—

1st. That plants do not solely depend upon the atmosphere for the carbon they contain, but, on the contrary, that they luxuriate abundantly when peat charcoal is given to their roots.

And yet, will it be believed, that I was refused a gold medal by a

scientific body for an essay written upon the disease of the potato (although offered a silver one, which I refused), because, as I have been informed, I put forth in that essay "the fallacious doctrine" that carbon *could* be assimilated by the roots of plants; and that the disease of the potato would be checked by planting that root in powdered peat charcoal; a practise since adopted by many, and proved to be eminently effective. But the following extract sets the question at rest as to carbon being assimilated by the roots of plants;

"Public attention has been directed within the last year or two to *peat charcoal* as a Fertiliser; and during the present autumn (1849) the efficiency of the substance has been publicly tested in London. As there are abundant sources of it in England, Scotland, and Ireland, we are sanguine that this hitherto unprofitable substance will be the means of conferring important advantages upon agriculture, and upon the country at large. To the zealous labors of Mr. JASPER W. ROGERS, we are indebted for publicly propounding this subject, and for carrying it out to the test of public experiment. Dr. A. BUCHNER, senior, writing in the 'Garten Zeitung,' makes pointed reference to these 'numerous experiments and observations' pronounces them to be 'very important contributions not only to vegetable physiology and dietetics, but also to the founding of a vegetable therapeutic system,' and makes a very clear scientific statement, though necessarily but a theoretic one, of the manner in which he supposes the charcoal to produce its beneficial effects.

"LIEBIG, though not offering any lengthened explanation, gives his powerful testimony in the same direction as Buchner, and says, 'Charcoal, in a state of powder, must be considered as a very powerful means of promoting the growth of plants on heavy soils, and particularly on such as consist of argillaceous earth. The free access of the carbonic-acid gas of hot atmospheres to the plants promotes their growth, increases their vigor, and enriches their secretions. The application of the same gas to their roots, although it has not been examined with the same care as its action upon their leaves, is yet evidently attended with the highest advantages."—*Rural Cyclopædia.*

2nd. It has been proved that manure composed of proper proportions of peat charcoal and night soil, is infinitely more lasting than guano,—and at least its equal in producing power—whilst even one-tenth part of charcoal, in place of two parts, to one of night soil, as I originally promulgated, will effect thorough deodorization, and destroy all unhealthful exhalation, if the charcoal be properly made and used.

In my first three lectures on this latter fact at the "Mechanics' Institution," London, it was so much doubted, that judges were demanded by the audience, to examine the intermixing machine, because it was stated by many present that it was a mechanical contrivance which separated the night soil, and concealed it in some "secret drawer," allowing the peat charcoal to escape. The machine was accordingly opened, and the weight of the single product ascertained to be the same as the whole that went into the "hopper:" until then the fact which I have stated was entirely disbelieved. A similar examination was demanded at the Royal Dublin Society *and made.* Perhaps you will allow me to introduce here an extract from the "*Mark Lane Express,*" with the *London Judges'* certificate as to facts.

"On Monday a very unusual meeting of a highly interesting character took place at the Mechanics' Institute. There were present an assemblage of above 600 persons composed of many of the leading medical and scientific characters in London, and several foreigners of distinction. The primary object of the meeting was to test, by actual experiment, the deodorizing properties of peat charcoal when applied to, and intermixed with, night soil; the operation commenced, and in a few minutes the whole quantity intermixed was carried off by the audience, who came from all parts of the theatre to take it, in *handfuls*—a few minutes before all noses were turned away from the buckets in which the night soil was brought; a few minutes after it was taken up in *handfuls* and put into paper bags and stowed away, possibly in the same pocket with the pocket-handkerchief.

"The following certificate was then signed by the chairman and judges:—

"At a meeting held on the evening of the 1st October—we, the undersigned, having been requested by the assembly to act as judges of a fact propounded by Mr. JASPER ROGERS, concerning the chemical properties of peat charcoal, do unanimously decide as follows:—That the deodorizing power of peat charcoal has been fully proved upon human excrement.

"(Signed):—A. KING, M.D., M.R.C.S., 24, Lower Calthorpe Street; J. LYON, Secretary of King's College Hospital; SAMUEL GRIFFITH, M.R.C.S., 1, Bloomsbury-place; D. WALKER, M.A., F.G.S., F.S.S., Head Master, Agricultural College, Kent; J. C. NESBIT, F.G.S., F.C.S., 38, Kennington-lane, London; G. GARRETT, Commissioner of Paving, 130, Jermyn-street; W. O. YOUNG, Sun-court, Cornhill."—*Mark Lane Express.*

3rd. I have proved by the operations of the Irish Amelioration Society, at Derrymullen, in the county Kildare, that in place of an average produce of 25 per cent. of charcoal from the turf of commerce of Ireland (the calculation set forth in my publications and the prospectus of the Society), the actual working produce was about 34 per cent.! or exceeding one ton of pure charcoal to three tons of the peat prepared by the peasantry of the country, and dried by the atmosphere only. It was also proved that the actual cost of this charcoal, delivered to the Store in Dublin, was under 25*s.* per ton; whilst the demand there at 35*s.* per ton was so great, that the supply was perfectly inadequate.

These are realities which the minutes and books of the Society demonstrate. Permit me to add the interesting fact, that within the period of one year, which, only, was occupied in the drainage of the bog, and the construction of buildings and machinery capable of being made to manufacture about 300 tons of peat charcoal per week—the poor-rate of "the Electorate" in which the works were situate was reduced from 6*s.* 9*d.* in the pound to *nothing*. About 1,100 persons were taken from the poorhouse, in consequence of the employment afforded; and although the rate of wages (*sixpence per day*), upon which the laborers of the country had barely upheld existence, had been raised to *a shilling* (giving, perhaps, only enough to produce physical capability), the Society was then making a profit of 10*s.* upon each ton of charcoal.

I am, Gentlemen,

Your obedient servant,

JASPER W. ROGERS.

QUARTERLY METEOROLOGICAL TABLE FOR DIFFERENT PARALLELS OF LATITUDE.

PARALLELS OF LATITUDE, &c.	Mean temperature of the Air.	Mean of the Highest Readings of the Thermometer.	Mean of the Lowest Readings of the Thermometer.	Average Daily range of Temperature.	Average Monthly range of Temperature.	Average Quarterly range of Temperature.	Mean temperature of Evaporation.	Mean temperature of the Dew Point.	Mean amount of Cloud.	Rain.		Mean weight of Vapour in a cubic foot of air.	Mean additional weight required to saturate a cubic foot of air.	Mean degree of Humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Mean height above the Sea Level.
										Average number of Days.	Average fall.						
	°	°	°	°	°	°	°	°			in.	gr.	gr.		in.	gr.	feet
In the Counties of Cornwall and Devonshire	53·0	77·2	33·6	14·8	34·0	43·6	49·6	46·1	5·2	43	7·5	3·8	1·0	0·790	4·5	534	132
Newport and Ryde	52·2	79·0	30·6	18·8	38·0	48·4	48·7	45·2	6·0	43	8·0	3·7	1·0	0·793	4·4	536	69
South of latitude 51°	51·7	75·4	34·1	13·3	31·8	41·3	48·7	44·9	5·7	42	7·7	3·7	0·9	0·806	4·4	536	43
Between the latitudes 51° and 52°	51·7	79·4	30·5	18·5	39·2	48·9	48·6	44·9	6·6	44	8·1	3·6	0·9	0·793	4·4	532	213
Between the latitudes 52° and 53°	51·3	79·6	30·5	17·6	39·9	49·1	48·9	44·4	5·6	43	6·5	3·6	0·9	0·799	4·3	535	140
Between the latitudes 53° and 54°	50·7	77·1	29·9	17·4	35·6	47·2	47·2	43·3	6·1	40	6·9	3·4	1·0	0·779	4.2	535	142
Liverpool	52.4	72·6	35·2	11·5	26·4	37·4	49·2	45·8	6·5	38	5·7	3·8	0·9	0·805	4·5	535	37
Newcastle and North Shields	48·5	68·5	32·8	10·1	25·9	35·7	46·6	44·6	5·0	44	5·1	3·6	0·5	0·883	4·3	538	123
Dunino and Arbroath	49·2	74·0	29·5	17·0	33·5	44·5	45·0	40·3	5·5	32	3·8	3·1	1·1	0·739	3·7	534	150

In the formation of this Table the results from Jersey and Guernsey have not been combined, on account of the difference between the ranges of temperature of the two places. The results from Ventnor are not combined on account of the much higher temperature, and less range of temperature than those at the other stations in the Isle of Wight.

Extracted from the Register General's Return from the Spring Quarter, 1853.

THE TORBANEHILL MINERAL CASE.

"*Audi alteram partem*" is a good old adage, which requires, probably, more consideration in these railway days than at the period of its first enunciation; for so completely has the world entered into a running match with time, that he is accounted slow who waits for the development of a truth before he records it, or who listens to the whole of a story ere he gives judgement upon its merits. In the time of Horace it was enough, "*carpere diem*," but now, a journalist at least must do something more than take the old scythe bearer "by the forelock," he must lead him by a few furlongs of "chain cable." In some remarks contained in our last number upon the merits of the celebrated Boghead Coal case, we had taken for granted that much, if not all, of the mere facts detailed by the Dean of Faculty on the part of the pursuers was substantially true, and as we had then no other report than that furnished by the pursuers, we transcribed a part of the address of the Dean of Faculty, which went to shew that the Messrs. Russell had discovered the Boghead Coal on the lands of Torbanehill, but had kept that knowledge secret whilst they negociated with Mr. Gillespie for a lease; this is, however, very wide of the truth, and it gives us great pleasure to be in a condition to correct so unjust an imputation. The Dean of Faculty wished the jury to believe that Messrs. Russell had furnished only an imperfect, or rather, we should say, a mutilated, copy of their Boring Journal, whereas we now find that so early as June, 1850, Messrs. Russell wrote to Mr. Gillespie, "We have now pretty fully explored your mineral field, and as far as we can see, the only seam we would be justified in trying to work for a profit is the *thin parrot coal*, ten to twelve inches in thickness, which WE ARE WORKING at Boghead, and which we *have also found* in the pit at Torbanehill, though very troubled in some, and altogether wanting in other parts of the mine which we have run in." A letter to the same purport was also written, it appears, in August, 1850; and the copy Journal of the Bores furnished to Gillespie were not at all of the imperfect character insinuated, and, indeed, asserted by the Dean of Faculty; nor was Mr. Gillespie, unprovided, as it is now proved, with a competent mining agent to see justice done him; on the contrary, his interests were protected by Mr. Geddes, a gentleman of great experience, talent, and integrity. Upon the whole, therefore, it gives us much pleasure thus again to open out this matter, in order that the Messrs. Russell may be relieved from our previous condemnation—a condemnation, be it observed, based upon published and heretofore uncontradicted statements, and which we now repudiate. Perhaps the following brief extract upon this subject from the speech of the Lord President will form the most fitting conclusion to these remarks:—

"There is another part of the case on which a great deal has been said, and on which I do not mean to say much, for it merged into a sort of personal question, about the conduct and motives of the two gentlemen who are litigating here. I do *not* see *any* reason to *impute improper motives* either to the one gentleman or the other. It appears to me that they were entitled to look to their own interests in making their bargains, and I don't see that anything has come out to *justify* the imputation of improper motives on either side—or what was *unfair*. In making the bargain, Mr. Russell was entitled to make as good a bargain as he could, and Mr. Gillespie was equally entitled to do the same."—"But as to motives, I see no reason for imputing any on either side. A correspondence has been spoken to, and a discussion seems to have gone on. Undoubtedly at that time the Boghead Coal had begun to be worked, and the article had been going to the market, and was called by Mr. Russell when he sent it to the market, Boghead Coal. It appears that in the Summer of 1850 he was active on this subject, and in trying to push his Coal in the market. In the Summer of that year he went to Cupar to sell it, while engaged in his trials. Mr. Gillespie and he had a good deal of correspondence, in which this Boghead mineral was made matter of reference both as to the terms of lease and as to the seam found, and which was proved to be a part of the same seam found at Boghead, and in September I think of the year, 1850, Mr. Russell and Mr. Gillespie had a meeting at Rothesay, where they went to a gas manager to see what he thought about it, and what was likely to be got for it, and it is very plain that at that time Mr. Gillespie knew that the coal was expected to

be raised on his property, and that it was one of the richest gas coals in Scotland—'in the market'—were the words, I think, used by the witness. There can be no doubt of that at all. It appears plain on the face of the correspondence, that he did not put himself into the hands of Mr. Russell to tell him what he should do. Mr. Gillespie had for advising him his own agent, and his own mining engineer, Mr. Geddes. There was nothing concealed in the borings made by Mr. Russell, because they were made at the sight of Mr. Geddes. There could be nothing wrong, and if Mr. Russell wanted the lease at a less fixed rent than Mr. Gillespie wished, he had a right to do so; and if Mr. Gillespie wanted more he had a right also. It was all quite fair."—"As to motives, I see no reason for imputing any."

Opinion of German Men of Science on the Torbanehill Mineral.

The Torbanehill Mineral has been the subject of dispute in Germany. We have not room for the particulars (which will be found in the *Mining Journal* for Oct. 8); we must, therefore, content ourselves with stating, that the scientific men connected with the Board of Customs at Berlin, have decided that the Boghead and Torbanehill Mineral is *not* coal, but *bituminous shale*, which is also stated to be the general opinion among German chemists.

PROCEEDINGS OF SOCIETIES.

British Association for the Advancement of Science.

Notice of Changes in Wood obtained from the Submerged Forest at Wawne, Holderness.

By T. J. Pearsall.

Considerable masses of wood have at various times been dug up from the district of Holderness. Some peculiar changes were observed to occur in specimens of these deposited in the Philosophical Institution at Hull, amongst others, the escape of a gas of a very pungent odor, and eventually crystals of some hydrocarbon compound, of a large size, were discovered to have been formed.

Mr. Lowe made some remarks on the analogous slow combustion of coal giving rise to a similar strongly acid vapor, and on the formation of crystals of the same general character in other bodies.

Dr. Price instanced the formation of these crystals in cedar. A desultory conversation arose on the probable age of the submerged forest at Wawne, in which it was elicited that although its age could not be determined, yet that the fact of its submergence in the autumn was evident, since the nuts of the beech and the hazel were found with the wood. That the submergence has also taken place since the existence of man in these islands was shewn by the discovery of a copper-plate of an exceedingly good manufacture, and remarkable as having been mended by rivetting in a very careful and particularly neat manner.

Notice of Peculiarly formed Substances called Lime Flowers from the Brickwork of the Reservoirs of the Hull Waterworks before they were completed for Use. By T. J. Pearsall.

The cement upon which the brickwork of the reservoir of the Hull Waterworks was laid being imperfect, and never setting, there were produced upon one side only of the reservoir some remarkable forms. These partook, in many respects, of the conditions of organisation; long stems were formed, and in most of them a bulb was produced, which, opening, resembled a half-opened tulip, or the flower of the crocus. These stems and bulbs varied in length from two or three to fifteen or eighteen inches; they were arranged between the joints of the bricks, and extending for 200 or 300 feet. Analysis proved these lime flowers to be nearly pure

carbonate of lime, and they were doubtless formed by the gradual forcing out of the lime into the water of the reservoir, where it combined with the carbonic acid. The causes which determined these very singular forms were not easily ascertained; but as all the stems were hollow, it is not improbable that air was forced up through the solid substance, and to the same cause may be attributed the flower-like top.

Numerous examples of these curious bodies were exhibited by Mr. Pearsall, and a long discussion ensued, in which Dr. Daubeny, Professor Andrews, Mr. Hopkins, and Dr. Price took part.

On the Formation of Magnesian Limestone.

By J. T. JOHNSTON.

The author produced specimens of magnesian limestone formed by deposition from a spring near the village of Neesham, on the northern bank of the Tees. This limestone possessed the color, general appearance, and porous structure of the limestones of the county of Durham, and contained as much magnesia as some of the purer beds of magnesian limestone in that county. From the production of this limestone he reasoned as to the deposition of dolomitic limestones in general, and the relative probability of the two theories which ascribe their magnesia to the impregnation of previously existing limestones, either by sublimation from beneath or by percolation from above. He considered both agencies inadmissible as general causes, and was favorable to the view that, as a general rule, magnesian limestones were deposited from aqueous solution, though occasional impregnation of previously existing rocks by percolation was by no means unlikely.

On the Corrosion of Iron-built Ships by Sugar Cargoes.

By Dr. GLADSTONE.

The author stated that his attention had been drawn by his brother, Mr. George Gladstone, to the fact, that the owners of iron-built vessels object to sugar cargoes, on account of the rusting of the metal by the saccharine juices which exude from the casks; and this had led to a chemical examination of the reaction then instituted. It was found that when pieces of iron were placed in bottles containing a solution of cane sugar, the metal at the edge of the liquid soon became deeply corroded, but that which was permanently immersed in the fluid remained bright for a considerable time. The solution soon gave indication of the presence of protoxide of iron, which absorbing oxygen from the atmosphere was speedily thrown down as the red sesquioxide, leaving the sugar free to dissolve a fresh quantity of iron, the precipitated oxide in the mean time forming a deposit. After eighteen months, the liquid was of a deep red-brown color; it became pale blue with ferrocyanide of potassium, black with sulphuret of ammonium; alkalies produced no precipitate; nitric acid peroxidised it. A portion dried and analysed gave 20·78 parts of metallic oxide to 100 of combined sugar, which is almost exactly in the proportion expressed by the formula $C^{12}H^{11}O^{11}$, FeO. The author, however, considered that this might differ from the true composition by one equivalent of water. No such iron compound could be formed by direct combination. In vain was it attempted to dissolve any freshly-precipitated and well-washed oxide of iron in a solution of sugar; and almost equally unsuccessful was the attempt to do so when the oxide was liberated by means of potassa in the presence of sugar itself. It was found that under all circumstances of dilution or quality of the sugar solution, iron was attacked; the presence of zinc in contact with the iron did not prevent its being acted upon; nor was there any marked difference when the salts of sea-water, or the nitrates, sulphates, or chlorides of the alkalies were added to the solution. No other ordinary metal was found to be so easily acted upon as iron. Copper was very little affected by the sugar. Lead was slowly attacked, indications of the presence of its oxide in solution being obtained after three days' exposure. Tin appeared to give the binoxide. Zinc was little affected when alone; it seemed to be dissolved more quickly when in contact with iron. It is doubtful whether mercury was touched by the sugar solution; silver certainly was not. The author regretted that his experiments did not suggest any method by which the corrosion

of iron ships by sugar cargoes might be prevented. They shewed rather the strong disposition to combine which exists is between the two substances; and how a small quantity of sugar may eat continuously into a large sheet of iron. The attention of chemists was especially drawn to the fact that the iron enters into combination with the organic matter, not when it has already been oxidised, but only when in a metallic condition, rendering the action, as would be imagined, more complicated.

Description of some New Kinds of Galvanic Batteries. Invented by Mr. KUKLA, of Vienna.

The combination used in one of these, is antimony, or some of its alloys, for a negative plate, with nitric acid of specific gravity 1·4, in contact with it, and unamalgamated zinc, for a positive plate, with a saturated solution of common salt in contact with it. A small quantity of finely powdered per-oxide of manganese is put into the nitric acid, which is said to increase the constancy of the battery. The alloys of antimony which Mr. Kukla has experimented with successfully are the following:—Phosphorus and antimony, chromium and antimony, arsenic and antimony, boron and antimony. These are in the order of their negative character, phosphorus and antimony being the most negative. Antimony itself is less negative than any of these alloys. The alloys are made in the proportions of the atomic weights of the substances. All these arrangements are said by Mr. Kukla to be more powerful than when platinum or carbon is substituted for antimony or its alloys. In this battery a gutta-percha bell-cover is used over the antimony, and resting on a flat ring floating on the top of the zinc solution,—this effectually prevents any odor, and keeps the per-oxide of nitrogen incontact with the nitric acid solution.—When a battery of twenty-four cells was used, Mr. Kukla found that in the third and twenty-first cells pure ammonia in solution was the ultimate result of the action of the battery; but only water in all the others. This experiment was tried repeatedly, and always with the same result.—A battery was put into action for twenty-four hours,—at the end of that time the nitric acid had lost thirteen-twentieths of an ounce of oxygen, and one-quarter of an ounce of zinc was consumed. Now as one-quarter of an ounce of zinc requires only 0·06 of an ounce of oxygen to form oxide of zinc, Mr. Kukla draws the conclusion, that the rest of the oxygen is converted directly into electricity; and this view, he says, is confirmed by the large amount of electricity given out by the battery in proportion to the zinc consumed in a given time. In the above battery each zinc plate had a surface of forty square inches. The addition of per-oxide of manganese does not increase the effect of the battery, but it makes it more lasting:—the per-oxide of nitrogen, formed in the bell-cover, taking one atom of oxygen from the per-oxide of manganese;—this is evident from only the oxide of manganese being found in the battery after a time; in the salt solution no other alteration takes place than what is caused by the oxide of zinc remaining in a partly dissolved state in the solution. For this battery Mr. Kukla much prefers porous cells, or diaphragms of biscuit ware, as less liable to break, and being more homogeneous in their material than any other kind.

This battery is very cheap, antimony being only 5*d.* per lb., wholesale, and the zinc not requiring amalgamation.—The second arrangement tried by Mr. Kukla was antimony and amalgamated zinc with only one exciting solution, viz., concentrated sulphuric acid,—this battery has great heating power, and the former great magnetising power,—it, however, rapidly decreases in power, and is not so practically useful as the double fluid battery, which will exert about the same power for fourteen days, when the poles are only occasionally connected, as in electric telegraphs. Certain peculiarities respecting the ratio of intensity to quantity when a series of cells is used, have been observed, which differ from those remarked in other batteries.—Mr. Kukla, on directing his attention to the best means of making a small portable battery for physiological purposes, has found very small and flat Cruikshank's batteries, excited by weak phosphoric acid (one of glacial phosphoric acid to twenty of water), to be the best; phosphoric acid being very deliquescent, and forming with the zinc, during the galvanic action, an acid phosphate of zinc. A battery of this description does not decrease in power very materially until it has been three hours in action.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On Mildews. By Professor F. Crace-Calvert.

My attention has been at various times drawn to fabrics which had become mildewed after having been kept for a short time, or, more frequently, when they had been shipped to warm and moist climates, as those of South America, or the East Indies, which are so favorable to cryptogamic vegetation.

The production of mildew deserves the serious consideration of manufacturers who are engaged in producing goods for foreign markets, as not only affecting their pecuniary interests, but also as injuring the reputation of their goods in such markets. I am well aware that sometimes the blame does not rest with the manufacturer, bleacher, or finisher, but with the firms from which they receive orders, by whom they are called upon to produce a given quantity of goods from a certain weight of silk, a practice which is highly prejudicial to the ultimate wear of the fabrics. It is a fact that thousands of pieces of silk have, during the autumn and winter, become covered with a white mildew, which, when allowed to remain, stained the fabric permanently.

It is, for example, a common practice for a silk-dyer to receive sixteen ounces of raw silk, which he has to deprive of four ounces of gum before he can dye it, and then he is expected to return from thirty-two to thirty-six ounces of black silk; thus adding twenty-four ounces of stuff to the twelve ounces of genuine silk. And what renders this practice still more blameable is, that a portion of these twenty-four ounces is glue or gelatine, a substance which enters rapidly into decomposition, and mildews. I would ask, how can such silk resist wear and tear? and how can it be expected that so large an amount of such matter will not enter into decomposition when exposed to damp?

It is well known in the trade, that cotton goods are liable to similar deterioration: fustians, for example, are highly charged with bone-size; some of these fustians I have found to contain between sixteen and eighteen per cent. of foreign matter, of which bone-size was the chief ingredient. Here, again, we have an animal matter disposed to ferment, and thus to produce that mildew so common in this class of goods. The pungent odor of the room where the fustian goods are stored, shews that decomposition is already active. I regret to say that this is not the only class of cotton manufactures which contains heterogeneous matters; white calicoes are sometimes highly weighted. I have had some which had become mildewed in going to the West Indies, and which

my analysis proved to contain nine per cent. of mineral, and nine per cent. of vegetable matter, or, to use a more explanatory term, "of sour flour," that is to say wheat flour, which has been placed in large vessels with water, and allowed to ferment, thereby generating the germ of ultimate decay or disease. It is my conviction that this practice should not be followed, and that such stiffening should be rejected, as well as rice flour, and that only starch or farina flour should be used for the purpose, as they contain no nitrogenous substances, which generally facilitate fermentation.

To give an idea of the rapidity with which mildew is capable of being produced, I succeeded lately in generating it in "bandanna" cloth in the space of six or seven days, by simply moistening some pieces, and placing them in an obscure and warm spot. This rapid production was so much the more interesting, as the goods were finished with only ground rice. I have often observed printed calico which had become mildewed, and no doubt the principal cause was, that rice flour, or sour flour, had been used in the finishing processes.

Dr. Henry Brown, of Manchester, who has had the kindness to examine for me some of the mildews under the microscope, states, that "there were several collections of fibres and granules between the crossed threads of the cloth, which exactly resembled the *flocci* and sporules of the fungus he had frequently seen on the surface of mucilage that had been kept for some time."

I believe all these remarks would be of comparatively little value to the trade, if I were not to publish the best materials I have found to prevent or impede the production of mildews, and these I give with perfect confidence, as they have been applied in manufactures and have proved successful. To prevent size made with flour from becoming mildewed, I add about three parts of sulphate of zinc to one hundred parts of flour, and then mix the whole with water, and boil in the usual way

To prevent mildew on fustians which contain bone-size, I add to every gallon of the latter one ounce of arsenious acid previously to the pieces being passed through the size. As to the mildew on silk, a silk manufacturer of Manchester has for the last two or three years cured this disease by a process which I discovered at the time, and, therefore, it cannot be described.

I shall complete these remarks on mildew by stating, that the greatest care should be taken to pack goods as dry as possible, and also that the present general mode of packing is not sufficient to protect them from external injuries, for I have found in several instances in which goods were returned from abroad, that the stains on them did not arise from mildew, but from dirt or coloring matter which had penetrated through the packing, which latter consisted of packing-cloth, oil-cloth, packing-cloth, and two folds of paper; therefore a better mode of protecting goods to be sent abroad should be adopted.

On Ferri Potassio-Tartras. By J. Denham Smith, F.C.S.

In the "Pharmacopœia Londinensis," A.D. 1787–1788, we find, for the first time, directions given by the College of Physicians for preparing

a potassio-tartrate of iron, under the term *ferrum tartarizatum*, a name it retained until the sweeping change of 1836, when its present designation was bestowed upon it.

This preparation, under various names (Jourdan enumerates no less than twenty-seven), has long held a place in the European pharmacopœias; the formulas in all being very similar, and frequently rivalling each other in the impossibility of complying with their directions. All these methods were merely modifications of, or identical with, that of the "Pharm. Lond." 1787–8, which had simplicity, and but little else to recommend it. Thus, when done into English, ran the recipe,—

"Iron filings, by weight, one pound,
Crystals of tartar, powdered, by weight, two pounds.
With distilled water, make them into a thick paste, which expose to air, in a shallow earthen dish, for eight days; then, after drying this paste on a sand-bath, rub it to a very fine powder."

The "London Pharmacopœia," of 1809, orders this powder to be a second time treated with water, again exposed to the air for eight days, and again dried and powdered; yet, even then, most of the iron remained in the metallic state. Phillips, in his "*Experimental Examination*," 1811, describes it as "usually of a light-green color, readily attracted by the magnet, unalterable by exposure to air, and with difficulty soluble in water; that which I obtained from Apothecaries' Hall contained twenty per cent. of metallic iron unchanged by digestion with the tartar." This chemist recommended the employment of soft iron, a lengthened exposure to the air, and subsequent solution of the product to free it from excess of metallic iron, tartrate of lime, and uncombined bitartrate of potassa, advice but partially followed by the College in the work of 1824; so that it was not until 1836, that a good formula was inserted in the London Pharmacopœia, when a modification of a process then recently recommended by Soubeiran and Capitaine was adopted. The present potassio-tartrate of iron differs from that of 1836, by a closer resemblance to that obtained by the French process just alluded to, the ammonia being now dispensed with.

The *ferrum tartarizatum* of the Edinburgh College, resembles the London *ferri potassio-tartras* of 1836, in containing ammonia, which is absent from the present preparations of both the London and the Dublin Colleges, that of the latter differing from our own only in its proportion of iron. The London formula specifies the proportions of the peroxide of iron and bitartrate of potassa to be used, whilst Soubeiran, "*Nouveau Traité de Pharm.*," 1840, directs the indefinite quantity "Hydrate de peroxide de fer humide, Q. S.," so that if there really be any difference between the product of these formulas, which I greatly doubt (since it is only by prolonged digestion, that the peroxide ordered by the London College is dissolved by the bitartrate of potassa), it can only be that Soubeiran's contains a little more oxide of iron than that made by the other formula.

In preparing the salt analysed, the process of the Pharm. Lond. was exactly followed, especial care being taken to thoroughly wash

the precipitated peroxide of iron, which, excepting too small a quantity to be worth noticing, by prolonged digestion dissolved in the bitartrate of potassa. The clear filtered solution when dried on earthenware in thin layers, yielded brittle laminæ precisely resembling the best specimens met with in the shops. *Ferri potassio-tartras* is perfectly transparent when in thin plates, drying in glittering scales of a beautiful deep jacinth-red tint, which when freshly exposed to the air curl up and split in every direction; the mass, when just taken from the drying plates and exposed on a sheet of paper, being seemingly animated, so lively is this motion, which is probably due to the absorption of a little hygroscopic water. It is not deliquescent; on the contrary, it is persistent in the air, and under ordinary circumstances the solution spontaneously dries up; the salt only becoming somewhat damp when in a very moist atmosphere, and not deliquescing even then, if properly prepared. It is neutral to test-paper and very soluble in water, but not so much so as Soubeiran's description would lead us to imagine, since a solution, when tolerably concentrated and cooled, lets fall a voluminous coffee-brown deposit, which readily redissolves by heat. This chemist is also in error in stating alcohol to be a ready solvent for it. Mr. Squire's remark, in his Conspectus of the three Pharmacopœias, is much nearer the truth, since this salt is soluble in weak tinctures only, and insoluble in rectified spirit.

Wackenroder, quoted by Christison, maintains that this salt, even when prepared by Soubeiran's method, contains both oxides of iron; "for, if a concentrated solution be heated with solution of potassa, the precipitate is not the sesquioxide of iron, but the black oxide," a reaction I have never observed, the red-brown hydrated sesquioxide invariably manifesting itself; although this decomposition not being instantaneous on the addition of the alkali and application of heat, the solution deepening rapidly in color, becoming turbid, and not till then does the hydrated oxide separate; a reaction which may have led to Wackenroder's mistake. The composition assigned by Wittstein to this salt, who regards it as containing both the oxides of iron, and indeed the whole account of the potassio-tartrate of iron in the *Dispensatory*, 1848, shew that Dr. Christison, when describing the new *ferrum tartarizatum* of the Edinburgh College, mentally reverted to the old preparation, combining the characteristics and possible constitution of the latter, with the special properties and reactions of the former salt, viz., the ammoniated potassio-tartrate of iron.

That a difference exists between *ferri potassio-tartras* and *ferrum tartarizatum*, when the latter is prepared even by the best of the processes involving the action of bitartrate of potassa on metallic iron, is obvious by a glance at the two preparations; since the latter is of a dark olive-green color, furnishes a solution of a similar shade, and is likely enough to be the salt described and analysed by Wackenroder and Wittstein, containing either a mixture of the protoxide and peroxide of iron, or the magnetic oxide (ferrous ferric oxide), characters inapplicable to the present preparation of either of the three Colleges of the United Kingdom.

To determine the amounts of potassa and peroxide of iron in this salt is easy enough, requiring only the care and precautions imperative in any analysis the object of which is to determine the constitution of a salt; but neither lead nor lime furnishes a re-agent on which I could rely for the estimation of the tartaric acid, the weight of each of their respective tartrates differing far too greatly in repeated experiments to command any reliance on these precipitants. The lime-salt comes nearer to the truth than does lead; yet when a pure transparent bitartrate of potassa was examined for its weight of acid with chloride of calcium, a notable deficiency of tartrate of lime invariably resulted; so that, although both lead and lime yielded results with the potassio-tartrate of iron, indicating an amount of dry tartaric acid varying from 39 to 41 per cent., still being satisfied of the indecisive nature of these determinations, and of their inexactness under the most favorable conditions, I have relied on the analytical determinations of the two bases, as the ground-work of the calculated composition; assuming the potassa to exist as a true neutral tartrate, on the evidence of the perfect neutrality of the potassio-tartrate of iron, as well as the theoretical probabilities in its favor, this involves the necessity of an equivalent quantity of tartaric acid being combined with the sesquioxide of iron existing in the salt, as that existing in combination with the potassa.

100 grains of the potassio-tartrate of iron, above described, and freed from hygroscopic water, were gently ignited at a low red heat, in a current of air. The mass, when cold, was moistened with a solution of carbonate of ammonia, and again ignited at the same temperature; this was repeated, and the residue when ground and boiled in water left, after thorough washing with water just acidulated with nitric acid to remove the last traces of potassa, 33·50 grs. of peroxide of iron.

The solution and washings, slightly supersaturated with sulphuric acid, evaporated to dryness and then ignited until no acid vapors were liberated, yielded 29·50 grs. of sulphate of potassa = 16·10 grs. of potassa. These results, reduced to equivalents, point to the following composition of the *ferri potassio-tartras* of the "London Pharmacopœia":—

	Expmt.				Calculated.
16·10 KO^2 + 22·10 C^4HO^5	= 38·20	or,	2 Eqs.	of Tartrate of Potassa	224 = 37·84
33·50 Fe^2O^3 + 22·10 „	= 55·60	„	1 „	2-5 Tart. of Peroxide of Iron	332 = 56·08
Loss HO . .	= 6·20	„	4 „	Water . .	36 = 6·08
	100·				592 100·

In this view, 2 eqs. of tartaric acid combine with 5 eqs. of peroxide of iron, and this with a double equivalent of neutral tartrate of potassa. the tartaric acid in both compounds retaining its constitutional water, or 4 eqs.

This is a complex constitution, and were it a salt of one of the inorganic acids, it ought to be received with suspicion; but these double organic salts often present, as yet, seemingly strange compositions, and I hold it to be the duty of the chemist to adhere to results indicated by

analysis, when carefully performed, rather than to permit the analysis to be controlled by theoretical considerations, or probabilities. If we assume that this salt is a tristartrate, the acid retaining its constitutional water, its composition would stand :—

1 Eq.	Tartrate of Potassa. .	114	35·85
1 „	Tristartrate of Iron . .	186	58·49
2 „	Water	18	5·66
		318	100·

Call it a ditartrate ; then,—

1 Eq.	Tartrate of Potassa . .	114	41·00
1 „	Ditartrate of Iron . .	146	52·52
2 „	Water	18	6·48
		278	100·

but both these estimates differ too widely from the results of analysis to claim reception, although the former of the two is possibly true.

It may be mentioned, that the weight of salt obtained from the process on a small scale agrees with the composition assigned to it, but I do not lay much stress on this occurrence.

It may be held that the second equivalent of acid in the bitartrate is capable of taking up more peroxide of iron than goes to form a two-fifth tartrate ; this may be so, and it is likely enough that a tristartrate may be formed by following Soubeiran's process with care ; but if so, such a salt would not be the *ferri potassio-tartras* of the "*Pharm. Lond.*" It may also be assumed with respect to this salt that the ferric compound is not a definite one, an objection which in this case cannot be overthrown ; still I submit that the composition assigned to the salt of the Pharmacopœia is consonant to the results of analysis, and that we are justified in assuming its definite constitution under such conditions, albeit that constitution may not accord with our preconceived or theoretical notions on the subject.

NOTE.—A useful translation of Wittstein's "Pharmaceutical Chemistry," by Mr. Darby has just come into my hands, from which I find that the German pharmacist adheres to the opinion quoted by Christison, that the potassio-tartrate of iron, when made from the hydrated peroxide and cream of tartar, contains *protoxide* of iron. I quote the translation : "Cream of tartar being an acid salt, readily takes up the moist hydrated oxide of iron, forming a brownish yellow liquid ; the iron in this solution is partly reduced to the state of protoxide during the reaction by the cream of tartar. From analysis of the compound, it appears that in every 7 at. of peroxide of iron, 1 at. gives off 1 at. or 1-3rd of its oxygen, which causes the formation of 2 at. of protoxide of iron. *The liberated oxygen is entirely given off ;* at least I have been able to discover the separation neither of oxalic, formic, nor carbonic acids, which we should expect to result from the oxidation of tartaric acid. The two atoms of protoxide of iron, and the other 6 at. of peroxide unite with 8 at. of bitartrate of potassa, to form a double or rather a triple salt, consisting of neutral tartrate of potassa, neutral prototartrate of iron, and basic pertartrate of iron." Were it not for the words, " From analysis of the compound," this statement might admit of criticism, and its formula to be classed with the composition deduced by Soubeëran, which does not pretend to be founded on analysis ; as it stands, I must confess my inability to explain it, and am thus forced to confine

myself to simply controverting the statement, and denying the existence of protoxide in the salt, or the liberation of oxygen gas during its preparation in the latitude of London, whatever unique reactions may take place under a Bavarian sky.
The italics are mine. J. D. S.

CHEMICAL TABLES.

COMPILED BY THORNTON J. HERAPATH.

THE practical chemist has long felt the want of a convenient text-book of reference adapted for consultation in the laboratory and manufactory. Having been for some time in possession of such a work in manuscript, I am at last induced to adopt the advice of my friends and pupils by bringing it before the public, or at least those portions of it to which experience leads me to believe it will prove of the greatest service,—to the readers of "THE CHEMIST." In regard to the selection, great care has been taken that the subjects should be of such a nature as business transactions and private research call most frequently into requisition, and at the same time to condense the facts into the smallest possible space compatible with accuracy and facility of comprehension. The tables will consist of:—

1. Table of Densities or Specific Gravities.
2. Table of Thermometric equivalents and of the Effects of Temperature.
3. Table of the Solubility of Organic and Inorganic Substances in various Menstrua.
4. Table shewing the Behavior of Substances with Re-agents, and for testing by the Blow-pipe. Proximate Organic Analysis.
5. Table of the Adulteration of Drugs, Medicines, Chemicals and Articles of Food.
6. Tables for Calculation in Analysis,

with some few others of minor importance.

In compiling the first two tables, I have, of course, been prevented by the limits of this Periodical from inserting every determination which has been made of the density and boiling point, &c., of each substance noticed. Consequently, only those results have been taken which I consider the most trustworthy. In a few instances, where great discrepancies occur, or doubts still exist with regard to the correctness of the results, I have thought it advisable to give the contradictory numbers, attaching a note of interrogation to the one which is generally believed to be the most open to objection.

These tables will be continued from month to month until completed.

No. I.—DENSITIES OR SPECIFIC GRAVITIES.

When an * is attached to a name, it must be understood that the *preceding* numbers have been merely copied from some standard work, and are not the result of the Author's experiments.

Substances.	Density at 60° F.	Authority.
Acadiolite	2·0 to 2·1	Dr. Thomson.
Acanticone	3·43 to 3·46	———
Acetal	0·821 at 72°	Stas.
„	0·842	Liebig.
Acetic acid, monohyd.	1·063	Mollerat.
„ „ 3-hyd.	1·085	Ure.
„ „ commer.	1·025	Brande.*
„ „ of L. Ph.	1·063 to 1·068	Gray.*
Acetone	0·75	Trommsdorf.
„	0·78	Chenevix.
„	0·792 at 64°	Liebig & Dumas.
„	0·822	L. Gmelin.
„	0·925	Löwig.
Acetum distillatum of E. and D. Ph.	1·005	Gray.*
Achirite	3·278	W. Phillips.
Achmite	3·398	Dr. Thomson.
Acidum hydrochl. L. Ph.	1·16 at 62°	
„ E. Ph.	1·17 at 60°	
„ D. Ph.	1·16 at 60°	
Acidum, hyd. dil. E. Ph.	1·05 at 60°	
„ „ D. Ph.	1·08	
Acidum, hydrocyan.	0·998	
A. nitricum L. Ph.	1·50 at 62°	
„ E. Ph.	1·50 at 60°	
A. nit. dil. L. Ph.	1·08 at 62°	
„ E. Ph.	1·077 at 60°	
„ D. Ph.	1·08 at 60°	
A. phosphor. dil.	1·064	
A. sulph. L. Ph.	1·845 at 62°	
„ D. & E. Ph.	1·845 at 60°	
A. sulph. dil. L. Ph.	1·110 at 62°	
„ „ E. Ph.	1·090 at 60"	
„ „ D. Ph.	1·084	
Actinolite	3·2 to 3·5(?)	
„ glassy	2·95 to 3·93	L. Gmelin.
Ærosite	5·8 to 5·9	Bonsdorff.
Æschynite	5·55	Dr. Thomson.
Æther, acetic	0·89	Liebig.
„ benzoic	1·054 at 50°	Graham.*
„ boracic	0·885	Ebelmen.
„ butyric	0·902 at 32°	Pierre.
„ carbonic	0·965 at 66°	Ettling.
„ cinnamic	1·131	Marchand.
„ citric	1·142	Malaguti.
„ formic	0·912	Liebig.
„ hippuric	1·043 (?)	Stenhouse.
„ iodic	1·84	Johnson.
„ mucic	1·17 (?)	Malaguti.
„ nitric	1·112	Millon.
„ nitrous	0·947 (?)	Graham.*
„ œnanthic	0·862	Pelouze.
„ oxalic	1·092	Graham.*
„ pyrocitric	1·050	Malaguti.
„ pyromucic	1·297	„
„ salicylic	1·097	Baly.
„ silicic	0·932	Ebelmen.
„ succinic	1·086	D'Arcet.
„ sulphuric	0·6974	Löwig.
(oxide of ethyl)	0·715 at 68°	Graham.*
„ sulphurous	1·085	Ebelmen.
„ valerianic	0·894	Grotte and Otto.

Substances.	Density at 60° F.	Authority.
Æ. nitrosus D. Ph.	0·90 at 60°	
Æ. sulph. L. Ph.	0·75 at 62°	
„ D. Ph.	0·766 at 62°	
„ E. Ph.	0·735 at 60°	
Ætherole	0·917	Graham.*
Æthyl, bromide	1·473 at 32°	Pierre.
„ chloride	0·874 at 42°	Brande.*
„ iodide	1·92	Marchand.
„ sulphide	0·842	Regnault.
Agalmatolite	2·815	Klaproth.
„ „	2·895	Thomson.
Agate	2·35 to 2·66	Cresy.*
Alabaster	2·31 to 2·326	Beudant.
Albite	2·608 to 2·619	G. Rose.
Alcohol	0·793	Löwig.
„ absolute	0·7938	Drinkwater.
A. absolutum, E. Ph.	0·796 at 60°	
„ P. C.	0·7964	
„ D. Ph.	0·810	
„ L. Ph.	0·815	
Aldehyde	0·79	Gluckelberger.
Allanite	3·119 to 3·80	Dr. Thomson.
Algerite	2·78	Crossley.
Allantois, liq. of	1·007	Vauquelin.
Alluandite	3·468	Damours.
Alum	1·714	Muschenbrock.
„	1·724	Kopp.
„ ammon.	1·626	„
„ chrome	1·848	„
Alumina	4·154	Filhol.
Aluminite	1·705	Stromeyer.
Alum-stone, massive	2·752	Thomson.*
„	2·587	Haüy.
Allophane	1·85 to 1·89	Stromeyer.
Amalgam, native	13·755	Haidinger.
Amazon-stone	2·581	H- Rose.
Amber	1·06 to 1·07	Brande.*
Amber	1·08 to 1·085	Ure.*
Ambergris	0·78 to 0·92	Brisson.
Amblygonite	3·0 to 3·04	Breithaupt.
Amethyst, common	2·75	Moseley.*
„ oriental	3·391	„
Amianthus	1·551	Thomson.*
„	2·313	Moseley.*
Ammonia, liq. anhyd.	0·760	Faraday.
„ sat. aqueous solu. of,	0·850	Brande.
„ bicarb. of,	1·586 at 39°·1	Playfair & Joule.
„ muriate of,	1·533	„
„ nitrate of,	1·58	„
„ sulph. of,	1·75	„
Ammoniæ, aqua, E. Ph.	0·96 at 60°	
„ D. Ph.	0·95 „	
Am., liquor, L. Ph.	0·96 at 62°	
„ *fortior*	0·882 „	
Amnios, liq. of,	1·005 to 1·009	
Amphodelite	2·763	Nordenskiöld.
Amyle	0·77 at 52°	Frankland.
„ oxide	0·779 at 72°	Riechker.
Analcime	2·068	Haidinger.
„	2·278	Thomson.
Anatase	3·857	Haüy.
„	3·826	Moha.
Andalusite	3·13 to 3·31	Thomson.*
Andesine	3·733	Abich.
Anglarite	1·787	Berthier.
Anglesite	6·298 to 6·659	Thomson.*
Anhydrite	2·89 to 2·94	Haidinger.
Aniline	1·028	Fritsche.
Anorthite	2·65 to 2·76	H. Rose.
Anthophyllite, hydrous	2·911	Thomson.
Anthracite	1·559 to 1·630	W. R. Johnston.
„	1·61 to 1·69	T. Herapath.
Antigorite	2·622	Weiser.
Antimonic acid	3·78	Playfair and Joule.

Substances.	Density at 60° F.	Authority.
Antimonious acid	4·074	Playfair and Joule.
Antimony, red	4·09	Klaproth.
„	4·5 to 4·6	Mohs.
„ sesquisulph.	4·75	Karsten.
„ „	4·62	Mohs.
„ „	4·52	Haüy.
Antrimolite	2·096	Thomson.
Apatite	3·1 to 3·23	Haüy.
Aphrodite	2·73	Svanberg.
Aphthitalite	1·73	Phillips.
Aplomb.	3·45	Cresy.*
Apophyllite	2·359	Thomson.
„	2·335	Haidinger.
Aqua distil.	1·000	
„ *barytæ muriatis*	1·23	
„ *calcis muriatis*	1·202	
„ *potassæ*, E. Ph.	1·072	
„ „ D. Ph.	1·080	
„ *pot. carbh.*	1·320	
„ „ *sulp.*	1·117	
„ *sodæ carb.*	1·024	
Aqua marina.	2·65 to 2·76	Cresy.*
Arcanson	1·083	„
Arfwedsonite	3·369	Haidinger.
Argillite	3·609 to 3·68	Cresy.*
Arkansite	3·892 to 3·949	Rammelsberg.
Arragonite	2·685 to 2·930	Stromeyer.
„	2·92	Bournon.
„	2·927	Biot.
„	2·932	Haidinger.
Arsenic, pt. sulph.	3·40	Brande.*
„ ter-sulph.	3·459	Karsten.
„ acid	3·40	Brande.*
„	3·92 at 39°·1	Playfair and Joule.
„	4·25	Filhol.
Arsenical pyrites	6·127	Thomson.*
Arsenious acid	3·884	Filhol.
Arsenious acid	3·702	Karsten.
„	3·698	Dumas.
„	3·695	Guibourt.
„	3·69 to 3·71	Leonhard.
Arsenio, siderite	3·52	Dufrénoy.
Asbesterite	3·0 to 3·31	Thomson (l)
Asbestos	0·681 to 0·998	Cresy.*
„ saturated with water	1·25 to 1·35	„
Asparagin	1·52	Graham.*
Asphalt	1·073 to 1·16	Thomson.*
„	1·205	Klaproth.
Asphaltum, cohesive	1·45 to 2·06	Cresy.*
„ compact	1·07 to 1·165	„
Atacamite	4·43	Thomson.*
Augite	3·297	„
„	3·226 to 3·777	Cresy.*
Automolite	4·261	Ekeberg.
Aventurine	2·643 to 2·666	Cresy.*
Axinite	3·271	Haidinger.
Azure-stone or Lazulite	2·772 to 2·945	„
BABINGTONITE	3·35 to 3·5	
Ball or "Stiff"	2·558	Couper.
Balsam, Copaiba	0·95	Schonberg.
„ „	0·996	Ure.*
„ Mecca	0·876	Bonastre,
„ of Peru	1·15	Thomson.*
Barium, chloride, crys.	2·664	Filhol.
„ do. fused	3·75	„
„ perox. of	4·96 at 39°·1	Playfair and Joule.
Barley, English	1·25 to 1·33	Ure.*
„ Scotch	1·227 to 1·265	„
Baryta	5·456	Filhol.
	4·986 at 39°·F.	Playfair and Joule
	4·733	Karsten.
„ hydrate; (1 at. HO)	4·495	Filhol.
„ hydrate; (9 at. HO)	1·656	„

Substances.	Density at 60° F.	Authority.
Baryta, carb. of	4·301	Mohs.
„ (*Witherite*)	4·330	T. Herapath.
	4·292	Haüy.
„ „ artif.	4·565	Filhol.
„ nitrate of	3·200	„
„ sulph. of	4·4714	Mohs.
„ (*Baroselenite*)	4·472	Thomson.*
	4·483	T. Herapath.
„ „ artif.	4·535	H. Rose.
Baryto-calcite	3·868	Thomson.
„ fluate of lime	3·750	Smithson.
Basalt	2·422 to 3·000	
Basanite	2·59 to 2·64	Hofmann.
„	3·00	Moseley.*
Baudisserite	2·808	Breithaupt.
„	2·915	Klaproth.
„	2·950	Stromeyer.
Beaumontite	2·250	Delesse.
Beer:—		
Bock-bier	1·013 to 1·020	
Burton, 1st S.	1·111 to 1·112	Richardson.
„ 2nd S.	1·097 to 1·111	„
„ 3rd S.	1·077 to 1·092	„
„	1·047	L. Hoffmann.
Com. Ale	1·070 to 1·073	Richardson.
Pale Ale	1·0088 at 52°	L. Hoffmann.
„ Allsop's	1·01	Dr. Ure.
„ Bass'	1·006	„
Porter	1·050	Richardson.
„ best	1·072	„
Stout	1·055 to 1·065	„
Table beer	1·033 to 1·039	„
Benzoïn, gum	1·063 to 1·093	Ure.*
Benzole	0·850	Mansfield.
Benzule, chl.	1·106	Graham.*
„	1·196	Brande.*
Beraunite	2·878	Breithaupt.

Substances.	Density at 60° F.	Authority.
Beryl, oriental	3·549	Moseley.*
„ occidental.	2·723	„
Berzelite	7·0 to 7·1	Thomson.
Bezoar, oriental	1·463	Berthollet.
Bile, human	1·025 to 1·027	Berzelius.
„	1·025 to 1·030	Dr. Garrod.
Biotine	3·110	Monticelli.
Bismuth, blende	5·912 to 6·006	Thompson.
„ glance	5·85 to 6·55	„
„ native	9·737	„
„ needle ore of	6·100	Chapman.
„ „	6·125	John.
„ „	6·757	Frick.
„ nitrate, cr.	2·736	Playfair and Joule.
„ ochre	4·361	Brisson.
„ oxide of	8·179	Playfair and Joule.
„ „	8·200	Brande.*
„ sulphide	7·000	Karsten.
„ „	7·591	W. Herapath.
„ „	7·807	Wehrle.
„ telluride	7·500 to 8·400	„
„ „	7·514	Baumgartner.
Bitumen	1·073 to 1·160	Thomson.*
„ from Judæa	1·104	Cresy.*
„ elastic	0·9053 to 1·2330	Hatchett.
Blende	4·049	Thomson.*
„	3·70 to 4·20	Ure.*
„	4·063	T. H. Henry.
Blood, human	1·050	Thomson.
„	1·053	Richardson.
„	1·0527	Haller.
„	1·0570	Berzelius.
„	1·058 to 1·062	Becql. and Rodier.
„ serum of	1·0257 to 1·0264	Richardson.
„ „	1·027 to 1·029	Berzelius.
„ „	1·047 to 1·050	E. Davy.

Substances.	Density at 60° F.	Authority.
Blood, crassamentum of	1·245	Dr. Jurin.
Blue-spar	3·021 to 3·046	
Bondite	3·571	Thomson.
Bones	1·777 to 2·034	
Boracic acid	1·480	Brande.
„ vitreous	1·75	Breithaupt.
„ „	1·803	Davy.
„ „	1·812	Th. Herapath.
Boracite	2·938 to 2·974	
Borax	1·692	Filhol.
„	1·703	Th. Herapath.
„ octohedral	1·815	Brande.*
„ „	1·8233	Th. Herapath.
„ vitreous	2·3670	Filhol.
„ „	2·3802	Th. Herapath.
„ sat. solu.	1·0211	„
Bornine	7·500 to 8·440 (?)	Wehrle.
Boron	2·000	Brande.*
Botryogine	2·089	Berzelius.
Botryolite	2·885	Klaproth.
Boulangerite	5·69 to 5·97	Rammelsberg.
Bournonite	5·776	Hatchett.
Brandy, Armagnac	0·922	Ure.*
„ Cognac	0·935	„
Brass	7·9 to 8·9	Brande.*
„	7·82 to 8·73	Ure.
„ cast	8·10	Watson.
„ sheet	8·52 to 8·62	Ure.*
„ plate	8·441	Tredgold.*
„ wire	8·49 to 8·73	Ure.*
„	8·544	Tredgold.*
Braunite	4·818	Thomson.*
Breithauptite	3·80	Wälchner.
Brewsterite	2·432	Thomson.
Brick, common	1·557 to 2·000	Tredgold.
„ red	2·168	Rennie.
„ pale red	2·085	„
Brick, London	1·845	Tredgold.
„ Welsh fire	2·408	„
Bromal	3·340	Thomson.*
Bromine	2·966	Graham.*
„	3·060	Kopp.
Bromoform	2·900	Cahours.
Bronze	8·482	Ure.*
„ Chinese	8·815	„
Brookite	4·125 to 4·169	Breithaupt.
„ transpt.	4·128 to 4·131	H. Rose.
„ opaque.	4·165 to 4·167	„
Brown-spar	3·404	Thomson.
Brucite	2·350	Haidinger.
Bruennerite	3·100	„
„	4·250	Mohs.
Bucholzite	3·193	Thomson.
„ hydrous	2·855	„
Bucklandite	3·510	Hermann.
„	3·945	G. Rose.
Buntkupferenz	5·003	Thomson.
Buratite	3·200	Delesse.
Burgundy-pitch	1·080	Thomson.*
Burr-stone	2·511	„
Bustamite	3·12 to 3·25	„
Butter	0·922	
Butyral	0·821 at 71°	Chancel.
Butyric acid	0·963	Pelouze.
„	0·967 at 77°	Thomson.*
Butyrone	0·23 (?)	Chancel.
Bytowrite	2·801	Thomson.
„	2·733	Hunt.
Cacoxenite	2·336	Richardson.
Cadmium, oxide	6·9302	Karsten.
„	8·183	W. Herapath.
„ carbonate	4·420	„
„ „	4·494	Karsten.

Substances.	Density at 60° F.	Authority.
Cadmium, sulphide	4·605	Karsten.
„	4·900	Breithaupt.
Caffeine or Theine	1·230	Thompson.*
Calamine or Zinc-spar	3·584 to 3·598	Smithson.
„	4·334 to 4·376	Karsten.
„	4·442	Haidinger.
„ electric	3·378	„
„ „	3·434	Smithson.
„ hydrous	3·584	„
Calc-spar	2·508	Haidinger.
(Iceland-spar)	2·524 to 2·723	Beudant.
„	2·721	Thomson.
„	2·735	Th. Herapath.
Calcium, fused. chl.	2·240	Brisson.
Calculi, urinary	1·463 to 1·741	Filhol.
„ biliary	0·900 to 1·060	
„ salivary	2·209 to 2·302	
Calomel, native	6·482	
„ artif.	7·176	Haidinger.
Camphene	0·860	Ure.*
„	0·880	Thomson.*
Camphor	0·9857 to 0·997.	Blanchet.
„	0·9887	Ure.*
„	0·9960	Shaw.
Cancrinite	2·4530	
Candite	3·6170	Gmelin.
Canzeranite	2·690	Dufrénoy.
Caoutchine	0·64	Brande.*
Caoutchouc	0·919 to 0·9424.	Ure.*
„	0·942	Th. Herapath.
Capnomor	0·977	Reichenbock.
Capric acid	0·91	Brande
Caproïc acid	0·622	„
Carbolic acid	1·062	„
Carbon, by decomp. of carb. acid	1·571	Poselger.
„ proto-chl.	1·550	Brande.*
„ chloro-sulp.	1·460	Kolbe.
„ bisulphide	1·263	Cluzel.
„ „	1·265	Couërbe.
„ „	1·272	Berzelius.
„ „	1·300	Lampadius.
Carbonic acid, liquid	0·83 at 32° F.	Thilorier.
Cardol	0·978	Stœdeler.
Carinthite	5·706 to 6·760	Haidinger.
Carnelian	2·59 to 2·62	
Castor	2·38 to 2·40	Plattner.
Catapleïte	2·80	Weibye.
Cat's-eye, common	2·600 to 2·625	Cresy.*
Catechu	1·28 to 1·39	Ure.
Cedren	0·98	Walter.
Celestine	3·945 to 3·963	Thomson.
„	3·966 to 3·976	Th. Herapath
Cerin	0·969	John.
Cerite	4·500	Cresy.*
„	4·912	Haidinger.
Ceruse	6·4 to 6·75	Brande.*
Ceylanite	3·575	Thomson.*
„	3·765 to 3·793	Cresy.*
Chabasite	2·088	Thomson.
„	2·100	Haidinger.
„	2·472	Lehunt.
„	2·717	Haüy.
Chalcedonite	6·400	Brooke.
Chalcedony	2·583 to 2·620	Hoffmann.
„	2·583 to 2·664	Brisson.
Chalk	2·657	Moseley.
„	2·4 to 2·6	Ure.*
„	2·315	Tredgold.*
Chalk-stones	1·5771	Th. Herapath.
Chalilite	2·252	Thomson.*
Chamoisite	3·00 to 3·40	Berthier.
Charcoal, birch	0·542	Kirwan.
„ fir	0·441	„

On the Use of BENZOLE *in the Preparation of* VEGETABLE ALKALOIDS.* By JOHN WILLIAMS.

THE application of Benzole to the elimination of various vegetable alkaloïds, having engaged my attention for some time past, I venture to lay the present paper before you, more in the hope of its receiving practical adaptation from your hands, than from any idea that the subject has been worked out to its fullest extent.

First, then, a few words respecting Benzole :—

This important and interesting body was first discovered by Faraday, and named by him *bicarburetted hydrogen;* it was afterwards produced by the decomposition of benzoïc acid by heat in presence of lime or baryta, and hence named benzole ; it has, however, since been proved to exist in very considerable quantities in coal-tar naphtha, or that portion of the liquid products of the destructive distillation of coal, which floats on water, boils below the temperature of 212°F., and congeals to a solid mass at 32°. The ordinary coal-tar naphtha of commerce is a very impure product ; the mode of separating the benzole from it, was, I believe, first pointed out by Mansfield. He took the ordinary naphtha, distilled it in a still with a double head, so constructed that only that portion of the naphtha which distils at the lowest temperature was collected : the product thus obtained was digested with oil of vitriol, which turned it black. I have found it necessary that this part of the process should be repeated at least twice, or the proper degree of purification would not be effected. The benzole thus separated was now washed with water, dried with anhydrous sulphate of soda, and re-distilled ; it was then exposed to a freezing temperature, and when solid, subjected to pressure, when the pure benzole was obtained ; but, for the purposes I have to point out, I do not consider this last operation necessary.

Benzole, thus obtained, is a clear, volatile liquid, having still a distinct odor of coal gas, but more aromatic ; it is quite insoluble in water, and is excessively inflammable ; this last property renders great caution necessary in conducting the processes I am about to describe. Benzole, in its powers of solution, may be likened to ether, but, from its low price, and from its not being so volatile as that body, it possesses practical advantages in working, which, I think, will more than counterbalance its disagreeable odor and great inflammability.

The first application of benzole I have to describe is in the preparation of quinine and its allied bases. An acid infusion of the bark was prepared in the usual manner ; sufficient caustic potassa was added to render it strongly alkaline ; it was then shaken with about 1–4th of its bulk of benzole ; the whole of the quinine, quinidine, quinoïdine, &c., was at once yielded to that agent. I found, however, that the cinchonine was not extracted without greater difficulty. The benzole was distilled off, leaving the mixed alkaloïds as a slightly yellow, resinous

* Read before the "Chemical Discussion Society," Oct. 27th, 1853.

mass, which, when dissolved in weak sulphuric acid and filtered, yielded a solution from which the white bases were at once precipitated, and separated in the usual manner.

This process, I think, would not answer for manufacturing quinine on a large scale, but I find it is a very elegant, certain, and easy mode of performing the analysis of cinchona barks, for if moderate care is used, the alkaline liquid remaining after the action of the benzole does not contain the smallest trace of alkaloïd.

The next substance acted upon was the quinoïdine or amorphous quinine of commerce, which, as usually found, is a blackish, resinous looking substance, with a peculiar, burnt, empyreumatic odor. When this body is powdered and digested with benzole, it yields all its bitter principle to it, the benzole generally separating from the mass almost colorless. By distilling, or dissolving the residuum in weak sulphuric acid, filtering (which effectually deprives the liquid of any odor of benzole with which it may be contaminated), and precipitating with caustic soda, the quinoïdine or amorphous quinine is obtained of a pale straw color, and when dissolved in weak SO^3 yields a nearly colorless solution. Of course the quantity yielded is comparatively small—rarely more than a drachm from an ounce—but often not nearly so much, thus rendering the real quinoïdine a more expensive body for medicinal use than even quinine itself.

I may here mention one great advantage of the use of benzole in preparing these alkaloïds, which is, that the coloring matter contained in the liquid, extract, or powder acted on, is almost invariably left behind, so that with one operation the base is obtained pure, or nearly so, and any odor arising from the benzole is lost when the base is dissolved in acid, filtered, and re-precipitated.

Morphine was the next body to which my attention was directed, but in this instance I failed to obtain a satisfactory result; morphine being but slightly, if at all, soluble in benzole, in which particular benzole again agrees with ether.

With the next alkaloïd, however, I was more successful. Nux vomica seeds were taken, softened in boiling water, sliced in a proper machine, and boiled in many successive waters, until that menstruum ceased to extract either color or taste from the seeds. It might be advantageous to use weak sulphuric acid in this operation; but, for reasons which it is not necessary to mention, this was not done. The liquids were now boiled down in a copper to a convenient bulk, filtered, and again evaporated to the consistence of thin treacle. To this aqueous extract, a strong solution of caustic potassa was added until it was strongly alkaline. It was then mixed with an equal bulk of benzole, well agitated, and kept in a warm stove for about twelve hours. The first benzole was poured off, and a second, but smaller quantity was added, and left to digest in like manner with the extract, and afterwards the mixed benzoles distilled—the residuum, treated with acetic acid, filtered, and precipitated with caustic soda; the precipitate was white, and consisted of strychnia and brucine. These were dissolved in sulphuric acid and separated in the usual manner. The

quantity of strychnine yielded by this process is large; and, when it is dissolved in a little weak alcohol and allowed to crystallise, the base is yielded of great beauty and perfect purity.

The next process I have to describe is one for preparing cantharidine. This substance, though of great use to the medical man, has hitherto, from the great expense of manufacturing it, been comparatively little used in its pure form. Powdered cantharides are to be heated to about 150°, with rather more than an equal bulk of benzole; after about an hour's digestion, the magma is to be transferred to a percolator; and when the first portion of benzole has gone through, fresh warm benzole is passed through it. A pound of cantharides requires about from six pints to a gallon of benzole to be thoroughly exhausted. The benzole is now to be distilled. A rather dark colored, oily liquid is the result, which is to be poured into an evaporating-basin, and left for, say, twelve hours; at the end of which time the liquid will be found studded with magnificent crystals of cantharidine sometimes a quarter of an inch long.

I found some difficulty in purifying these crystals from the mother-liquid, but at length succeeded by washing the whole mass in a beaker-glass with a little cold ether, which dissolved the dark oily liquid freely, but scarcely affected the cantharidine; the crystals subsided to the bottom, and the supernatant liquid being poured off, the crystals were washed once or twice more with very small quantities of ether, and then turned out on bibulous paper; they speedily dried but were not quite white; by dissolving them in a flask in fresh benzole, heating, adding a little charcoal, filtering and allowing the liquid to cool, they were obtained of perfect whiteness, but not so large as at first. From a pound of cantharides about thirty grains of cantharidine may thus be readily obtained.

I must caution those who may follow this process, to beware of the blistering property of the benzole solution of cantharidine in all its stages; unless great care be used severe blisters will be the painful result.

The following applications of benzole have not yielded quite such satisfactory results as the foregoing, but still such as to encourage us to persevere in the application of this body to delicate chemical manipulation, and to prove that it is an important addition to our means of organic research.

Atropine was obtained, though not in distinct crystals, by the following method. An aqueous extract of belladonna, prepared with great care, *in vacuo*, was rubbed with sufficient water to soften it; then a little very concentrated solution of carbonate of potassa having been incorporated, the benzole was added, and the whole well rubbed together. After two or three quantities of benzole had thus been rubbed with the extract they were mixed and distilled; the dark oily residuum was treated with acetic acid, filtered, and precipitated with ammonia, a white curdy precipitate was formed, which, however, was not yet pure, as it refused to crystallise when dissolved in alcohol and evaporated: a resinous or oily matter still contaminated it which interfered with its purification. This subject still engages my attention,

and I hope at a future opportunity to be able to give a better account of the matter.

Extract of henbane, prepared with equal care, was treated in a similar manner; the acid solution of the oily residuum did not, however, give a curdy precipitate with ammonia, but only a slight opalescence; however, upon shaking the liquid with an equal bulk of ether, separating and allowing the ether to evaporate very slowly, a minute quantity of crystals were on one occasion produced, which burnt perfectly, were soluble in a few drops of dilute acetic acid, and precipitated again as a white cloud by ammonia, which cloud was instantly taken up by being shaken with ether.

I next turned my attention to the so-called sulphate of bebeerine of commerce. It was treated with a solution of caustic potassa, and then with benzole; the benzole being distilled off yielded the bebeerine of a pale straw color, but in very small quantity. It was soluble in acids and precipitated by alkalies, and possessed the usual basic characters. I did not attempt to crystallise it.

Extract of digitalis was tried and readily yielded its alkaloïd.

Extract of conium also, the benzole being washed with weak sulphuric acid, yielded the sulphate of conium, which, afterwards distilled with strong potassa, yielded the coneïne in a state of purity.

Taraxacum, sarsaparilla, and colocynth, were tried, but no active principles could be eliminated from these bodies by this process.

Nut galls, in fine powder, were treated with benzole. Tannin, of good color, was the result of its evaporation; but from the impossibility of perfectly freeing it from the odor of the benzole, I fear that this process will not prove of practical use, unless in the production of tannin on the large scale for use in the arts, when the odor would be of minor importance.

I hope to be able on a future occasion to give you a few more illustrations of the practical use of benzole in operative chemistry.

TRANSLATIONS AND ABSTRACTS.

On Alloys, *considered in Relation to their* Chemical Composition.

By M. A. Levol.

This second part comprises the study of the alloys of gold and silver, of gold and copper, and of silver and lead, in the same point of view in which I studied, in the first part, the alloys of silver and copper, that is to say, admitting always, hypothetically, that the metals experimented on are capable of forming with each other combinations in definite proportions, and, starting from this supposition, to seek by experiment the formula proper to each atomic binary alloy whose homogeneousness shall have been established by the analysis of its different parts.

Like the former, these new experiments were made with matters

run into a cast-iron mould formed of two pieces, by means of which a spherical mass of gold could be obtained, weighing, with the jet, about 1 kilogramme; first tested at various parts of its surface, this mass was afterwards divided into two equal parts following the axis of the jet, then analysed internally, as I had done with the alloys of silver and copper.

Study of the Alloys formed of Gold and Silver.—The study of these alloys presented in the first place, in a scientific point of view, a very peculiar interest; it is known, indeed, that notwithstanding the numerous analyses which have been made by different chemists of those among them which constitute native gold, and notwithstanding the isomorphism of gold and silver, it is a question which appears still unsettled, whether gold and silver always occur in atomic proportions in their natural alloys. Now, the mode of investigation which I adopted for the general study of alloys appeared to me of a nature to enable me at least to invalidate the fact, in case I should be able to demonstrate that certain atomic combinations of gold and silver did not present a homogeneous composition. On the other hand, in commerce, we frequently meet with alloys of this kind, and the particular examination of them which I wished to make must naturally lead me to results interesting to the industry of the precious metals, for I need not say that the homogeneousness of the alloys is intimately connected with the fixation of their real value; these researches would, therefore, have a two-fold interest.

"Lewis, in his "History of Gold," and afterwards, Hatchett, in his "Observations" on the various alloys of that metal, have spoken of the alloys which it forms with most metals, and, among others, with silver, as presenting very little homogeneousness; I may here quote what the first of these chemists has said on this subject. ("History of Gold," French translation.*)

"Many people suppose that gold fused with other metals is equally diffused throughout the mass, so that the quantity which can be extracted from every part of the mixture will bear exactly the same proportion to that part which the whole of the gold does to the entire mass. It appears, however, in many cases, that there is a perceptible inequality in the distribution. M. Hellot, in his French translation of the German work of Schlutter, on cast-iron, and the assaying of minerals and metals, gives the details of an experiment which clearly demonstrates this inequality. A quantity of silver, weighing full twenty pounds, and containing about 1-56th of gold, was fused in a crucible and poured into cold water, in order to granulate it. By holding several times an iron spoon in the water, under the stream of the metal, three several portions of the metal were collected. These three portions, tested separately, were found to differ in the quantity of gold which they contained.

"A curious experiment made by Homberg, is detailed in the "Memoires de l'Académie des Sciences" for the year 1713, which I shall venture to give here, on account of its singularity, although I have not yet tried it. Equal parts of gold and silver, fused together and reduced to fine grains, were put into a crucible over a mixture of nearly equal parts of decrepitated common salt and crude nitre. The crucible having been kept over a small fire in a wind furnace, for a quarter of an hour,

* We have sought for Lewis' "History of Gold" in the British Museum, but without success; we have, therefore, been compelled to retranslate the French translation. (Eds.)

and afterwards allowed to cool and then broken, the gold was found at the bottom in a mass, and the silver above in two pieces with a few grains enveloped in the salts which had not been completely fused. The silver was perfectly pure, and without the slightest admixture of gold; but the gold retained about 1-6th part of silver. Homberg repeated the experiment with various mixtures of the two metals, and always found the silver separated from the gold, and quite pure; but the gold retained a little silver, except in two cases, in which it was also found very pure. He observes that, unless the gold and silver are in nearly equal proportions, the separation does not succeed, and that the only delicate point in this process, is to seize the exact point of fusion; for if the heat be continued too long, or if the mixture be allowed to become too liquid, the two metals, after being separated from each other, again become united, p. 165.

"We have already seen that, in certain cases, the alloy may be unequally distributed during fusion, and that the upper and lower portions of the mixture are found to vary in richness. In the large ingots or pieces of gold cast in a mould, it would therefore be necessary to take a little from the bottom, and a little from the top, so as to obtain a mixture corresponding as nearly as may be in quality to the entire mass," p. 249.

It is evident that Lewis does not speak, in this respect, from his own observations, and I may remark, that the experiments which he cites, and which were also afterwards quoted by Hatchett, who seems to admit their accuracy, do not appear to have been made in such conditions as were indispensable for them to be convincing. It must not be forgotten, indeed, that the affinity between metals, even those most disposed to combine, is always very feeble, so much so that they may remain in immediate contact in the liquid form, that is to say in the condition most favorable for generally facilitating all combinations, without combining in any way. I shall presently have to mention a very remarkable fact on this subject. The union of the metals can therefore be effected, when it is possible, only under the influence of a mechanical force, by agitation, or, as it is called in foundries, by minting (*brassage*), which gradually brings their particles in contact. Now, in the two experiments quoted by Lewis, no mention is made of this important and really indispensable manipulation; hence, I think I am warranted in saying that they are not conclusive.

My experiments on the alloys of gold and silver related to the following four binary combinations:—

$$Au^2 + Ag\,;\ Au + Ag\,;\ Au + Ag^2\,;\ Au + Ag^{10}$$

The first alloy ($Au^2 + Ag$) is of a greenish yellow color, and its composition is represented in thousandths, as follows:—

Gold . . .	645·1
Silver . . .	354·9

The sphere formed by this alloy having been analysed, at different points of its surface, and in its internal and intermediate parts, furnished concordant numbers as follows:—

Superficial parts	Above	645·00
	Below	645·25
	Lateral parts . .	645·00
		645·25
Central part		645·25
Internal excentric parts	Above	645·00
	Below	645·00

The alloy $Au^2 + Ag$ may therefore be regarded as homogeneous; but does it result from this that the affinity between gold and silver is very great? This may be judged of from what I am about to relate concerning an accident which occurred in the preparation of this alloy.

As is the custom when metals are alloyed, to obtain the alloy $Au^2 + Ag$, I fused the most refractory metal, the gold, first; its weight was 709·6 grammes: the gold being melted, I added to it sufficient silver to bring it to 645·1 thousandths, which I wished to obtain, that is to say 390·4 gr. of pure silver, then I heated it to liquify the entire mass which the addition of the silver had solidified; this result obtained, I introduced an earthen rod into the bath to stir it, but the movement immediately determined a brisk effervescence, so sudden and so violent indeed, that a considerable portion of the fused matter flowed into the furnace over the edges of the crucible, although the edge was several centimetres above the level of the fused metal. This accident is easily explained: it is evident, indeed, that the two metals, although fused, being superposed in the order of their densities, the silver being uppermost, this metal could absorb oxygen in contact with the air, as it does when fused separately, so that in this case the stirring having induced the combination of the gold with this oxidised silver, the oxygen which it could not retain in uniting with the gold was disengaged; whence the effervescence; whence, also, this evident consequence, that two liquified bodies which may remain superposed without combining, must possess only a feeble mutual affinity. It also clearly results from the same fact, that in such circumstances the silver has a greater affinity for gold than for oxygen.

To repeat this very curious, and, as regards affinities, interesting experiment during a lecture, it would doubtless be best to operate in the conditions which were thus realised accidentally.

The second alloy (Au + Ag) contains in 1000 parts:—

Gold	480
Silver	520

Notwithstanding the large proportion of gold which it contains, this alloy has only a slightly yellowish white color, approximating to that of the alloy of silver of 900 thousandths; rolled to the ordinary thickness of the assays of gold (0 mm 376) and treated with nitric acid in the same manner as those assays, it still retains 217 thousandths of silver; the sphere must therefore be formed of an alloy thus constituted:—

Gold	689
Silver	311

After being remelted, it had a green color.

The sphere, analysed like the foregoing, gave the following results:

		Gold.
Superficial parts	Above	480.25
	Below	480.75
	Lateral points . .	480·50
		480.25
Central part		480·00

Internal excentric parts.	Above	480·25
	Below	480·25

The alloy Au + Ag is therefore homogeneous.

The third alloy ($Au + Ag^2$) possesses the pure white color of fine silver, although it contains nearly one-third of its weight of gold, since its composition is thus expressed in thousandths :—

Gold	312·5
Silver . . .	687·5

Rolled to the thickness of the assays of gold, and treated like them, with nitric acid, this alloy yielded up all the silver it contained.

The sphere $Au \div Ag^2$ furnished the following results :—

		Gold.
Superficial parts	Above . . .	312·50
	Below . . .	313·00
	Lateral parts . .	312·25 / 312·25
Central part		312·25
Internal excentric parts	Above	312·25
	Below	312·25

The alloy $Au + Ag^2$ is therefore homogeneous like the preceding.

These results appear to weaken what Lewis and Hatchett advanced relative to the alloys of gold with silver in the point of view under consideration, and to confirm the anticipations which the isomorphism of these two metals had suggested to modern chemists ; I considered, therefore, that I might terminate the study of these alloys, concluding, however, with the examination of the combination $Au + Ag^{10}$, which is thus represented in 1000 parts :—

Gold	83.4
Silver . . .	916·6

The alloy $Au + Ag^{10}$ is known in the trade in gold and silver, as silver containing gold, and in it the silver always greatly predominates ; so that, as regards the industry of the precious metals, I have thought that it would be proper not to omit to examine at least one alloy in this category.

The compound $Au + Ag^{10}$, tested like the foregoing alloys of gold and silver, furnishing exactly the same numbers in its various parts, superficial and internal.

It results then, from the whole of these experiments, that the alloys of gold and silver, cast in the spherical form, may be considered, provided they have been properly mixed in the bath, as perfectly homogeneous.

Study of the Alloys of Gold and Copper.—Hatchett appears to be the only chemist, hitherto, who has examined the alloys of gold and copper in respect to their homogeneousness. We owe to him three experiments which he made incidentally on these alloys ; the first would prove, as regards their homogeneousness, or rather as regards that of the only alloys which he analysed (the alloy of 956·4 for the first, and the alloy of 913·4 for the last two), the negative, and the other two, the affirmative ; however, it must be observed that, in the course of

his work, he refers several times to the *unequal distribution* of the alloy in the combinations of gold with the different metals, among others with copper; but we can understand that the result of his own experiments on this matter, which were very few, and, indeed, contradictory, must have left him in doubt on this question, with which, I repeat, he was occupied only incidentally.

The experiments which I have made on the compounds of gold and copper, relate to the following alloys,—

1st Alloy, $Au^4 + Cu$=in 1000 parts	Gold	925·0
	Copper	75·0
2nd alloy, $Au^8 + Cu$=in 1000 parts	Gold	903·0
	Copper	97·0
3rd alloy, $Au^9 + Cu$=in 1000 parts	Gold	861·2
	Copper	138·8
4th alloy, $Au + Cu$=in 1000 parts	Gold	756·0
	Copper	244·0
5th alloy, $Au + Cu^2$=in 1000 parts	Gold	608·0
	Copper	392·0
6th alloy, $Au + Cu^{10}$=in 1000 parts	Gold	236·8
	Copper	763·2

The presence of gold would not be suspected in the last alloy; it is of a copper-red color; there is nothing remarkable in the physical properties of the others.

All these alloys, among which I may remark that the second ($Au^8 + Cu$=903 of gold) and the fourth ($Au + Cu$=756 of gold), approximate singularly, in their composition, to the composition of the alloys of gold used in France for making money and the setting of jewels, all these alloys, I say, having been recognised as homogeneous by the experiments made on their surface and internally, it would, I think, be useless to lengthen this memoir by detailing here the numerical results; but I must observe that, to obtain the alloys of gold and copper, it is indispensable to guard against two circumstances which it is very difficult to completely avoid, especially in the most cupreous alloys; I allude to the oxidation on one hand, and, on the other, notwithstanding the very marked affinity which is generally supposed to exist between gold and copper, to the difficulty of alloying these two metals. Indeed, the first four combinations were obtained very easily; but for the latter, it was necessary to fuse and mint them several times in order to arrive at results which should present a satisfactory agreement in analysing the spheres in their various parts; this is, moreover, a fact which had not escaped the attention of Hatchett. With regard to the oxidation, it appears to me to have a considerable influence, the effect of which consists in the almost absolute impossibility of obtaining in a mass of gold, even slightly oxidised, an uniform composition on the periphery of the spheres, so that we may find differences of several thousandths between diametrically opposite parts of the same zone. I have remarked that, during the solidification of the alloy of gold and oxidised copper, the parts most affected are expelled from the central

parts towards the surface; but this elimination is very capricious, because it is often subjected to influences very difficult to master. I may add that it appeared to me that gold fused with copper shields this metal from oxidation less than one would be disposed to think.

As it results from these researches on the alloys of gold and silver, and gold and copper, that these various alloys are homogeneous, we are naturally led to enquire whether the isomorphism, which is the cause by which the homogeneousness of the first may be explained, could not likewise be called to aid with regard to the alloys of gold and copper, and whether more profound observations would not teach us that, like gold and silver, gold and copper are isomorphous? But, for this to be the case, silver must be isomorphous with copper, as it is with gold, and this is difficult to admit when we call to mind the well-proved heterogeneousness of most of the alloys of silver and copper.

Annales de Chimie et de Physique, October, 1853.

(To be continued.)

MINERALOGICAL NOTICES.

In the second part of a paper in *Silliman's Journal*,* "On the Re-examination of American Minerals," by Professor J. L. Smith,—*Chesterlite, Loxoclase,* and one variety of *Danburite* are proved to be identical with *Orthoclase*, another variety of *Danburite* is regarded as identical with *Oligoclase*. For the Greenwood mica and *Biotite*, of Putnam, Co., New York, he adduces the—

Formula.—RO3, $SiO_3 + R_2O_3$, SiO_3.

The latter variety has been confounded with *Pyrophyllite* by some collectors.—The Chesterfield talc proves to be a Mica, probably *Muscovite.*

Rhodophyllite, identical with *Rhodochrome.*

Formula.—4RO3, $SiO_3 + R_2O_32$, $SiO_3 + 10HO$.

Cummingtonite on analysis is shewn to be a hornblende, to which species it was referred from its physical characters.

Formula—(Fe, Mg, O) $4,SiO_33 = RO\ 3$, $SiO_32 + RO$, SiO_3.

The *Hydrous Anthophyllite* of Thomson corresponds with the same formula, and has the composition of a magnesian asbestiform hornblende. *Munrolite* is identical with *Kyanite.*

Formula.—Al_2O_33, SiO_32.

Ozarkite proves to be an amorphous *Thomsonite*

Formula—RO, $SiO_2 + 3Al_2O_3$, $SiO_3 + 7HO$.

Professor Shepard's *Dysyntribite* is shewn to be a rock of indefinite composition, having a relation to *Agalmatolite.*

Gibbsite, pronounced by Hermann to be a hydrous phosphate of alumina containing thirty-seven per cent. of phosphoric acid, proves to be, after very careful examination, a hydrate of alumina.

Formula.—Al_2O_3, HO3.

* Vol. XVI. No. 46, p. 41.

Some specimens analysed, gave but a *trace* of phosphoric acid, others not that.—*Emerald Nickel* is proved to be a distinct species (which was doubted by Miller and Brooke, in the recent edition of Phillip's Mineralogy).

Formula.—NiO, $3CO_2 + 6HO$, or NiO, $CO_2 + 2$ NiO, HO3.

Dr. Genth likewise contributes to the same journal the analyses of the complex mineral *Tetradymite*, for which he adduces the FORMULA Bi (Te, S)$_3$ (?) *Apophyllite* examined for its amount of fluorine, from Rammelsberg, suggesting that, from the variable quantity of this element (from 0·34 to 1·54 per cent.), it might be a silicate, wherein a part of the oxygen is replaced by fluorine—gives the general,

Formula.—K (O, Fl), $2Si (O, Fl)_2 + 8Ca (O, Fl)$, $Si (O Fl)_2 + 16$ HO.

W. P. Blake has discovered a crystallised carbonate of Lanthanum, associated with the zinc ores of the Saucon Valley, near Bethlehem, Lehigh Co. Pa. in Silurian limestone. It occurs as an aggregation of thin plates and scales of a delicate pink color and pearly lustre, forming a light reticulated mass, which by a lens was resolved into well-formed tabular crystals of oblique aspects; the angles were determined as 94° for the obtuse, and 86° for the acute. The crystals doubly refract light—H·2—Sp. gr. 2·666.

Analysis:

HO	CO_2	LaO & DiO
24·09	22.58	54·90

Formula LaO, $CO_2 + 3HO$.

The author proposes for it the name, *Lanthanite.*

Dr. J. Zimmerman's observations *(Taschenbuch für Mineralogie)* on the structure of agates, viz.: that the different varieties of Quartz, amethyst, calcedony, carnelian, jasper, &c., formed the concentric layers of the nodules, has been turned to practical account by Professor Dr. F. Leydolt, who having dissolved out the amorphous from the crystalline bands by the action of fluoric acid, employed the flat surface of the agates thus engraved to print fac-similes of themselves, and thus to illustrate his very interesting paper in "The Transactions of the Imperial Geological Institute of Vienna," "On the internal structure of agates." The results of this process are exquisite, perfectly demonstrating Professor Leydolt's view, that the parts towards the outer surface consist of several spherules variously combined, which are composed of layers of diverse characters, and that the centre of the nodule is amethystine quartz, having a nucleus of very small concentric spherules.

Under the head of "MINERALOGICAL BIBLIOGRAPHY" we may notice the following works lately published:—

REPERTORIUM DES CHEMISCHEN THEILS DER MINERALOGIE. Von C. F. RAMMELSBERG. Fünftes Heft, 1849—1853. 8vo., Berlin.

This forms the Fifth Supplement to Professor Rammelsberg's "DICTIONARY OF THE CHEMICAL PART OF MINERALOGY," a valuable work of reference which should be in the hands of every mineralogist.

DIE MINERAL-NAMEN UND DIE MINERALOGISCHE NOMENKLATUR. Von FRANZ VON KOBELL. München, 1853.

This work professes to give the origin of the names of minerals, but in this respect it is somewhat faulty, and its authority cannot always be depended on; nevertheless, it is an important addition to mineralogical literature. Rules are likewise proposed for the construction of a sound system of nomenclature. In this department, mineralogy, indeed, stands in need of reform.

MINERALOGIE UND GEOGNOSIE. Von F. A. ROEMER. Plates. Hanover.

ELEMENTE DER MINERALOGIE. Von C. F. NAUMANN. Third Edition. Leipzig.

DIE PROBIERKUNST MIT DEM LOTHROHRE. Von C. F. PLATTNER. Third Ed. Leipzig.

DAS MOHSSCHE MINERAL SYSTEM, dem gegenwartigen Standpunkte der Wissenschaft gemäss bearbeitet Von Dr. ADOLF KENNGOTT. Vienna.

DAS KRYSTALLO-CHEMISCHE MINERALSYSTEM. Von GUSTAVE ROSE. Leipzig, 1852.

LERBUCH DER KRYSTALLKUNDE ODER ANFANGSGRÜNDE DER KRYSTALLOGRAPHIE, KRYSTALLO-PHYSIK, UND KRYSTALLO-CHEMIE. Von Prof. C. F. RAMMELSBERG. Berlin, 1853.

An excellent introduction to practical Crystallography. The exposition on the relation of chemical composition to external form is extremely valuable. S. H.

ORGANIC CHEMISTRY.

Action of CARBONIC ACID *on* QUININE *and* CINCHONINE; *formation of* CRYSTALLISED CARBONATE *of* QUININE. By M. LANGLOIS.

WE passed a current of carbonic acid gas into water, containing recently precipitated quinine and cinchonine. The prolonged action of carbonic acid gas determines the solution of quinine and cinchonine; but the former is more readily dissolved than the latter. Both solutions, exposed to the air, lose a portion of their carbonic acid, and furnish the first crystals of carbonate of quinine, the other cinchonine only. We shall see, as we proceed, to what this difference is owing.

Crystallised carbonate of quinine may easily be obtained by following the directions which we shall give.

Ten grains of sulphate of quinine must be dissolved in distilled water, with the addition of a few drops of sulphuric acid. Ammonia is added in order to precipitate the quinine; the latter is collected on a filter and washed; it is afterwards diffused while moist, in a quart of water. The liquid, which has a milky appearance, is put into a test-glass with a foot, into which is passed a stream of well-washed carbonic acid, procured by the decomposition of marble, by means of hydrochloric acid. In less than an hour the quinine is almost entirely dissolved. The liquor, although super-saturated with carbonic acid, always retains an alkaline reaction.

The quinine combines directly with the carbonic acid without dissolving, when it is not diffused in a large quantity of water. By operating, on the contrary, in the way which we have directed, we obtain a complete and very limpid solution, from which are obtained, after exposure to the air, crystals of carbonate of quinine, which increase in size during from twenty to twenty-four hours. After this time no more

is deposited, although the liquor still contains some. Spontaneous evaporation furnishes only quinine; the latter is instantly precipitated by ammonia, potassa and soda, which saturate the carbonic acid. Lime-water acts in the same way, forming, moreover, a deposit of carbonate of lime.

The solution of this carbonate of quinine furnishes at first, as is evident, crystals, represented by the saline combination; and afterwards, this combination is destroyed, giving rise to carbonic acid and quinine. There is, therefore, a perfect analogy between these phenomena and those produced by a solution of carbonate of cinchonine. This latter never gives crystals, because the salt exists in small quantity in it; which is owing, doubtless, to the solubility of cinchonine in water, increasing very little by the intervention of carbonic acid.

Carbonate of quinine is under the form of transparent, acicular crystals; these crystals promptly effloresce in contact with the air; they are soluble in alcohol, insoluble in ether, and they restore the blue color of reddened litmus paper. In the presence of the acids, they cause a brisk effervescence.

At the temperature of 230° F. they are decomposed; the carbonic acid is disengaged, and the quinine remains without undergoing any alteration. It melts only when the heat reaches 338° F.

We found in the decomposition of carbonate of quinine, at a moderate temperature, an easy means of analysing it. The experiments were repeated several times; but we shall content ourselves with detailing only one.

We took the weight of a glass tube 12 to 15 centimetres long, and closed at one end. We introduced into it 0·399 of carbonate of quinine. It was then put into communication by means of a cork covered with caoutchouc, with a bent tube, which is passed under a graduated bell glass, placed over mercury contained in a test-glass, with a stand. The extremity of this tube went beyond the surface of the metal, and reached into the empty part of the bell glass, into which the carbonic acid gas was received. The sealed tube containing the salt was heated in an oil-bath, into which dipped the bulb of a thermometer. As soon as the temperature of the oil-bath attained 230° F., the carbonate of quinine was decomposed, disengaging carbonic acid, and without undergoing perceptible changes in its physical characters.

I obtained from the 399 milligrammes of salt employed for the experiment, 21·36 of carbonic acid gas at the temperature of 32° F., and at the pressure of 76 centimetres. This volume of gas weighed 0·0422 gr. Carbonic acid ceased to be disengaged a long time before the oil-bath had attained the temperature of 338° F., which is that at which quinine enters into fusion, and at which it is entirely freed from the water which it contains. By means of a few pieces of blotting-paper, the moisture which remained adhering to the sides of the tube was easily removed. The weight of this empty tube being known, I obtained, by weighing it again, that of the quinine contained in it. The weight of the latter was 321 milligrammes. We have thus, on one hand, in this analysis, the proportion of the carbonic acid, and on

the other, that of the quinine. The water was estimated by the difference; 0·399 gr. of carbonate of quinine furnished,—

	Grammes.
Quinine	0·3210
Carbonic acid	0·0422
Water	0·0358

These numbers lead us to represent the composition of this salt by the following formula:—

$$(C^{20}H^{12}NO^2, HO)\ CO^2, HO.$$

In fact, we have in hundreths,—

	Theory.	Experiment.
Quinine	80·21	80·45
Carbonic acid	10·88	10·58
Water	8·91	8·97
	100·00	100·00

Six successive experiments on various quantities of carbonate of quinine, gave similar results.

As it must be regarded as neutral, in establishing its composition, we shall also have fixed the cypher of the equivalent of quinine, which corresponds with that admitted by Liebig.

The decomposition of carbonate of quinine at a moderate temperature has enabled us to prove again the non-formation of this salt by double decomposition, that is to say, by treating a saline solution of quinine with carbonate of potassa or soda. The precipitate which is formed contains only quinine, retaining always, notwithstanding repeated washings, a greater or less quantity of the carbonate employed. It is to the presence of the latter, that the precipitate owes the property of effervescing with the acids; but when it is fused in a glass tube, not the smallest trace of carbonic acid is evolved. That which I here say of quinine, applies also to cinchonine, and perhaps even to all the other vegetable bases. We had already given this idea in a note inserted, some years ago, in Vol. XXXII. of the "*Annalen der Chemie*," but then our opinion was founded solely on the results of some reactions which were not of the value of those which we now obtain, by the employment of heat.

Comptes Rendus, November 7th, 1853.

Note on CAPROIC ALCOHOL. By M. V. F. FAGET.

Caproïc alcohol is a limpid, aromatic, very refringent liquid, insoluble in water.

Its density in the liquid state is 0·833, and 0·754, at the temperatures of 32° and 212°F.; the density of its vapor is 3·53. Its composition corresponds to the formula $C^6H^{14}O$.

At a high temperature, potassa converts it into caproïc acid, with a disengagement of hydrogen; the analysis of the silver salt leads to the formula $C^6H^{11}Ag.O^2$.

Three or four grammes of a sample mixed with amylol, treated also

with sulphuric acid, gave a sulpho-salt crystallising in bractese. This salt, which was in too small quantity to be completely purified, left 40·4 per cent. of sulphate of potassa.

A similar quantity of a sample collected between 154° and 166°F., treated with bichromate of potassa and sulphuric acid, gave caproïc acid and an oil boiling at about 320°F., whose vapor-density was 7·34, and whose composition corresponded to $C^{12}H^{26}O^2$. This would be that of caproïc ether, œnanthylic alcohol or the œnanthylic ether of caproïc alcohol, $C^8H^{18}O$.

Other samples collected between 331° and 383°F. gave vapor-densities and analytical results indicating mixtures of œnanthylic alcohol $C^7H^{16}O$, and of Bouis' caprylic alcohol.

Owing to want of matter, I was not able to prepare an ether from caproïc alcohol.

The purest sample of this alcohol was collected between 298° and 309°F.

Comptes Rendus, November 7th, 1853.

ANALYTICAL CHEMISTRY.

NEW PROCESS *for Determining the* INDUSTRIAL VALUE *of* ANIMAL CHARCOAL.

By M. CORENWINDER.

THE employment of animal charcoal in the manufacture of native sugar is unquestionably one of the most beautiful applications which have been made in our time of chemical observations to industry.

Unfortunately no method has hitherto existed by which a value can be assigned to animal charcoal, based on one of its most important properties. A process enabling us to fix this value by cyphers, and thus to assign to it a power capable of being estimated even by persons inexperienced in chemical manipulations, as has been done with regard to commercial soda and potassa, and the chlorides of soda and lime, was greatly to be desired.

At present, when we wish to estimate the industrial value of animal charcoal, we endeavor to determine the comparative degree of its decoloring power, with a sample whose properties are known, by placing it, as nearly as possible, in the same physical state as that which serves as a term of comparison.

The decoloring power of animal charcoal should doubtless be taken into consideration, but this matter possesses another property to which attention has not been seriously paid, namely, its absorbent power.

In the present state of the art of sugar-refining, this latter property is certainly more important to be considered than its decoloring power, since we are able to perfectly free the crystals of sugar from the colored syrup with which they are contaminated, by means of centrifugal apparatus. Moreover, the absorbent power of the charcoal acts in the same direction as the decoloring power, which is evidently due to the absorption of the colored matters in solution in the juices or syrups.

The comparative value of animal charcoal may therefore be established from the quantity of lime which a given weight of this matter is capable of absorbing. Having frequently observed that this power of absorption, which is considerable in new animal charcoal, is much less so in that which has been revived, I have thought that we might base on this property a satisfactory process for giving to this product a metrical efficiency—a determinate value; and this, more especially as this same property is, unquestionably, the most important to the manufacturer, since it has the effect of freeing syrups from a body which is injurious, and which prevents the crystallisation of a certain quantity of saccharine matter.

This much being admitted, it was easy for me to find a method, within the reach of every one, of determining the industrial value of animal charcoal.

Suppose that we have prepared a solution of saccharate of lime; it is easy to determine how many degrees of the dilute sulphuric acid employed in alkalimetry are necessary for saturating a known volume of this saccharate (fifty cubic centimetres, for example). This done, when I have several samples to test, I commence by bringing them as nearly as possible into the same state of division, by passing them separately through the same sieves; then putting a determined weight (fifty grammes) of each sample into separate flasks, I add to each the same volume (one decilitre) of the saccharate, and I allow the contact to continue for an hour.

I then filter the liquids separately. I take, successively, fifty cubic centimetres, and determining, separately, how many degrees of normal sulphuric acid are requisite for completing the saturation, I know by the difference the proportional degrees of lime which have been absorbed by each sample of animal charcoal. That which has absorbed the most is decidedly the best for the consumer, and should be preferred.

This process of estimation is so simple, and so easy of execution, that those whom it concerns will doubtless very soon put it in practice. However, in order to give it greater precision, I have been led, after several trials, to prepare the saccharate of lime and the metrical sulphuric acid in the following manner:—

I begin by preparing an acid liquid, composed of twenty grammes of pure monohydrated sulphuric acid, diluted with water, so as to make exactly one litre.

I also prepare a solution of saccharate of lime,* so that the volume

* If we could completely dissolve a given weight of lime in a saccharine solution, it would take 11·40 gr. of pure lime to exactly saturate twenty grammes of pure sulphuric acid. But as we cannot, I operate as follows:—

I dissolve in water from 125 to 130 grammes of white sugar. I add from fifteen to twenty grammes of quick lime, and boil. I filter in order to separate the undissolved lime, and I make up the filtered liquid to about one litre. I afterwards try with fifty cubic centimetres of this solution, how many degrees of metrical acid are requisite for saturating it. Supposing that it requires 125, I make this proportion: $125 : 100 :: 100 : x = 80$. Then taking eighty centilitres of the saccharate prepared, adding water to make up 100 centilitres, I have a metrical solution of saccharate of lime, which saturates exactly its volume of the metrical sulphuric acid, and which may be used for testing.

of one litre is exactly saturated by this litre of dilute sulphuric acid. In this manner, any volume whatever of this saccharate (fifty cubic centimetres, for example), will necessarily be saturated by a graduated burette, containing also fifty cubic centimetres of metrical sulphuric acid.

In operating on the samples of charcoal to be tested, in the manner above indicated, I first ascertain how many degrees of the burette are necessary for completing the saturation of fifty cubic centimetres of the filtered liquid, after its contact with the charcoal. If it requires 35, for example, 100—35, or 65, represents the proportion of lime absorbed by the charcoal; this is, therefore, as conventionally established, the cypher which represents its metrical power or its degree.

We may operate with a burette the zero of whose graduation is at the lower part; in this way, we read directly the degree of the charcoal tested.

After giving several examples, shewing the value of his method, the author concludes his work with the following remark:—

"We should be deceived if we thought we could rely on the cyphers which I have just mentioned for calculating the absolute absorbent power of animal charcoal for lime. I have made experiments which prove that it absorbs so much the more lime as there is more in solution. There is an equilibrium between the action of the charcoal, the solvent power of the water, and the capacity of saturation of the sugar, which varies according to the quantity of the elements present in the solution.

Comptes Rendus, No. 16, October 17th, 1853.

Improved Methods of conducting Volumetrical Analysis.

By Dr. Mohr.

The attempts which I have made to render volumetric analysis, and more especially alkalimetry more perfect and precise, relate partly to the apparatus and partly to the process.

Among the various forms of apparatus contrived for this purpose, the burette of Gay Lussac has met with the most general application. But this excellent instrument presents, in certain respects, deficiencies which are inseparable from it in its present form, and which in all cases make themselves felt.

In the first place, it is difficult to fill the instrument precisely to the zero point by pouring out of a larger bottle, and it is seldom that the right level is obtained the first time. Perhaps too much may be poured in, and in pouring it out again either too much or too little may be removed, because, by inclining the burette, the level of the liquid is no longer parallel with the graduation. In both cases the operation of filling must be repeated, and it is only after the exercise of some patience that the level of the test liquid is made to coincide with the zero point.

During the experiment the burette remains inclined, and by holding it with the spout over the test-glass, the latter may be shaken and the

operation continued. When, however, the reaction does not appear until after some time, as, for instance, the deposition of chloride of silver, or when it is requisite to apply heat meanwhile, as in the volumetric determination of alkaline carbonates, grape sugar, &c., the burette must then be set aside and the liquid allowed to run back. On resuming the operation, it is then very difficult to add only a single drop at first, and when the saturation is nearly complete, the precise point may be passed by an accidental flow of liquid, and the whole experiment destroyed.

Another disadvantage is, that the quantity of liquid that has been used cannot be read off during the flow. This is particularly desirable in repeating an experiment. Supposing that in the first experiment 32·3 cub-cent. have been added, on repeating the experiment 32 cub-cent. may be added at once, and the last 0·3 cub-cent. added very carefully. But while the burette is in an inclined position, it is almost impossible to pour out exactly 32 cub-cent., partly because the graduation no longer indicates how much has flowed out, and partly because during the flow, the narrow tube is filled, and it should be empty during the reading.

I have therefore endeavored to obviate these disadvantages in the most simple and certain manner, and at the same time to give the apparatus such a form that any one may construct it without possessing so much dexterity in glass-blowing as is required for making Gay Lussac's burette.

After making a great number of experiments with valves and glass stop-cocks, I have abandoned the use of them. When stop-cocks are used, the operation may be effected with great ease and certainty ; but I was unable to procure any which closed so well that I could allow the tube to remain filled with test liquid from one experiment to another. The glass stop-cocks made by Geisler, in Bonn, are admirable ; when closed they remain for a long time air and water-tight, but when solutions of crystallisable substances, such as oxalic acid or caustic soda, are employed, there is always an efflorescence formed round the cone which is pushed out of its place, causing leakage.

I have, however, succeeded in substituting for these expensive glass stop-cocks, an arrangement which may be constructed by any person with ease, which remains absolutely air and water tight for an indefinite period, which may be opened, and regulated at will by the pressure of the fingers, and which costs almost nothing. It consists of a small piece of vulcanised caoutchouc tube, which is closed by means of a clamp of brass wire. The ends of this clamp, which I call a pressure cock (quetschhahn), are bent laterally at right angles in opposite directions, and furnished with knobs so that when both ends are pressed, the clamp is opened, and a single drop, or a continuous current, of liquid may be allowed to escape at pleasure. The opening of the cock is effected on the same principle as that on which the blowpipe forceps of platinum are made.

The measuring tube is a straight glass cylinder as uniform as possible, graduated into 0·2 or 0·1 cub-cent., and somewhat contracted at

the lower end, so as to fit into the caoutchouc tube. A small piece of glass tube, inserted below the pressure-cock, forms the spout.

There is every probability, from the simplicity and durability of this arrangement, that it is capable of numerous applications in the chemical laboratory. It may be used, instead of cocks, in Dobereiner's lamp, for gasometers, for regulating the flow of gas or of water from condensors or syphons. A two-legged tube, furnished with one of these cocks, and placed in a flask of liquid, will thus serve the purpose of a bottle with a glass cock. The pressure-cock has, moreover, the advantage of not leaking, for it closes of itself when the pressure of the finger is removed.

The measure furnished with a pressure-cock is fastened upon an appropriate stand which can be placed at any required height. When used, it is filled above the zero point with test-solution,—the cock opened for an instant so as to let the air escape from the spout,—and the level of the solution then adjusted. This is done by bringing the eye level with the zero point, and applying a gentle pressure to the cock until the liquid has sunk so low that the inferior curve of the liquid touches the graduation like the circle of a tangent, the cock is then closed and at the same moment the liquid remains at zero and continues to do so for weeks when evaporation is prevented. The test measure is now normally filled and the experiment may be commenced; this is done sitting, while the filling of the measure is done standing.

The weighed substance is introduced into an appropriate glass, and the test-solution is allowed to flow into it by gently pressing the cock. Both hands are at liberty, for when the cock is released it closes of itself. The volumetric operation may be interrupted at pleasure, in order to heat the liquid, shake it or do whatever else may be required. The quantity of liquid used may be read off at any moment, and in repeating an experiment the limit of the quantity used before may be approached so near that the further addition of liquid may be made drop by drop.

This apparatus may be used for all kinds of test liquids except permanganate of potassa, which is decomposed by the small piece of caoutchouc, and rendered variable in value. For this substance I employ a glass tube (strchhaber) on the principle of atmospheric pressure contracted above and below, and graduated from the lower extremity upwards in 0·2 or 0·1 cub-cent. As with this liquid the reaction becomes visible immediately, and the application of heat is not required, it does not occupy more than a few minutes, and for that time the pipette may be held in the hand. This very compendious apparatus may be used for all volumetric experiments in which the reaction is immediately apparent. I shall take another opportunity of making known the necessary particulars relating to its convenient form.

By far the most important application of the volumetric method to chemical operations is the system of alkalimetry introduced with such success by Gay Lussac and Decroizilles. However, the process is deficient in the requisite accuracy, on account of the uncertain effect of the bi-carbonated alkalies, and the standard test-solution is pre-

pared with hydrated sulphuric acid, a substance which is not always uniform in its composition, or easy to manage.

For this acid test-liquid there must be an equivalent alkaline liquid which is saturated by an equal volume of the acid liquid.

In the first instance, and for some time, I employed for this purpose a solution of ammonia, but the volatility of this alkali is a great objection to its application. Every time the bottle is opened, and especially on pouring the liquid into the burette, ammonia escapes, as may be perceived by the smell. This loss of ammonia may be recognised with much greater distinctness by the larger volume of the solution which is required to neutralise the acid solution when the bottle containing it has been in use a long time and frequently opened. The use of ammonia becomes, therefore, altogether uncertain; and I have unwillingly abandoned it on account of the readiness with which it may be prepared free from carbonic acid. As a substitute, I have chosen caustic soda. It is objected to this alkali that it readily attracts carbonic acid. But ammonia likewise attracts carbonic acid, although less apparently, for the carbonate of ammonia does not effloresce out during drying, but volatilises. In order, therefore, to prevent the absorption of carbonic acid by the caustic soda, I have adopted a simple arrangement which perfectly answers this purpose. It is, perhaps, impossible to close a bottle so perfectly that air cannot penetrate in consequence of changes in temperature and atmospheric pressure. Instead of attempting to prevent this perfectly, I allow the air to have free passage to or from the bottle, but interpose a substance which absorbs the carbonic acid completely. The bottle is closed with a cork, into which is fitted an ordinary chloride of calcium tube, filled with a finely powdered mixture of sulphate of soda and caustic lime. This tube is left open, and an equilibrium may thus be established between the air inside and outside without a trace of carbonic acid gaining access. I adopt this method of preserving not only the standard test-solution, but likewise my specimens of caustic potassa or soda, lime-water, baryta-water, &c. A bottle of baryta-water which has stood open in this manner for fully six months, does not shew the slightest film of carbonate on the surface of the liquid or the side of the bottle. In the case of the caustic soda test-solution, it is of especial importance to prevent the absorption of the smallest trace of carbonic acid, for the presence of this substance diminishes the precision of the analysis by rendering the change of the litmus from red to blue less sudden and marked.

On a Method of VOLUMETRICAL ANALYSIS *of very general applicability.*

By R. BUNSEN.

(*Continued from page* 106.)

To give some idea of the extraordinary accuracy of this method, which is equally applicable to the analysis of the lower oxides of chlorine, it will perhaps suffice to bring forward the results of a determination of the density of chlorine gas. This was effected by allowing a stream of chlorine gas developed from hydrochloric acid and oxide

of manganese, to flow through water and over chloride of calcium, into a glass tube of 91·005 cub. cent. capacity drawn out at both ends, and tightly connected with the generating apparatus by means of vulcanised caoutchouc. When the tube had acquired the constant temperature of (35°·78′F.), and the atmospheric air was perfectly displaced, it was closed at the end next the generating apparatus by pressing together the caoutchouc tube, and at the same moment the other end terminating in a fine point, was dipped into a solution of iodide of potassium. A rapid absorption of the chlorine immediately ensued, iodine was liberated, and the tube became perfectly filled with the liquid. This liquid introduced into a beaker-glass, gave on the volumetrical determination of the iodine $n = 9$; $t = 44·7$; $t' = 5·0$; $a = 0·0025387$. From these data it may be calculated that the glass tube containing 91·005 cub. cent. of water at 39°·2′ F. contained 0·28191, grms., chlorine under a reduced barometer level of 0^m 7457 and a temperature of 35°·75′ F. This gives as the specific gravity of chlorine :—

Calculated.	Found.
2·4489	2·4482

III. *Bromine determination.*

The centigrade value of a solution of bromine may be easily determined in the same manner as chlorine. The following formula serves for the calculation :—

$$x = \frac{100\ Br.}{A\ I} a(nt - t').$$

As commercial bromine always contains a small quantity of chlorine which cannot be separated without great difficulty, I have for the purpose of testing the method, preferred employing bromine which was separated from pure bromide of potassium. For this purpose 02·869 grms. of pure bichromate of potassa were mixed with two or three grm. of bromide of potassium, and distilled with concentrated hydrochloric acid from a small glass flask into a solution of iodide of potassium containing 0·9030 grm., so as to prevent any loss. As one equivalent KO, $2Cr^2O^3$ liberates exactly three equivalents of bromine, the bromine disengaged by the bichromate, should, according to calculation, weigh 0·4629 grm. The volumetrical determination gave the following data : $n = 6$; $t = 55·4$; $t' = 44·4$; $A = 1·3659$; $a = 0·0025387$. Hence follows the result :—

	Employed.	Found.
Iodide of potassium . .	66·11	66·20
Bromine	33·89	33·80
	100·00	100·00

IV. *Chlorine and Iodine determination.*

When it is desired to determine a mixture or compound of x chlorine and y iodine, it is best to measure two equal volumes of the liquid containing these substances. The one volume is treated with

sulphurous acid, until it becomes colorless, and then precipitated by a silver solution. The weight of the precipitated chloride and iodide of silver after being digested for some time in dilute nitric acid, may be represented by A. In the second volume, the quantity of iodine a (n t—t′) which is equivalent to the chlorine and iodine contained in it, is determined in the same manner as in the case of bromine. From these experiments the following equations are obtained :—

$$\frac{Ag+Cl}{Cl}x+\frac{Ag+I}{I}\gamma=A\,;\quad\frac{I}{Cl}x+\gamma=a\,(n\,t—t').$$

and consequently

$$x=\frac{A—\frac{Ag+I}{I}\,a\,(n\,t—t')}{\frac{Ag+Cl}{Cl}—\frac{Ag+I}{Cl}}$$

This method is applicable only when the liquid does not contain any hydrochloric acid or other compound of chlorine. As this is a condition which so seldom occurs in practice, it is better in all cases to precipitate and determine the iodine in one volume of liquid, as iodide of palladium. If the quantity of palladium obtained by igniting the iodide of palladium is represented by π, the first of the two equations takes the following form :

$$\frac{I}{Pd}\pi=\gamma,$$

whence it follows that :

$$x=\frac{Cl}{I}a\,(n\,t—t'—\frac{Cl}{Pd}\pi.$$

The following analysis of a simple chloride of iodine, prepared with nitro-hydrochloric acid, gave :

$\pi=0{\cdot}1156$; $t=241{\cdot}6$; $t'=131{\cdot}4$; $n=1$; $a=0{\cdot}0050$.

	Calculated.	Found.
Iodine	21·85	21·85
Chlorine	78·15	78·15

V. *Chlorine and Bromine determination.*

In order to ascertain the percentage of chlorine in bromine, it is sufficient to dissolve a well-dried determinate quantity A, in the solution of iodide of potassium, and then to determine as above the quantity of iodine a (nt—t′) separated. The equations upon which the calculation is based are as follows, chlorine being represented by γ, and bromine by x:

$$x+\gamma=A\,;\frac{I}{Br}x+\frac{I}{Cl}\gamma=a\,(nt—t'),$$

whence it follows that,

$$\gamma=\frac{a\,(nt—t')—\frac{I}{Br}A,}{\frac{I}{Cl}—\frac{I}{Br}}$$

For the purpose of testing this method, 0·1148 grms. of bromine containing chlorine, but free from iodine, and dried over chloride of calcium, was weighed in a glass bulb, converted into hydrobromic acid by means of sulphurous acid, and then precipitated with silver solution. The chloride and bromide of silver obtained, weighed 0·2826 grm. When 0·1148 is represented by A, and 0·2826 by B, the percentage of chlorine contained in the substance examined may be ascertained by means of the equations:

$$x + y = A'; \frac{Ag + Br}{Br} x + \frac{Ag + Cl}{Cl} y = B'$$

$$\text{or } y = \frac{\frac{Ag + Br}{Br} A' - B'}{\frac{Ag + Br}{Br} - \frac{Ag + Cl}{Cl}}$$

0·094 grm. of the same bromine digested in iodide of potassium gave, by the volumetrical determination, A=0·0948; n=1; t=80·5; t'=17·3; a=0·002578.

	Weight determination.	Volumetrical determination.
Bromine . .	93·42	93·34
Chlorine . .	6·58	6·66
	100·00	100·00

VI. *Valuation of Chlorites and Hypochlorites.*

The solution of the salt is mixed with the solution of iodide of potassium and hydrochloric acid until a faint acid reaction is caused. The weight of chlorous acid x, or of hypochlorous acid x', is then ascertained, by means of the volumetrical determination of the quantity of iodine separated =a (nt—t'), form the following equations in which A represents the weight of the salt or mixture of salts operated upon:

$$x = \frac{100\ ClO^2}{4\ IA} a\ (nt - t')$$

$$x' = \frac{100\ ClO}{2\ IA} a\ (nt - t'$$

For the purpose of testing the method, a mixture of caustic potassa with hypochlorite of potassa was selected. In order to be certain of using a definite weight of the latter, 0·3256 grm. of pure bichromate of potassa was heated with hydrochloric acid, and the chlorine disengaged passed into a solution of 4 grm. of caustic potassa. As 2 eqts. KO^2CrO^3 generate under these circumstances, exactly 3 eqts. of chlorine, the solution of potassa must contain 0·1427 grm. of the latter substance.

The volumetrical determination gave n = 4; t = 83·6; t' = 8; a = 0·00278. This corresponds with the composition.

	Taken.	Volumetrical determination.
Hydrated potassa . .	96.66	96·52
Hypochlorate of potassa.	3·45	3·48
	100·00	100·00

The method is peculiarly well adapted to the technical valuation of chlorinated lime. When such a quantity of a solution of this substance is used as corresponds to the weight $\frac{100\ Cl}{I}$ a of dry chlorine, the difference of the two volumetrical observations (nt—t'), gives the bleaching power of the substance directly on the percentage of chlorine.

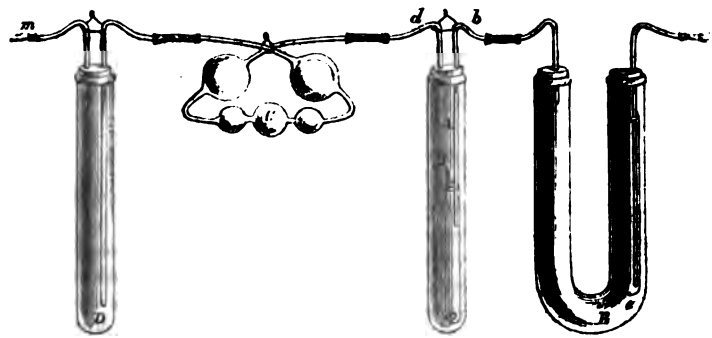
(To be continued.)

New Apparatus *for* Estimating Carbonic Acid.

By M. S. De Luca.

The apparatus for determining carbonic acid in use in laboratories, have generally a fault in their construction, which injures the clearness of the results, always leaving a doubt as to their accuracy; in fact, with these apparatus, the estimation is made by the difference, that is to say, by the loss of weight in the substance analysed. To obviate these inconveniences, I have invented an apparatus which, by being under control, removes the cause of error.

This apparatus, represented in its complete state by the following figure, is composed, 1st. of a tube *A*, in which the decomposition of the carbonate takes place; 2nd. of a washing-tube, *B*, intended to deprive the air of its moisture and carbonic acid; 3rd. of two different portions, *C* and *D*, which are intended to receive all the carbonic acid proceeding from the decomposition of the substance analysed.



The apparatus *A* contains a small tube *o*, closed at one end, into which is introduced the carbonated substance, the weight of which has been previously ascertained; the tube *o* communicates with the ex-

ternal air by the tube *b*, which may be closed by means of a small glass tube corked at one end, and into which is fixed a small caoutchouc tube. This same tube *o*, is connected with a small tube *c*, bent parallel to *o*, and which descends nearly to the bottom of the tube *A*. Into this large tube is poured concentrated sulphuric acid in sufficient quantity to come about half up *o*. This acid is intended to react on the carbonate contained in the tube *o*: this operation, to which we shall shortly recur, causes a disengagement of gas, which passes through the tube *d*, which communicates with an apparatus *C*, intended as well as *D*, to retain the carbonic acid produced by the reaction.

The apparatus *C* is formed of five bulbs, one of which, the inferior bulb, elongated at each end, contains 5 cubic centimetres; the two middle ones contain 10 cubic centimetres; and the two superior bulbs 15 cubic centimetres. Into this apparatus are put 15 cubic centimetres, of a concentrated solution of potassa.

Any traces of carbonic acid which may escape the action of this caustic ley, are sure to be condensed in the tube *D*, filled with pieces of potassa.

Finally, the washing apparatus *B*, is composed of an U tube, containing in one of its branches a tube *e*, corked at one end and filled with pumice-stone, which has been steeped in a concentrated solution of potassa, or of small pieces of caustic potassa, and into which descends almost to the bottom a tube, communicating with the external air. After depositing its carbonic acid in this tube *e*, the air passes out by the bent tube *f*, and passes through a column of sulphuric pumice, filling the U tube, *B*.

The corks of this U tube are covered with sealing-wax: it is the same with the cork of the apparatus *D*. The cork of the apparatus *A* is partly covered with sealing-wax, so as to leave the tube of communication *b*, at liberty.

These four apparatus, *A*, *B*, *C*, and *D*, are connected in a very simple manner; for this purpose I use small glass tubes, open at each end, into which have been introduced caoutchouc tubes.

This manner of closing tubes, or of establishing a communication between different apparatus, by means of glass tubes containing tubes of vulcanised caoutchouc, advantageously replaces the inconvenient method of using tubes of simple caoutchouc, fixed on to glass tubes by means of silk, as employed in organic analyses.

The above description will be sufficient to explain the manner of using this apparatus. Nevertheless, here are some further details, which will complete the description.

The substance to be analysed is weighed directly in the tube *o*, then moistened with water; the tube is then closed with the cork which contains the two tubes *b* and *c*; the necessary quantity of sulphuric acid is poured into the tube *A*, the tube *o*, furnished with *b* and *c*, is introduced, and the large tube *A* is corked with the corresponding cork, which is supplied with two openings intended to give passage to the two tubes *b* and *d*; a platinum-wire, which unites these last two tubes, facilitates the handling of the apparatus, when it is

wished to ascertain the weight: it is the same with the apparatus *C* and *D*.

After weighing the three apparatus *A*, *C*, and *D*, the small tube *b* is closed with a glass tube closed at one end and lined with caoutchouc; the three apparatus are to be connected as above-described, and a portion of the air contained in the apparatus is removed by the mouth at *m*. On ceasing the inspiration, the external air returns into the apparatus to re-establish the equilibrium of pressure; but as it cannot enter the tube *o*, which is closed by the sulphuric acid, it acts by pressure on this acid, and forces it to penetrate into the tube *o*, where it meets with the carbonated substance. The decomposition immediately commences; the carbonic acid produced by the reaction is disengaged by the tube *c*, passes first through the sulphuric acid, where it leaves its humidity and passes thence into the tubes *C* and *D*, where it condenses.

When the reaction is finished the tube *b* is opened to make a communication with the apparatus *B*; a fresh inspiration is made at *m*, and a current of dry air is thus caused which passes through the tubes *o* and *A*, and removes from them all the carbonic acid which is likewise deposited in the apparatus *C* and *D*.

One precaution should be taken before the establishment of the current of air, namely, to push the tube *b* half-way down the tube *o*, so as to force the air to reach the bottom of the tube, and thus remove any little acid which may be there.

The weighing is then proceeded with; the loss of weight of the apparatus *A* indicates the quantity of carbonic acid contained in the substance analysed; this loss should correspond to the augmentation of weight of the two apparatus *C* and *D*.

An experiment only requires from 200 to 300 milligrammes of matter.

With the same apparatus *A*, when slightly modified, I determine the carbonic acid when found in the state of bicarbonate, and the water which this same bicarbonate abandons under the influence of heat. For this purpose the tube *b* is moved which is engaged in *o*, and it is pushed down almost to the bottom of *A* parallel to *o*, which is raised sufficiently to put it into communication with *d*; on the other hand, the tube *c* is plunged to a level with the bottom of *o*, and rises in an upright position parallel to the tube *d* until it has passed through the cork; this tube *c* is open at both extremities.

Thus placed, the apparatus thus modified is made to act in the following manner: into *o* is put some concentrated sulphuric acid; into *A* is introduced a known weight of bicarbonate, and this bicarbonate is carefully heated with a spirit lamp. What occurs under these circumstances is well-known: one equivalent of the carbonic acid, constituting the bicarbonate, is disengaged, enters *o* by the tube *c*, deposits its moisture in the sulphuric acid, and goes out by the tube *d* into the apparatus *C* and *D*.

To remove the last portions of carbonic acid which remain in the tube *A*, a current of air is driven through it by means of the apparatus *B* in the above-mentioned manner.

Experiments made with this apparatus, in the laboratory of M. Pelouze, and in that of the *Collège de France*, have given the most satisfactory results, for the determination of simple carbonic acid, for the estimation of the carbonic acid in the state of bicarbonate, and for that of the water of crystallisation.

The whole apparatus is placed on a wooden stand.

Journal de Chimie Médicale.

AGRICULTURAL CHEMISTRY.

On the INFLUENCE *of* IODURETTED MANURES *for preserving the* VINE *from the attack of the* OIDIUM TUCKERI, *and on the* PECULIAR QUALITIES *of the* WINE *from* VINES *thus treated.*

By M. RIVET.

I WAS invited in 1847, by Dr. Loze, to examine a variety of wine to which extraordinary properties were attributed. In 1849, my friend, M. Souleyet afforded me a similar opportunity, boasting of the efficacy of this wine in certain diseases. I had detected iodine in it, when I was informed that the vines from which it had been produced had not been attacked by the *Oidium*, and that M. Mouriès had obtained remarkable cures by the aid of iodised manures. I then followed up the experiments which I now have the honor of submitting to the judgement of the Academy. My researches have brought to light the following facts :—

1. The manure produced by the fermentation of marine plants has been employed since 1835 in some parts of Spain. The soil into which this manure is introduced, contains, on the average, 1–600,000th of iodine. The vines growing in it have hitherto escaped the *Oidium*.

2. The wine produced by these vines possesses special properties ; it bears the name of Malaga Rives de Mer in commerce ; it is very rare. It is the natural product richest in iodine ; it contains this principle in the proportion of 1–50,000th on the average.

3. The iodine naturally contained in vegetables or in animals, exerts an action which, from its nature and intensity, cannot be compared with that of its chemical preparations. This principle is in organised beings in a state of combination which heightens and modifies its action.

Comptes Rendus, November 7th, 1853.

FORENSIC CHEMISTRY.

Memoir on the MEANS *to be employed for* DETECTING *and rendering perceptible* FRAUDULENT ALTERATIONS *in* PUBLIC *and* PRIVATE DOCUMENTS. By MM. CHEVALLIER and LASSAIGNE.

THE numerous experiments which have already been tried at various times, have made known the processes which may frequently be put in practice for causing the reappearance of traces of writing effaced by chemical reactions, and for throwing light on the work of the guilty

person.* But there are cases in which all the means proposed for this purpose fail, and then the criminal may escape justice from the want of conclusive material proofs.

The study which we have made of many questions connected with this subject, has led us to undertake various experiments, some of which have given results too important for us to defer their publication.

If, as has already been proved, it is not always possible to cause the reappearance of the effaced writing, for which other written words have, with a fraudulent intention, been substituted; at least, as our experiments demonstrate, we may recognise by some effects which are manifested on the surface of the altered paper, the places where the criminal act has been performed, circumscribe them by a simple chemical reaction visible to the least practiced eye, and even measure their extent. In a word, the invisible alterations produced on a deed are susceptible, owing to the partial modifications which the surface of the paper has undergone, of being differently affected by certain chemical actions, and of being rendered visible.

The experiments to which we devoted ourselves, on the occasion of a judical investigation of great importance, committed to us by one of the judges of the *Tribunal de Première Instance* of the department of the Seine, have led us to the knowledge of the facts which we now publish:—

1.—The surface of paper sized in the ordinary way, or letter paper, no longer presents, with certain reactions, the same uniformity when it has been either accidently moistened in several places by various liquids, or left in contact, for a certain time, with agents capable of removing or destroying the characters which have been traced on it with ink.

2.—The application of a thin layer of gum, of starch or farina, of gelatine or fish glue, with the view of sizing certain parts of the paper, or of causing certain bodies to adhere to it momentarily, is detected by an action similar to that which shews paper to have lately been blotted by the contact of liquids.

3.—The heterogeneousness of the pulp of the papers, and the kind of size with which they are impregnated, lead to differences in the results which are observed with the same chemical reagents.

We shall now, with the details into which we think we ought to enter, examine each of these propositions, and describe the means which we have employed in endeavoring to solve questions of so high a degree of interest.

1.—The homogeneousness of the surface of sized paper not partially altered by the contact of liquids (water, alcohol, salt-water, vinegar, saliva, tears, urine, acid-salts, and alkaline-salts), is demonstrated by the uniform coloration which this surface takes on being exposed, if not wholly, at least in various parts, to the action of the vapor of iodine

* In THE CHEMIST, Vol. I., First Series, 1840, will be found an interesting report by M. Chevallier, of some operations by which he caused the reappearance of some erased writing in a diploma.

disengaged at the ordinary temperature from a flask containing a portion of that metalloïd.

In order to study the effect of this iodous vapor on the surface of a paper, we used with advantage a bottle with a wide mouth, this mouth was kept closed with a round plate of ground glass. Twenty or thirty grammes of iodine crystallised in plates, as it occurs in commerce, were placed at the bottom of the bottle. The portion of the paper on which we wished to make the vapor of iodine act, was placed in connection with the mouth of the bottle, and was kept there by covering it with the ground glass lid, on which a weight was added so as to exert a slight pressure, thus closing it more perfectly.

The surface of the dry paper was thus left to the action of this vapor for three or four minutes only, in a room the temperature of which was about 60° F., and after the expiration of that time the paper was withdrawn and attentively examined,

The employment of a small square vessel of glass or porcelain covered with a plate of glass, enabled us to expose a greater surface of the sheet of paper to the action of the vapor of iodine; we also made use of a wooden box closed with a lid sliding in a groove, such as is used for iodising the silvered plates in the daguerreotype.

When the surface of the paper has not been stained by any of the above-mentioned liquids, an uniform yellowish, or light brownish yellow coloration is noticed on the whole extent exposed to the vapor of iodine; in the contrary case, the surface which has been moistened and afterwards dried in the open air, is perfectly distinguished by a different and well circumscribed tint.

On the papers into whose paste starch and resin have been introduced, as glazed letter-paper and common papers prepared mechanically, the stains present such delicate reactions that we may sometimes distinguish by their color the portion of paper which has been moistened with alcohol from that which has been moistened with water. The stain produced by alcohol takes a bistre-yellow tint; that formed by water is colored of a more or less deep violet blue, the dessiccation having been effected at the ordinary temperature.

For the stains occasioned on these same papers by other aqueous liquids, the tint, apart from its intensity, resembles that of the stains of pure water. The feeble or dilute acids act like water on the surface of the same paper containing starch in its paste, but the concentrated mineral acids, by altering, more or less, the substances which enter into the composition of the latter, give rise to stains which present differences.

These reactions are not the same with regard to the papers employed in the manufacture of stamped paper. However, as we have established by numerous proofs, we may always recognise by the action of the vapor of iodine the parts of the paper which have been put in contact with chemical agents, the energy of which has been arrested by washing with cold water. We were able, on several ancient deeds, written on stamped paper, and a few words of which had been removed by us with chemical agents, to recognise the places where their

action was exerted, to see and to measure the extent which they occupied on the surface of the paper.

The testing of a paper with the vapor of iodine will present this double advantage over the methods hitherto practised for detecting falsifications in writings, that it points out at once the place in the paper in which any alteration may be suspected, and that, on the other hand, it enables us to act afterwards with the reagents proper for causing the reappearance of traces of ink, when that is possible. If the means which we now propose cannot always make the former writing appear, they demonstrate the places where the alterations must have been made, when, however, the want of uniformity presented by the surface of the paper is not explained by any circumstance. This proof becomes, therefore, a weapon which the guilty person cannot avoid. But might not the presence of a stain or several stains developed by the vapor of iodine, in different parts of a public or private deed, give rise to suspicion when these stains have, perhaps, been occasioned by the spilling of some liquid on the surface of the paper, and would it not be rash and unjust to raise an accusation from such a fact?

There would, indeed, be great temerity in drawing such a conclusion from a fortuitous circumstance, but the inferences which may be drawn from the place occupied by these stains on the surface of the paper, from the more or less significant words found in those places, would not permit an accusation to be so lightly brought, when simple reasoning would be sufficient to destroy its basis. Besides the subsequent reactions which would be made, would certainly never revive words formerly written and effaced, whilst the latter effects may often be produced, more or less visibly, on those parts of the paper on which falsification has been practised, figures or words being substituted for other figures or words.

The more uniform pulp of stamped papers, the different way in which they are sized, renders these papers more or less susceptible of being stained with water, alcohol, saline solutions, and the weak acids, and then the stains which are demonstrated in them by means of the vapor of iodine are due to liquids, the energy of which has affected the fibres of the pulp, or the size which holds them together. In all these respects, the stamped paper, the preparation and sale of which are superintended by the government, will always present greater guarantees against falsification than the ordinary papers made of a pulp to which starch and resin have been added.

The forger cannot allege in his defence, that the want of homogeneousness of the surface of the paper is due to the presumed action of water, saliva, tears, or other liquids which may have fallen on it accidentally, and may have modified that part of the surface of the paper with which they may have remained in contact for any length of time.

2.—The applications made to the surface of a sheet of paper, with the view of covering it again at certain parts with a fine layer of gum, gelatine, starch or flour paste, or in other places to cause other sheets

of paper to adhere, may be recognised not only by the reflection of light falling on the paper inclined at a certain degree of obliquity, and by the transmission of light through the paper, but also by the varying action which the vapor of iodine exerts on this surface which is not homogeneous. Papers containing starch and resin are more powerfully acted on by this vapor than stamped papers of a less complex composition. Both, in the parts covered with starch or flour-paste, are colored, in a few minutes, of a violet blue, but with starched papers alone, a more intense coloration is manifested on the places covered again with a thin layer of gum arabic, fish-size or gelatine, whilst the same substances spread on some portions of the surface of stamped papers are not colored more perceptibly than the parts which are not covered. By looking, then, at the surface of the paper held somewhat obliquely to incidental light, we distinguish clearly, by their different aspect, the parts on which these various substances have been applied.

The vapor of iodine, in condensing at the ordinary temperature on the surface of the papers to which any kind of size has been applied in various places, produces differences which are most commonly well recognised by the greater or less transparence of the paste of the paper.

3.—The heterogeneousness of the pulp of the various papers of commerce, and the nature of the size with which they are penetrated cause differences, either in the coloration which the surface of these papers takes when exposed to the vapor of iodine, or in the tint which is manifested in the portions of size deposited in certain portions of that surface; thus, papers with starched pulp generally turn brown or blue, according to the amount of water which remains in their interstices; papers, like those of the Imperial stamp, turn yellow only under the influence of the vapor of iodine, and the parts which have received superficially a layer of another agglutinative body, resist this action for a certain time, and are distinguished from the parts of the paper which are not covered with it.

The means mentioned in the course of this memoir were successfully applied by us, on the occasion of a cause tried this year before the Court of Assize of l'Hérault.

Journal de Chimie Médicale, November, 1853.

Note on the Means of Detecting and Demonstrating the Presence of Picric Acid *in* Beer. By J. L. Lassaigne.

The very powerful bitterness of picric acid, which resembles that which characterises the principle extracted from hops, and which has led to its use as a partial substitute for hops, in certain localities, cannot be distinguished in beer which contains it by the taste alone, as we have proved by direct experiments; but the employment of some very simple chemical reactions enabled us to determine its presence.

In studying the properties of picric acid we remarked that this

acid which communicates to water its color and bitterness, when dissolved in beer, is not precipitated by subacetate of lead, whilst the bitter and coloring principles of the hop are almost entirely precipitated by this basic salt. We have likewise proved that ordinary animal charcoal, as that which has been purified by the acids, absorbs, precipitates, and combines with, the coloring principle of the beer, whilst the picric acid remains in solution with its natural tint, and does not combine with the charcoal.

The means which I employ for detecting small quantities of picric acid added to beer is founded on the application of these two properties.

In the experiments which I have made on this subject, I acted comparatively on pure beer, of good manufacture, and on a certain quantity of the same beer to which I had added 1–12000th, and even only 1–18000th of picric acid.

By pouring into these beers an excess of tribasic acetate of lead, or agitating them with an excess of powdered animal charcoal, I proved that pure beer was very nearly, if not completely, decolored, whilst beer containing picric acid, in the above-mentioned proportions, remained of a citron-yellow color owing to the non-precipitation of this acid.

I do not think that this process will enable us immediately to detect in beer a smaller quantity of picric acid than I have mentioned; in such cases, it will be necessary to concentrate the liquids by evaporation.

Journal de Chimie Médicale.

PHOTOGRAPHY.

On a New Varnish *for* Heliographic Engraving *on* Steel.

By M. Niepce de Saint-Victor.

This varnish has the fluidity of albumen, and spreads and dries as easily as collodion, which enables us to operate ten minutes after the steel plate has been covered. The following is its composition:—

Benzine	100 grammes.
Pure Bitumen from India	5 „
Pure yellow wax*	1 „

I have also modified the solvent† in the following manner:—

Oil of naphtha	5
Benzine	1

I may also mention, that I have been enabled to render my varnish sufficiently sensible to light to be able to operate in ten minutes, or a quarter of an hour, at most, in the *camera obscura*, and a few minutes suffice when we operate by contact of the solar rays.

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* When the substances are dissolved, the varnish is strained through a cloth with pressure, and is then allowed to settle, and decanted; if the varnish becomes too thick, benzine may be added.

† See "*Comptes Rendus*, May 23rd, 1853.

The varnish is rendered sensitive by pouring on the plate anhydrous sulphuric ether, containing a few drops of essence of lavender.

After the plate is dry, it is exposed to the light.

The heliographic operations being completed, the steel plate is engraved by the processes described by M. Lemaitre (Sitting of May 23, 1853).

Observations.—It is essential that the steel plate be perfectly cleaned before applying the varnish; for that purpose an essence or oil of naphtha is used for removing the varnish, then alcohol and tripoli with cotton for drying it completely. Moisture must be avoided by every possible means, for it is injurious to the varnish. The exposure to light of the engraving on the plate should be from two to three hours, when we operate by contact (without ether); however, that depends on the intensity of the light and on the thickness of the layer of varnish. I recommend that this layer should not be laid on too thick.

The operation by contact appeared to me preferable to that of the camera obscura, as regards the vigor of the design.

In order that the heliographic operation may succeed well, the metal must be bare at the parts corresponding to the deepest shades only; then the demi-tints will exist naturally. After having removed the solvent, the plate is exposed to light, in order to dry and consolidate the varnish. The action of the varnish must always be stopped promptly, and if water removes the varnish, it is a proof that the light has not acted sufficiently, or that moisture has been present.

Direct or positive photographic proofs may be reproduced on thin paper, without the necessity of waxing them, and I have evidence that they are produced very well.

This varnish may be very well applied to lithographic stone.

I have tried to substitute essence of lavender for benzine in the composition of the varnish; but although this substance is much more sensitive to light than benzine, I prefer the latter, because it is much more easy to evaporate, and gives a more homogeneous layer.

However, perhaps essence of lavender with ether may one day be employed for operating in the camera obscura.

With the essence of lavender, the plate must be heated after having spread the varnish, in order to dry it more quickly; even then, it is necessary to wait twenty-four hours before operating.

These processes have already given beautiful results, in skilful hands.

Comptes Rendus, October 31st, 1853.

New Process *for* Positive Proofs.

I have tried a method of preparing my paper for positive proofs, which, as I have not seen it mentioned as employed by others, and the results appear to me very satisfactory, I am induced to communicate to you, and to accompany by some specimens, which will enable you to judge of the amount of success.

I use a glass cylinder, with air-pump attached, such as that described by Mr. Stewart, as employed by him for iodising his paper. I put in this the salt solution, and that I use is thus composed; two drachms of sugar of milk, dissolved in twenty ounces of water, adding—

Chloride of barium	15 grs.
Chloride of sodium	15 „
Chloride of ammonium	15 „

In this I plunge several sheets of paper rolled into a coil (taking care that they are covered by the solution), and exhaust the air. I leave them thus for a few minutes, then take them out and hang them up to dry; or as the sheets are rather difficult to pin, from the paper giving way, spread them on a frame, across which any common kind of coarse muslin or tarletan, such as that I enclose, is stretched.

I excite with ammonio-nitrate of silver, thirty grains to one ounce of water, applied with a flat brush.

I fix in a bath of plain hyposulphite of the strength of one-sixth. The bath in which the inclosed specimens were fixed has been in use for some little time, and therefore has acquired chloride of silver.

I previously prepared my paper by *brushing* it with the same salt solution, and the difference of effect produced may be seen by comparing a proof so obtained, which I inclose, with the others. This latter is of rather a reddish-brown, and not very agreeable tint. The advantages which the mode I have detailed possesses are, I think, these:—

Greater sensitiveness in the paper,

A good black tint, and

Greater freedom from spots and blemishes, all very material merits.

—"*Notes and Queries*," No. 208. C. E. F.

PHYSICS.

THERMO-CHEMICAL *Investigations Concerning* COMBINATIONS *Formed in* MULTIPLE PROPORTIONS. By M. P. A. FAVRE.

FIRST PART.

THE force which induces bodies to enter into combination, and which maintains the stability of the compound formed, is designated under the name of affinity.

Affinity is energetic in proportion to the intensity of the force brought into play for separating the combined elements.

A connection between affinity and calorific manifestations has long been admitted, and is now denied by no one. The works of modern thermo-chemistry appear even to authorise the establishment of a kind of proportionality between the heat disengaged in combinations and the degree of affinity inferred from the comparison of the whole of the chemical properties of the bodies. I may cite in support of this view, a memoir recently published by M. Silbermann and myself, referring particularly to the chapter on the calorific equivalents of bodies.*

* "*Annales de Chimie et de Physique*," April 1853, pp. 484 *et. seq.*

Considered thus, affinity would become, to a certain extent, susceptible of being measured by a certain quantity of latent heat, which becomes sensible when the bodies enter into combination, and which must be restored to them to operate their chemical segregation. This heat might be called the *latent heat of affinity*.

If the study of the chemical properties of compound bodies once generated has more than once aided speculative ideas, and elucidated the theories of the molecular constitution of compounds, it appears to me almost certain that an attentive and accurate study of the physical phenomena accomplished in the very act of combination, should be calculated to elucidate not only the problems relative to the molecular constitution of bodies, but likewise the progress of descriptive chemistry itself.

In our previously published thermo-chemical researches, M. Silbermann and I made known several results relative to the disengagement of heat which accompanies the formation of various definite chemical compounds; but we scarcely touched, in a thermo-chemical point of view, upon the phenomena which relate to the series of various combinations which the same two elements may form with each other, that is to say, the compounds which obey the law of multiple proportions.

The experiments hitherto known and made in this point of view have related merely to the hydratation, in various degrees, of sulphuric acid, or to a very small number of oxides and chlorides.

It was important to examine various series of compounds, especially the oxygenous compounds of the metalloïds.

The researches included in this first memoir will relate to the various compounds of the metalloïds with each other.

In comparing thus, for example, the quantities of heat disengaged by a constant weight of the same element (represented, for example, by its equivalent) in combining with different quantities of oxygen acid forming the series of oxidation, we might hope to obtain relations of some interest as regards molecular chemistry.

The results which I shall now give will form the first part of a necessarily more extended work.

However, it must not be concealed that it is with these kind of thermic phenomena as with the specific heats in their relations with chemical equivalents; that is to say, that the peculiar state of aggregation of the compound obtained, and, moreover, the differences in the physical state of the analogous elementary bodies which are compared, may introduce perturbations which might disappear, perhaps, if we could always take account of the calorific action which accompanies these changes of state and of mode of aggregation.

A few examples will render my idea more clear. Suppose that we have to compare the series of the oxygenous compounds of chlorine with that of bromine or iodine. The differences of physical state of these three bodies, which are so similar in their chemical properties, may and should render it more difficult to distinguish the simple laws which might preside over these phenomena, when their latent

heats are not known. Again, bodies which present the same physical state, but a variable molecular state, may give rise to some uncertainties in the comparisons which it is desired to establish. Thus, in comparing the series of the oxidation of phosphorus with that of arsenic, we should not neglect to set out, for example, from the ordinary state of phosphorus or from the amorphous state (red), in order to compare the thermic phenomena with those which correspond to the oxidation of the arsenic. It will be seen that my experiments completely justify this anticipation.

My researches have related, as often as it was possible, to the various modifications of one and the same body entering into combination.

The experiments now about to be detailed were made with the aid of the mercurial calorimeter described in the joint work of M. Silbermann and myself.* This apparatus being known to physicists, it is not necessary here to describe it, or to adduce proofs in support of the degree of accuracy to be expected from it.

With regard to the oxidation of metalloïds whose combinations with chlorine are decomposable by water, I have always had recourse to the action of hypochlorous acid in solution; the final reaction was always the production of an acid of the metalloïd, and a disengagement of chlorine resulting from the decomposition of the hypochlorous acid.

Consequently, the necessity of fixing, by previous experiments, the quantities of heat brought into play by the decomposition of the hypochlorous acid for taking account of this element frequently intervening in the various determinations will be understood.

Heat brought into Play in the Formation and Decomposition of Hypochlorous Acid.—The result sought for, that is to say, the knowledge of the heat disengaged by the oxidation of chlorine to the state of hypochlorous acid, may be arrived at by two different ways. 1st., By decomposing hypochlorous acid with hydrochloric acid. By taking account of the decomposition of the hydrochloric acid in solution and of the formation of water (these last two calorific effects being known by previous experiments), we have the elements necessary for knowing the heat absorbed by the decomposition of the hypochlorous acid in solution. 2nd., We arrive at the same result by making the chlorine react on dilute potassa. I have given the preference to the results furnished by this second mode of determination, for reasons which will be appreciated further on, when I detail the results furnished by the first method.

The following is the experiment which furnished the elements of the solution sought for, founded on the second method.

The reaction is formulised by the following equation:

$$2Cl + 2KO + \infty Aq = Cl\,K + Cl\,O,\ KO + \infty Aq.$$

In this equation there are reactions which give rise to disengagements, and others which occasion absorptions, of heat, and it is possible

* "*Annales de Chimie et de Physique*," t. xxxvi. p. 33.

to deduce the number sought, x, from an equation in which the other terms are known from previous experiments.

Thus we designate by :

x the heat brought into play by 1 equivalent of chlorine (35·5 gr.) passing to the state of hypochlorous acid in solution.

x' the heat disengaged by the combination of liquid hypochlorous acid with dilute potassa.

A the heat disengaged by 1 equivalent of chlorine which combines with the potassium to produce chloride of potassium in solution.

D the heat absorbed by the decomposition of an equivalent of dilute potassa.

R the heat collected by the calorimeter at the time of the reaction of the chlorine on the dilute potassa.

We should evidently have

$$R = x + x' + A - D.*$$

This equation contains two unknown ones ; A and D are known from previous experiment†. x' will be determined experimentally by combining dilute hypochlorous acid with dilute potassa in the calorimeter.

The value of x, that is to say, the quantity of heat brought into play by the formation of hypochlorous acid, may then be known—a quantity which cannot be considered *à priori* as either positive or negative.

To determine x', we must not operate with an excess of hypochlorous acid, for reasons which will be developed further on ; the acid not being in excess, the reaction is clear and terminates suddenly.

The pipette for measuring the volume of the normal potassa contained 0·340 gr. of real potassa ; I estimated in consequence a solution of hypochlorous acid, in order to employ volumes of potassa and acid capable of saturating exactly.

I found that 1 gr. of potassa treated by an equivalent quantity of hypochlorous acid gave :

I.	. . .	225·8
II.	. . .	227·1
III.	. . .	228·6
Mean.		227·2 units of heat.

This mean multiplied by 47—the equivalent of potassa—gives for the value of x', the number

10678·4 units of heat‡

which represents the *calorific equivalent* of hypochlorite of potassa. §

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* In general Σc designating the sum of the heats brought into play in the combinations, and Σd, the sum of the heats brought into play by the decompositions, we shall have :— $R = \Sigma c - \Sigma d$.

x figuring in the terms comprised in Σc or Σd.

† A=97091
D=76238.

‡ This is one of the lowest calorific equivalents among the acids; it is inferior to that of carbonic acid itself taken under the weight of 22 grammes.

§ I will here explain what I mean by *calorific equivalent*:—

In a memoir presented to the Academy of Sciences on the 21st of May, 1849,

In order now to obtain the value of R, or the heat disengaged by the reaction of the chlorine on the potassa, I operated in a tube placed in the calorimetric muffle, by means of arrangements analogous to those employed in circumstances described in a former work.

This test-tube contained 21 cubic centimetres of dilute solution of potassa, and was fitted with an abductor-tube dipping to the bottom of the liquid. The outer extremity of the abductor-tube was adapted by means of a caoutchouc tube to the glass stop-cock, through which the dried chlorine was passed from a gasometer. On turning the stop-cock, the chlorine passed into the solution. This stop-cock was opened, more or less, in order to regulate the flow of the gas from the gasometer with an uniform pressure. The gasometer in which the chlorine was contained a solution of common salt, and it was by this liquid that the gas was displaced.

The quantity of chlorine disengaged should never reach the proportion necessary for the complete saturation of the potassa.

To ascertain the proportion of chlorine which had acted, it was sufficient to weigh the calorimetric eprouvette bearing the gas-tube, before and after the action of the chlorine on the potassa.

The calorimetric effect was measured according to the method described in the memoirs already mentioned.

This done, a chlorometrical test of the decoloring liquor obtained was made, to serve as a means of control to the determination of the chlorine, and to prove that chlorate was not formed

The following is the result of the experiments :—

1 gr. of chlorine, absorbed in these conditions by dilute potassa, disengaged,—

I. . . .	338·6
II. . . .	341·0
III. . . .	341·4
Mean.	340·3 units of heat.

By multiplying this number by 71, that is, by the weight which

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M. Silbermann and I designated as the *calorific equivalent* of a compound body, the number of units of heat disengaged by the formation of a chemical equivalent of this compound expressed in grammes (H being 1 gr.).

Thus, the *calorific equivalent* of water, for example, is the heat disengaged by the formation of nine grammes of water expressed in units of heat=34462, which may also be called the *heat of formation* of an equivalent of water by attributing to it the weight of nine grammes.

We have thought it useful to assign a peculiar denomination to the calorific numbers regarded in this point of view, because they enable us to recognise relations interesting as regards theoretical chemistry, as we have tried to demonstrate.

In a recent edition of his "*Cours de Chimie*," M. Regnault has thought fit to apply the name of *thermic equivalent*, to the chemical equivalent of a body peculiarly satisfying the law of specific heats.

Whilst regretting the possibility of a confusion of terms, I may be permitted in this memoir to retain, at least provisionally, a denomination whose precise sense had already been fixed, leaving to physicists the definitive adoption of this expression, or of any other equivalent denomination, necessary, in all cases, for the numerical determinations to be made in this point of view.

represents two equivalents of chlorine, necessary for the formation of hypochlorous acid, we find :—

$$R = 24161{\cdot}3 \text{ units of heat,}$$

and as we have besides,—

$$A = 97091$$
$$D = 76238$$

then follows after substitution of various numbers,—

$$x = R + D - A - x' = -7370 \text{ units.}$$

There is, therefore, an absorption of 7370 units of heat at the moment of the union of the oxygen and the chlorine, to form an equivalent of hypochlorous acid weighing 43·5 and remaining dissolved.*

I have spoken above of a method which should furnish the same solution by the decomposition of hypochlorous acid with hydrochloric acid.

The reaction is formulised by the following equation :—

$$ClH + ClO + x\,Aq = 2\,Cl + HO + x\,Aq.$$

We should then have the relation,—

$$R = A - D - x.$$

A, heat disengaged by the formation of HO.

D, heat absorbed by the decomposition of dilute ClH.

x, heat absorbed by the decomposition of dilute ClO.

To make the experiment, 0·4176 of dilute hydrochloric acid were poured by means of a pipette, into an excess of hypochlorous acid, dissolved and saturated with chlorine in the calorimetric eprouvette.

In two consecutive experiments, the range of variation of the mercurial column did not exceed 3·5 mm.

By referring the results to 1 gr., we have

I. . . .	54·67
II. . . .	57·88
Mean.	56·27 units of heat.

By multiplying by 36·5, the equivalent of hydrochloric acid, we obtain for the value of R

$$2054 \text{ units}$$

whence

$$x = -7784 \text{ units,}$$

to represent the heat absorbed by the formation of one equivalent of hypochlorous acid remaining in dilute solution.

This number differs from the preceding 7370 found by the other method, but it cannot weaken it; for in the last experiment described, the effect on the calorimeter was comparatively very feeble, with equal

* In the foregoing experiments I have admitted the composition of hypochlorous acid established by M. Balard, in his remarkable memoir on that acid, as well as the equivalent ClO fixed by Gay Lussac.

quantities of matters in the employment of the two methods. In the last place R was 2054 units; in the other system of experiments R= 24161.

The errors of observation must therefore have a much greater influence to alter the true cypher when we operate by the means indicated in the last place.

In order to explain, moreover, the direction of the error in surplus, it will suffice to call to mind that the solution of hydrochloric acid not being saturated with chlorine, there is an abnormal elevation of temperature after the reaction, due to the solution of a portion of the chlorine which ought to have been disengaged in the gaseous state.

I shall therefore adopt the number 7370 for representing the quantity of heat absorbed by the formation of one equivalent of hypochlorous acid obtained in the state of solution.

Remarks relative to the Calorific Phenomena presented by the Combination of Hypochlorous acid with Potassa.—I have said above that the experiment relative to the combination of hypochlorous acid with potassa, for determining the heat disengaged, required particular precautions. When we operate with most acids indeed, we may maintain an excess of acid in the calorimetric eprouvette, which afterwards receives the alkaline liquor; the effect obtained is the same as in operating on a quantity of acid strictly equivalent to the quantity of potassa employed, provided that the product formed remains dissolved.

The combination of the potassa with the hypochlorous acid presented peculiarities which must be noticed here, I will first state what I observed, and then endeavor to explain it.

Whenever potassa is added in sufficient quantity exactly to saturate the hypochlorous acid contained in the calorimetric eprouvette, we obtain, for 43·5 gr. of hypochlorous acid combined, or one equivalent, 10678 units of heat.

When the quantity of potassa added is in the proportion of one equivalent to two of hypochlorous acid, we obtain a first prompt calorific effect which approximates to the above number; but the action continues slowly, and we finally obtain 22114 units of heat, for 87 gr. of hypochlorous acid and 47 gr. of potassa. Which tends to prove, that an acid salt is not formed, and that the second equivalent of hypochlorous acid is not decomposed, because by finishing the saturation with a new equivalent of potassa, we obtain 10678 units of heat.

It must therefore be concluded that, under the influence of an excess of acid, the hypochlorite formed undergoes a decomposition in which the free acid does not participate, and which complicates the calorific effects proved in this case.

It is known that M. Balard has pointed out the instability of hypochlorite of potassa in presence of free hypochlorous acid, and the transformation of the hypochlorite into chlorate and chloride.

Now it is easy to ascertain by a very simple calculation, taking account of the heat disengaged or absorbed by the reactions which

accompany this transformation, that the final result should be a considerable increase of the heat disengaged.

Which proves to me the accuracy of the calorific number obtained for hypochlorous acid, by effecting the saturation with the precautions desired, because the decoloring strength of the hypochlorous acid employed is found again entire in the hypochlorite formed, whilst it was impaired when we operated in different conditions.

Journal de Pharmacie, October, 1853.

(To be continued.)

Additional Observations on CHARCOAL. By M. C. DESPRETZ.

I HAVE employed the expression "truncated octohedra" in speaking of the small quadrangular pyramids formed particularly on the extremity of the wires; this is an expression which, from its impropriety, would give a false idea of the black crystals observed in the experiment made with the induced current. It would be supposed that the pyramidal points have no summit, whereas, in reality, they are complete. By the term "truncated octohedra," I intended to say that one-half of the octohedra did not exist; it was an inadvertence. I knew well that all who have the slightest knowledge of geometry are well aware that a truncated pyramid—that a truncated cone,—is a pyramid, a cone from which a portion of the summit has been detached.

These octohedra were arranged nearly as they are in a bed of alum or crystallised nitrate of lead.

The illustrious Dean of the Academy has kindly remarked that there is a deficiency in my note, and has asked me whether the small octohedra are opaque or transparent. They are opalescent and translucid; it is the same with the white plates which resemble the octohedra in appearance.

I had omitted to characterise the small octohedra completely in the *Comptes Rendus* of the 5th of September, (*See* "THE CHEMIST," Nov., 1853, page 112), although I had done so in the notes which had been asked of me by the Editors of the *Institut* and of the *Cosmos*.

The reflection of these small crystals, and of the white plates, is in all respects similar, in my opinion at least, to that of the rough diamonds which I have had an opportunity of seeing since I read the note.

Several members of the Academy have appeared to fear that the charcoal which I used contained impurities! I have stated that this charcoal was as pure as possible; it was the charcoal of which I spoke in the five communications which I had the honor of making to the Academy, in 1849 and 1850, on the fusion and volatilisation of bodies.* I prepared this charcoal with white, crystallised sugar-candy, in which M. Germain Barruel found only almost inappreciable traces of extraneous matters.

The charcoal alluded to burns almost without residue; it is the same with the products to which we call the attention of the Academy.

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* *See* "THE CHEMIST,"

In 1849, I directed my researches partly to the fusion and volatilisation of charcoal. I was bound not to incur the risk of falling into the error into which several physicists in various countries had fallen, of mistaking silicates for fused charcoal, an error which, perhaps, I also should have committed, had I been the first to endeavor to fuse charcoal.

Retort charcoal, the various graphites,—even the purest English graphite,—the anthracites, and wood-charcoal, treated in the air by the fire of the battery, exposed to the focus of a powerful annular lens, or to the action of the oxygen blow-pipe, always leave as a residue, colorless or colored vitreous globules. These are silicates of various degrees of hardness; this is a fact known to all chemists and physicists; whilst the charcoal obtained from white crystallised sugar-candy, subjected to the most intense heat of the battery, gives, as we have proved, black globules, soft to the touch, marking paper in the same way as crayons; it is graphite.

After my numerous researches on the fusion and volatilisation of charcoal, it was not to be supposed that I would select impure charcoal for the experiments in which I should endeavor to crystallise that body.

Some people object that rubies are polished with other matters than the diamond. Undoubtedly, rubies are rough-ground with emery on a cast-iron or leaden wheel, moved with more or less speed, and they are polished with Venice tripoli on a brass wheel. But the emery and tripoli are, in this operation, *mixed with water*, whilst on a fixed plane of rock crystal, it is only the powder of the diamond, which, mixed with a little oil polishes the ruby rapidly and perfectly. Neither silica nor alumina, nor any powder whatever, gives this result. M. Gaudin has completely proved this several times. Besides, the charcoal used in my experiments contains neither silica nor alumina; it cannot yield any.

Finally, the charcoal of gas-retorts, which wears the gravers so quickly, contains one-fiftieth of its weight of foreign matters, consisting of silica, alumina, oxide of iron, &c., and yet this charcoal is only a little superior, as regards the property of polishing stones, to wood-charcoal, to charcoal suddenly volatilised, and infinitely inferior to the charcoal deposited in our experiments by the humid way, or by the dry way.

Since reading my note, I have examined the product of an experiment which lasted, perhaps, six months. Liquid chloride of carbon, diluted with alcohol, was submitted to the action of two very small and very feeble elements; the positive wire (formed of copper) was covered with small greenish crystals; the negative wire (of platinum) was surrounded by a mammelated brownish sheath, with many small reflecting faces. In detaching it, it was broken and reduced to powder; this powder, tried with oil, by the process already indicated, approximated, in power, to the product of the induction apparatus.

Another experiment, arranged in the same manner, gave opalescent white crystals similar to those furnished with the induction apparatus.

The product of this experiment was mislaid before it could be subjected to the test for estimating its hardness.

It was natural to enquire whether the charcoal suddenly volatilised by a very powerful battery, does not furnish a matter analogous to that which we obtained by slow volatilisation or by slow deposition; this powder, mixed with oil and tested by the process of the fixed plane of rock crystal, was found to be rather superior to wood-charcoal, and rather inferior to the charcoal deposited on the sides of gas-retorts. This charcoal, suddenly volatilised, is probably too much divided to have any rapid action on hard stones.

We may class the various charcoals in the following order; wood-charcoal, charcoal suddenly volatilised by the heat of the battery, gas-charcoal, charcoal transported directly by the humid way, charcoal resulting from the slow decomposition of a mixture of chloride of carbon and alcohol, and charcoal deposited by the arc of induction.

These last two have the force of the diamond reduced to fine powder; however, we must remark that all the charcoal deposited in this kind of experiments has not the same force; it is especially the part in which crystals have been perceived that the power of polishing rubies has been found to exist in the highest degree.

All the processes followed, by nature or by art, for the production of crystals, are not equally applicable to the subject with which we are occupied.

The first condition of success is the slowness and the continuity of the operation. I have had recourse to five modes of experimentation. In the first mode, the pure charcoal is slowly carried away by the arc in the induced current at a temperature differing little from that of surrounding bodies, as shewn in the second part of my former note.

In the second, the charcoal is transported directly and slowly by the humid way.

In the third mode, carbonaceous combinations are decomposed by feeble currents; this third process is analogous to that which our colleague, M. Becquerel, followed for determining the crystallisation of insoluble compounds.

In the fourth, I decomposed certain carbonaceous combinations with Nairne's machine.

In the fifth, we made chemical mixtures in the conditions which we thought most proper for giving the results sought for.

In nature there are neither our galvanic batteries, nor our electrical machines, nor our induction apparatus. It certainly is not by the two processes which I have hitherto found most successful that diamonds are formed and grow in the interior of the globe; it is probably by that process which furnishes in laboratories the crystals most remarkable for the clearness and purity of their forms. Chemists know that whenever they leave for a long time a humid mixture of various substances capable of reacting on each other, they discover in it all the crystallised combinations possible, soluble or insoluble, oxides, sulphurets and salts, according to circumstances, and sometimes altogether.

To sum up :—

Have I obtained crystals of carbon which can be isolated and weighed, and of which the index of refraction and the angles of polarisation may be determined? Certainly not.

I have simply produced hitherto, by the arc of induction, and by weak galvanic currents, carbon crystallised *in black octohedra, in colorless translucid octohedra, in colorless and translucid plates*, the whole of which had the *hardness* of the powder of the diamond, and which disappeared in *combustion* without leaving any perceptible *residue*.

I beg the Academy to allow me to read a letter from M. Gaudin, who has most carefully studied the action of our products on hard stones, and particularly on rubies :—

"I hasten to give you the information which you have asked concerning the cutting of precious stones, and the experiments to which I subjected the various samples of carbon obtained by electricity which you have sent to me.

"The oriental stones, such as corundum, are rough-ground on a wheel of cast-iron, steel or lead, furnished with coarse emery; then the facettes thus produced are polished with Venice tripoli, on another wheel, of brass, with very fine notches. This operation is performed in water, since oil would prevent the emery and tripoli from biting. For polishing, strongly calcined alumina is much superior to tripoli, in the rapidity and beauty of the work.

"My persevering researches on the production of the artificial ruby and fused rock crystals, have led me to grind and polish my samples myself, in order to study their texture. I have found the employment of a plane of rock crystal, which enables me to cut a great number of globules at once, preferable to a lapidary's wheel.

"For the polishing of rock crystal and the ruby, after very numerous trials, I have found nothing comparable to very fine diamond-powder employed with oil; with this fatty body, alumina does not bite on fused rock crystal, and still less on the artificial ruby, which is harder than most oriental stones.

"I had, therefore, acquired much skill in judging of the operation of diamond-powder on the ruby, when you gave me an opportunity of trying your products; and the peculiar texture of my artificial rubies has greatly aided in giving much accuracy to any experiments.

"I ordinarily fix three rubies on a plate of gum lac, in order to have a solid support for each facette; and if, after having polished these rubies with the diamond, I rub them on the plane furnished with alumina and water, the polish of these rubies is removed, and we perceive, by the aid of a lens, that the removal of the polish arises solely from the hollowing of the softer parts; there results a damask which cannot disappear however long it may be worked with chemical alumina.

"Since I have been in possession of the small platinum-wire (one centimetre in length), put aside by you, charged with a great number of microscopical crystals of octohedral form, I have scraped this wire with the greatest care on the middle of my plane of rock crystal,

after having removed on this same plane, with alumina in water, the polish from three rubies fixed with gum lac, and having well cleaned the plane, an imperceptible quantity of oil having been added to the powder, I have immediately recognised a free working, quite similar to that of very fine diamond-powder.

"At the end of a few minutes the damask of the rubies had disappeared; all the prominences were levelled; the rubies presented, in a word, a perfectly plain and brilliant surface, such as I have never obtained except with diamond-powder.

"I was so struck with this result, that I felt bound to examine whether some of the diamond-powder formerly employed might not still remain, although rock crystal is not, like metals, penetrable to this powder.

"Therefore, after having again removed the polish from the same rubies with alumina, and having cleaned the plane and added a trace of oil, I recommenced the polishing operation, but without any result; alumina added to the oil produced none either; whilst the remainder of the matter adhering to the platinum-wire rapidly produced, as it had done before, a very bright polish.

"It was this that made me tell you that I am perfectly convinced of the existence of microscopical diamonds around the platinum-wire which you sent to me.

"The black powder obtained by the humid way, which I tried in your presence, also decidedly polishes the ruby, but not so quickly as the carbon obtained by the dry way; finally, the deposit obtained by sudden volatilisation by means of your great battery, gave only an almost inappreciable result, which appeared to me to be intermediate between that of wood-charcoal and that of the graphite of gas-retorts.

"I classify these bodies, therefore, in the following order of hardness:—

"1. Electrical deposit by the dry way;

"2. Electrical deposit by the humid way;

"3. Graphite of gas-retorts;

"4. Carbon suddenly volatilised;

"5. Wood charcoal.

"I have separately tried the graphitous sheathes of platinum-wires which had been used in the experiment by the dry way, and which, observed with a microscope, resemble charcoal made from an essence in a porcelain tube, and have not found them more biting than wood-charcoal.

"Finally, after eight or ten trials with wood-charcoal, graphite, your instantaneous deposit and alumina with oil, all of which appeared almost inert, and which demonstrated the complete absence of previous diamond-powder, I have just repolished, successfully, rubies whose polish had been purposely removed, by using a new specimen of charcoal which you have obtained by a slow chemical decomposition, and which I have found almost as hard as that obtained by the dry way.

"All this appears to me to confirm in the clearest manner the existence, on intact wires, of true implanted diamonds, the form and

color of which you have been able to see with the microscope; and when you have produced more, it will be easy to repeat my experiments on the hardness before a witness."

Comptes Rendus, No. 12.

PHENOMENA *Presented by certain* LIQUIDS *when* PROJECTED *in* SMALL DROPS *on the* SURFACE *of an* ETHER. By M. SIRE.

WHEN a drop of crystallisable acetic acid is allowed to fall into a glass tube of two or three centimetres in diameter, closed at one of its extremities, and containing ordinary sulphuric ether, raised to the temperature of 90° F, this drop remains floating on the surface of the ether, notwithstanding the difference of density, and, far from diminishing in volume, owing to evaporation, it swells rapidly so as to attain, in many cases, six times its original volume. This experiment may easily be repeated by maintaining the temperature of the ether by heating the tube in a sand-bath.

I have endeavored to produce the same phenomenon with other liquids; nitric acid and monohydrated sulphuric acid are the only liquids with which I have succeeded, operating always with sulphuric ether. I may mention, that hitherto the phenomenon has always been manifested in the highest degree with nitric acid.

Micrometric measures gave two 1–10th millimetres as the mean diameter of the drops which I projected on the ether by means of a very fine pipette. Having determined the primitive volume of the drops, and when they had reached their maximum size, I found that the proportion of the original volume to the final volume was on the average from 1 to 3 for sulphuric acid, from 1 to 5 for acetic acid, and from 1 to 12 for nitric acid.

When the order of the liquids is inverted, that is to say, when we project successively upon the above-mentioned acids, heated to about 104° F., small drops of sulphuric ether, we see the drop of ether roll quickly over the surface of the acids, rapidly diminish in volume, and then become confounded with the surface.

I have said that the drops, in swelling up, acquired a very rapid motion; sometimes a very regular rotation is produced. I have thought that I remarked that this motion occurred so long as the swelling continued, and that these two effects ceased together; thus, when the drops have reached their maximum size, they are quite motionless. By regularly maintaining the temperature, I have seen drops of acetic acid remain from twenty to twenty-five minutes.

When the drops are motionless, colored rings are observed on their upper part throughout the surface of a spherical segment, the height of which appeared to me to be 1-3rd of the height of the drops. These colors are very vivid and perfectly visible to the naked eye, especially in operating with nitric acid.

Comptes Rendus, Oct. 31, 1853

TOXICOLOGY.

On the COMPARATIVE POISONOUS *powers of* ARSENIOUS *and* ARSENIC ACIDS. By M. SCHROFF.

ARSENIC acid is generally considered as possessing more powerful poisonous properties than arsenious acid. This was the opinion of Orfila, Buchner and all toxicologists, and this opinion is also expressed in the fourth edition of Orfila's "*Médecine Légale.*"

However, in 1848, MM. Wöhler and Frerichs, expressed a contrary opinion in communicating a series of very interesting researches on the modifications which certain substances undergo in passing through the organisation. They founded them on two experiments in which a rabbit and a dog were poisoned by arsenic acid. The animals were really dead; but the local lesions produced by the poison did not appear as considerable as those produced by arsenious acid; this induced them to conclude that the action of arsenic acid was moderate in comparison with that exerted by arsenious acid. M. Schroff does not think that this conclusion is warranted even by the experiments of the authors whom we have quoted. He observes, with reason, that the intensity of the poisonous action of a substance, is not necessarily in proportion to the local lesions which it produces. In this respect he recalls to mind the numerous cases of poisoning with arsenious acid which were speedily followed by death, and in which it was impossible to find the least alteration in the stomach and intestines.

Moreover, the author has endeavored to solve this question by fresh experiments. Three rabbits were poisoned by arsenic acid dissolved in twenty times its weight of water. The first, to which was administered 0·342 gr. of this acid, died at the end of twelve hours; the second died in fourteen hours after taking 0·200 gr., finally the third sunk in about thirty-six hours, after having taken 0·070 gr. of arsenic acid.

Two rabbits to which had been administered 0·070 gr. of arsenious acid dissolved in one-hundred times its weight of water, sunk, the first in seven hours, the second in about eight hours. The author remarks that this dose of arsenious acid does not exactly correspond to the same weight of arsenic acid, the latter is in fact hygroscopic and can only with difficulty be obtained in the dry state. The amount of arsenious acid which corresponds to 0·070 gr. of the arsenic acid which he employed is estimated by the author at 0·06 gr. He therefore admistered to a rabbit 0·06 gr. of arsenious acid moistened with a little water and made into a paste with gum arabic. The animal was observed for fifteen days and did not appear affected by the poison. At the end of this time it received a fresh dose of 0·07 gr. administered in the same manner, and sunk in three days.

The author concludes from these experiments that arsenic acid is not a less energetic poison than arsenious acid, although in the majority of cases it produces less intense local derangements. As to arsenious acid itself, the intensity of its action is very variable, but it may be observed that it is greater in proportion as the acid is better dissolved. When administered in broth it produces a very intense local action,

but which may be limited to a very small surface of the organ, whereas it produces a less intense general inflammation of the stomach and a more rapid death, *ceteris paribus*, when dissolved in a hundred times its weight of distilled water.

Neues Reportorium für Pharmacie.

PUBLIC HEALTH.

ANALYSIS *of the* WATER *of the* RIVER TYNE, *at* NEWCASTLE.

By ROBERT DUNDAS THOMSON, M.D., F.R.S.E., &c.

THE following is a Report on a portion of the water of the River Tyne, which had been sent to Dr. Thomson for examination. No one who has seen the Tyne can help agreeing with Dr. Thomson's opinion of its water as a beverage.

"*Report on Water taken from the Tyne at Elswick, October 17th, 1853, two or three hours after low-water.*

"The water, when drawn from the jar in which it was conveyed from Newcastle to London, was turbid, from the presence of a considerable quantity of matter, mechanically diffused in the form of flocks throughout the mass of fluid. When the foreign matter was allowed to subside, the water presented a brownish aspect, similar to the color possessed by water which, after a fall of rain, has passed over a soil impregnated with peaty or vegetable substances. On separating the mechanically diffused flocks, which, on agitation, have imparted a muddiness to the water, they were found to consist of organised forms, similar to the appearances presented by infusorial animals, or diatomaceous beings, which have been claimed as belonging, according to some, to the animal, and, according to others, to the vegetable, world. These animal or vegetable beings were infested with large numbers of very distinct animalculæ or vibrios, which were sporting about with immense activity, and courting the shade of the living flocky masses. I found the weight of this foreign matter to amount in the gallon to 4·502 grains; of this only ·545 grains was matter destructible by heat. The remainder consisted principally of silica, and still retained an organised form, corresponding apparently with the shields of the diatomaceæ previously mentioned. These possessed a reddish hue from the presence of a minute quantity of oxide of iron, which either entered into the composition of the organised forms, or was derived from a minute portion of sand which may have been diffused through the water. The presence of those living forms in water used by the inhabitants of a city, it appears to me, cannot be viewed as otherwise than highly objectionable in a sanitary point of view, acquainted as we now are with the influence which the lower forms of animal and vegetable life exercise in nature. In addition to these forms presented by the organic matter described, I found no less than 2·68 grains per gallon of vegetable matter dissolved or finely diffused throughout the water, and which was in a condition approximate to that of organisation, since it contained a considerable amount of siliceous inorganic matter.

"The total quantity of matter, therefore, observed, from organised sources, which I found to exist in the water, amounted to no less than 7·172 grains per gallon. This must be viewed as a large amount when we take into consideration the fact that the total solid constituents in the gallon were 15·662 grains. The organic matter obtained in the analysis thus approximated to one-half of the foreign ingredients.

"I have also been able to obtain indications of the presence of nitric acid in the water; from which it is rendered probable that human excretions are mingled with the river, a circumstance which I have endeavored to prove in reference to the well-waters of Glasgow, and this renders river-water, and still more strongly the well-waters sunk in towns, in the highest degree objectionable for domestic use in a sanitary point of view. The time is fast approaching when the inhabitants of

cities, warned by an increasing knowledge of the science of health, will no longer consent to be supplied with river-water teeming with organised life, or with well-water drawn from pits communicating with the common sewers of cities, from which they are thoroughly impregnated with human excretions in solution.

"The river water of Newcastle I consider, both from my acquaintance with the locality and from the present analysis, to be a most improper source of supply to the inhabitants of that city. The constituents of the water in the imperial gallon are as follows:—

	Grs.
"Siliceous shields of diatomaceæ . . .	3·957
Organic matter accompanying the preceding . .	0·545
Organic matter from the filtered water . . .	2·680
Carbonate of lime	1·180
Siliceous matter	trace
Sulphate of lime	3·330
Sulphate of magnesia	0·190
Chloride of magnesium } Chloride of sodium }	3·780
	15·662"

BIBLIOGRAPHY.

A HAND-BOOK OF INORGANIC CHEMISTRY; BEING A NEW AND GREATLY ENLARGED EDITION OF THE "OUTLINES OF INORGANIC CHEMISTRY;" FOR THE USE OF STUDENTS. By WILLIAM GREGORY, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh. Third Edition. London: Walton and Maberly, 1853. pp. 291.

THE additions made in this edition of Dr. Gregory's well-known and useful little work, consist of such matters as the progress of the science has rendered necessary to be included in it. Some observations on the effects of heat on matter; an explanation of the nature of the galvanic current and electrical decomposition; and an account of the physical properties of gases and vapors have also been added. More wood-cuts are also given.

This work is so well known, that a lengthy review of it is quite unnecessary. Suffice it therefore to say, that the language in which it conveys its valuable information is so simple, as to suit the capacity of beginners in chemistry, and, at the same time, not puerile in style.

To those who desire to obtain a knowledge of inorganic chemistry, this hand-book will be found of great value; it is calculated to excite a desire to pursue the study, and after having carefully perused it, the student will be enabled and encouraged to venture upon the larger and more complete treatises, and to enter into the applications of the science.

We wish that Dr. Gregory had given the synonymes in every case, instead of only in some.

NOTES AND QUERIES.

QUERY.—Having some quantity of Nitrate of Silver Solution, which has become dirty and unfit for use, would the Editor of Photography inform me of the means of recovering the silver in a metallic state?—TYRO.

ANSWER.—Silver may be recovered from the nitrate by immersing in its solution a piece of bright copper—a penny-piece will do. When no more metal precipitates, wash the pulverulent mass by decantation. The residue is silver. Digesting with ammonia will remove any trace of copper that may be present. Finally, well washing with water will leave the metal pure, to be reconverted into nitrate if desired.

Another mode is to precipitate the silver by means of common salt. Wash the precipitate by decantation, and place it in contact with metallic zinc, adding enough sulphuric acid to slightly acidulate the mass. The silver will be reduced in a few hours to the metallic state. Wash well and re-dissolve in nitric acid, and evaporate to dryness. As the resulting nitrate of silver may contain a trace of zinc the first process will probably yield the best result. T. A. M.

A SUBSCRIBER wishes to be informed how ostrich feathers are bleached; if by sulphur, what preliminary operations are necessary, and how is the process conducted.

THE IRISH AMELIORATION SOCIETY.

WE have received from the Secretary of the Irish Amelioration Society, the following, intended as a reply to Mr. Rogers' letter inserted in our last. It seems that the process of manufacture patented by Mr. Rogers, has been in no way blameable for the failure of the Society's operations, and that perhaps, is all that our correspondent C. E. desires to know.

We have been obliged to expunge from the Secretary's letter a portion which appeared irrelevant and unnecessary.

We wish that some one would inform us how it is, if Mr. Rogers was the cause of the failure, that the Society has not, since his dismissal, succeeded to such an extent as to shew that he alone stood in the way of success.

THE IRISH AMELIORATION SOCIETY.
London, 23 *November*, 1853,
13, *Gresham Street*.

To the Editors of "THE CHEMIST."

GENTLEMEN,

With reference to the enquiry contained in No. I. of "THE CHEMIST" (New Series), as to "What were the points in which the Irish Amelioration Society's operations for the Manufacture of Peat Charcoal have failed," and Mr. Jasper Rogers' reply thereto, to which the attention of some of the Members of the Board of Directors of that Society has been called, through your courtesy,—they have but few, and very brief observations to make.

The reply to the enquiry is simply this,—That the operations of the Society have failed in consequence of disagreements with Mr. Rogers, the patentee, and likewise the working Engineer and Chief Superintendent of the Company in Ireland, of so serious and embarrassing a nature, as to paralyse the whole machinery by which the affairs of the Company and the manufacture of the peat were managed; and to compel the Directors to resort to the extreme measure of suspending Mr. Rogers, and calling upon the Company to dismiss him from his office.

At the same time that the Directors express their decided opinion that the failure has occurred through want of regularity, and the intractable disposition of Mr. Rogers, they are quite ready to admit his ingenuity; and they likewise give

him full credit for his early exertions, and concur with him in opinion, that the manufacture of peat charcoal may be effected with most important advantages both to the Irish proprietors and laborers, and so as to form a very valuable agent as a deodoriser, and in promoting agricultural purposes, more particularly when combined with sewage matter.

But in order to command these advantages, the business must be conducted with a close attention and precision, and a spirit of subordination and concert, which the Directors have not been so fortunate as to meet with in Mr. Jasper Rogers.

I am, Gentleman,

Your obedient servant,

WM. FOLEY, *Secretary.*

By order of the Court of Directors.

"PULVIS FERRI, *or* QUEVENNE'S IRON."

Our attention has been called to a case, the particulars of which we lay before the public with the view of doing justice to a manufacturer of pure chemicals, who is well known to our readers, and the character of one of whose preparations has been assailed, evidently not only without reason, but absolutely in defiance of the fact, that the preparation in question, as has since been proved, was seven per cent. superior to that manufactured by the very man who pronounced a strong opinion of its worthlessness. Doubtless the calumny has been spread far and wide among Pharmaceutical Chemists; we therefore feel justified in lending our columns to a short statement of the facts, from which our readers will see that we are only doing an act of justice.

The preparation called "Pulvis Ferri," D. Ph., was introduced on the continent by MM. Quevenne and Miquelard, under the name of "fer réduit," and ordered in the Dublin Pharmacopœia to be made by reducing the peroxide of iron by means of hydrogen gas, with certain precautions.

Mr. Heathfield, of Princes-square, Finsbury, having sent some of this preparation of his own manufacture, in the state of a nearly black powder, to Mr. Robertson of George-street, Edinburgh, one of the first and most careful dispensers in that city, was apprised a short time since by Mr. Robertson, that Mr. Hamilton of Dundee, had expressed much disappointment at finding that the preparation which he had been dispensing, and which had been procured from Messrs. Duncan, Flockhart & Co., of Edinburgh, being of a slate color, had produced a totally different appearance in a mixture from what the patient had had from Mr. Robertson. Messrs. Duncan and Co., on being applied to by Mr. Hamilton, informed him that they had been offered some of "Quevennes Iron" at one-third of the price charged by Mr. Morson, whose price was 4s. 6d. per oz.; and some of that cheaper preparation being sent to Mr. Morson, he had stated that it was nothing more than magnetic oxide of iron, worth 6d. an ounce. Mr. Robertson then sent, for analysis, to Dr. George Wilson, Professor of Chemistry, two samples of the "Quevennes Iron," one prepared by Mr. Heathfield, and the other by Mr. Morson. Dr. Wilson analysed these samples, and found that of Mr. Heathfield to be seven per cent. purer than Mr. Morson's.

Now, either Mr. Morson was ignorant of the quality of the article of which he expressed an opinion, or he most unfairly condemned the preparation of another manufacturer.

Great credit is due to Mr. Robertson, for the determined and manly way in which he persevered in sifting this matter, and too much praise can scarcely be bestowed on him for this, or for his honorable anxiety as to the purity of the medicines he dispenses

We have every confidence in Mr. Heathfield's scrupulousness as to the quality of his manufactures. It appears to us that an attempt has been made in this instance to injure the reputation of his preparations; and we are glad to find, that on examination, his "Quevennes Iron" turned out to be better than that with which it was compared.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the Poisonous Qualities *of* Chromium. By Wm. Herapath, F.C.S.

In "The Chemist" for November (p. 114), there was a paper by M. Jaillard on the poisonous properties of bichromate of potassa, in which the author calls the attention of toxicologists to the action of the compounds of chromium on the animal system. From the experiments described in that memoir, and from what has already appeared in the few cases detailed in established works, it is evident that chromium must be included amongst the most subtle and virulent poisons; and as it has been reported that bichromate of potassa has been, since difficulties have been thrown in the way of the purchasers of arsenic, resorted to as a human poison by the inhabitants of the cotton-printing districts, it becomes necessary that everything relating to its action should be known; I, therefore, send you the particulars of a case in which I have lately had to investigate the symptoms produced by small, but repeated, doses of that metal upon animals.

A short time ago a respectable farmer having had his cattle affected in a peculiar way, applied to me, and stated, that as the animals of his neighbors, bordering a brook of water, had been similarly affected, he naturally ascribed the injury to the water.

The symptoms described were as follows:—

The stomach and intestinal canal were strongly acted on. Great thirst; attempts to be sick (which in cows is indicated by restless motions of the mouth and frothing of the lips), diarrhœa, scouring, the fœces being black, and exceedingly offensive; loss of appetite; shortness of breath; secretion of milk reduced more than one-half; four cows, in the impregnated state, calved short of their time, and a mare's colt died in two minutes after birth; partial atrophy occurred, as all the animals lost flesh. But the best marked symptoms which corresponded with the action of chromium, were great lassitude and partial paralysis of the hinder extremities, and the eyes and mucous surfaces being covered with "matter."

Upon visiting the locality with my son, Mr. Thornton Herapath, I traced the stream up to a point where it received the wash from a soap-work, a sample of the spent leys from which, as it then ran, was taken, and again following the stream downward, several other bottles were filled. These leys, when subjected to analysis, were found to contain 7186·9 grs. of solid matter per gallon, amongst which there were 3·8 grs. of chromic acid, or 2·92 grs. of chromic oxide per gallon.

It was evident, therefore, that the soap-maker was not reproducing his chromic acid after bleaching palm oil, but allowing it to run to waste; and as the same matters, in an inferior degree, were traced down the brook, and as the symptoms were so nearly like those of chromium, and so different from those of spent ley, they must be attributed to the metal. As no death had occurred, the action of the poison upon the intestinal surfaces could not be ascertained.

On the ELECTRO-DEPOSITION *of* METALS.* BY J. B. HOCKIN.

Copper.—The solution I prefer for depositing this metal is formed by dissolving 1¼lb. of the purest commercial sulphate, in half a gallon of hot water, allowing the solution to settle, then adding to the clear liquid ⅛th of its volume of water, containing a wine-glassful of sulphuric acid.

The form of apparatus best suited, as regards constancy and uniformity of action, is that well known as the "Single cell battery," consisting of an exterior vessel, formed of any material incapable of being acted on by the acid metallic solution, and an interior cell of unglazed (and therefore porous) earthenware or porcelain. To procure this of the due degree of porosity is of the greatest importance, as, if it be too permeable, the liquids employed (solution of sulphate of copper in the exterior vessel, and water in the porous cell), will readily infiltrate each other; if, on the other hand, it be too close in texture—too great an impediment will be offered to the passage of the electric current. I may here remark, that the absorbent property indicated on applying the tongue to the *finer sort* of clay pipes, will give an approximative idea to those who wish to test this without putting the cell to the proof of actual experience.

The zinc employed need not be the best rolled zinc of commerce. I have obtained excellent results from the rough spelter fused with half an ounce of mercury to the pound, cast into slabs of half an inch thick, and of any required dimensions, carefully amalgamated on both sides previous to being employed. This amalgamation being repeated when the weight has been reduced by several days' work, serves perfectly to protect the plate from any sensible local action. A binding screw affixed to the zinc serves, at the same time, to suspend it in the porous cell, and to afford a medium of communication between it and the mould intended to be covered with the reduced copper. Contrary to the usual custom, I do *not advise* the employment of sulphuric acid in the *water contained in the porous* cell, except in the proportion of five drops to the pint; this is sufficient to commence the action of Electrolysis, which, once set up, causes a quantity of acid to permeate the porous vessel, and to be eliminated on the positive metal, equivalent to the oxide of zinc there generated, and requiring to be dissolved. I

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* Read before the Chemical Discussion Society, July 14th, 1853.

may here state (to render the process clear in a theoretical point of view), that I conceive the action of the battery here described to be as follows:—The result of placing a pair of dissimilar metals, connected by a conducting medium, in acidulated water, is the decomposition of the water (HO). O is eliminated on the positive metal, which combines therewith; this oxide, meeting with the acid, forms a salt of the metal. H is eliminated on the negative element; if it there meets with a metallic salt it abstracts its oxygen (to reproduce water therewith), and precipitates the metal in question, in a more or less perfectly reguline form. For the laws regulating this latter effect, I would refer to Mr. Smee's valuable work on Electro-Metallurgy.

The moulds I prefer for all objects not undercut, are of copper obtained by the Electrotype process, from plaster or other originals. Plaster medallions are to be well dried at a moderate heat, in an oven, saturated with hot wax—carefully avoiding any obliteration of the fine lines by the fluid material covering the surface—black-leaded, and covered with Electrotype copper; on removing the latter, the surface is always more or less rough, but (the subject being *all in intaglio*) it may be brought to a very fine polish by the use of certain soft stones, obtainable at most of the tool shops. The mould being now covered with varnish at the back, and black-leaded on the front surface (to prevent adhesion between it and the metal to be deposited), is capable of affording an almost unlimited number of fac-similes of the original.

Where but one copy is desired, a mould may be taken in wax, gutta percha, or a compound thereof with wax, made by boiling "masticated gutta," with one-fourth wax, fusible metal, &c.

The mode of taking a wax mould by which I have best succeeded, is the following: place the plaster in a flat hot-water bath, pour in boiling water until it reaches half-way up the sides. then allow it to remain until well heated, take it out, encircle the rim with a band of lead foil, so as to form a cup for receiving the wax composition.*

This latter having been melted at about 212°, is poured on the mould gently tilted, and when sufficient thickness has accumulated, it is placed flat, and allowed to cool. In the copying of metallic surfaces I have found, that, if they possess but a slight amount of polish, adhesion to the original by the deposited metal is best avoided by black-leading the former. Highly polished and burnished surfaces, on being freed from mechanical and chemical impurities, need no further preparation, *e. g.:* a daguerreotype picture may be copied most accurately by being previously washed in a weak solution of cyanide of potassium.

Coppering, *i.e.*, covering the baser metals with a coating of copper, is best effected by a solution of cyanide of copper in cyanide of potassium.

* Wax candle ends one pound, litharge one ounce, fused together for half an hour, allowed to repose, and the clear liquid poured off, produce an imperfect lead soap; emplastrum of the pharmaceutist, composition candles, stearine, tallow, &c., may all be used. The addition of the lead appears to produce hardness, and renders the mass much less crystalline, and diminishes its contractility on passing from the fluid to the solid condition.

It is remarkable that this operation does not succeed well without a two pair battery arranged for intensity; and, that, in opposition to the usual law, good reguline metal is not obtained unless hydrogen escape freely from the pole which receives the deposit.

Iron and steel should be cleaned previously by immersion in somewhat dilute sulphuric acid, and kept in contact with a piece of zinc. These, as well as tin and compounds thereof, are best coppered previous to being silvered; unless this be done, the deposited silver has but little attachment to them.

Silver Solution is formed from cyanide of silver dissolved in a *slight excess* of cyanide of potassium in the proportion of 1 oz. fine silver to two pints of liquid; it is readily decomposed in the cold by a single Smee's battery.

Gold Solution, from the terchloride of gold, to which pure cyanide of potassium is added until decolorised, then 1–4th more cyanide added. 1 drachm terchloride gold to a half pint of liquid. 1 element reduces it if kept at 212°.

Great difficulty has existed hitherto in metallising bodies, which are too frail to bear blackleading with the brush. The process, by impregnation with phosphorus dissolved in bisulphide of carbon and subsequent immersion in nitrate of silver solution, does not give uniform results. I have obtained results which promise well from the employment of iodised collodion, as in the ordinary photographic process on glass, viz., a flower, for example, is plunged into iodised collodion, dried in air for a minute, then immersed in nitrate of silver solution exposed to light for a few seconds, and the silver deposited in the metallic state by a bath of protosulphate of iron acidulated with nitric acid. On placing this in a *nearly neutral* solution of sulphate of copper, and in contact with a battery, the metal speedily extends over the silvered surface.

Observations on the PREVENTION *and* DESTRUCTION *of the* GRAPE VINE MILDEW. By THORNTON J. HERAPATH.

IN accordance with the promise I made to you a few weeks back, I now hasten to forward for publication a brief account of the experiments I have made during the past autumn on the prevention and destruction of the grape fungus, or *Oidium Tuckeri*. I shall not attempt at the present moment to bring forward any speculations or theory as to the cause of the blight, but will confine myself entirely to a plain statement of facts, and the results of my own investigations. The substances with which I have experimented, are—

1st. A solution of the super-sulphides of calcium, made by boiling together quick-lime and an excess of sulphur, and afterwards diluting with water to the necessary degree.

2nd. A weak solution of the super-sulphide of sodium.

3rd. A solution of the hypochlorite of lime, prepared by digesting the common bleaching powder of the shops in water; the powder being used in the proportion of about three ounces to the gallon of water.

4th. A sulphuretted alkaline soap, made by a process to be hereafter described.

Solutions Nos. 1, 2, and 3, effectually stayed the progress of the disease, but their application was found in practice to be attended with one great inconvenience; that is to say, the solutions did not attach themselves readily to the surface of the fruit; the thin film of waxy matter with which the latter was covered being found to exert, so to speak, a repellent action towards the fluid, which flowed off again without wetting it. This difficulty was only overcome at the cost of much time and trouble. With the alkaline soaps, however, the case was very different. This solution was easily applied, and spread itself readily over every part of the fruit, and moreover proved more destructive to the fungi than any of the other substances before alluded to. I am hence induced to give the preference to it. The mode by which I prepare it is as follows: I take equal parts by weight of common yellow and soft soap, dissolve them in boiling-water in a saucepan over a slow fire, taking care to stir the mixture occasionally with a stick, and then allow it to cool. To this saponaceous liquid I then add milk of lime, in small quantities at a time, until curdling is no longer produced. The clots of stearate and margarate of lime thus formed are next removed by straining the liquid through a piece of *coarse* towelling, and the clear solution which passes through is then boiled for a short time with a little flower of sulphur; the latter being used at the rate of a quarter of an ounce to the four pounds of the mixed soaps. So soon as the sulphur is completely dissolved, a sufficient quantity of water is added, and the mixture is ready for use. One pound of the mixed soaps will make about four gallons of the preservative fluid. This may be distributed over the diseased vines either by means of a hand syringe or an ordinary garden-pump.

It is sometimes preferable to dip the grapes by hand into a basinful of the composition, and paint over the stems and young shoots affected with a common brush. If under glass, one application suffices; but I have not yet had an opportunity of ascertaining if this is the case with out-of-door vines. Before applying the preparation to the great bulk of the grapes, it is advisable to try one or two experiments with a separate bunch, in order to ascertain if the composition is too strong. When such is found to be the case, of course all that is necessary is to add more water.

In conclusion, I may observe, that most of my experiments were carried on in the green-houses of Mr. Green, of Cotham New Road; and I cannot allow this opportunity to pass without thanking that gentleman for the kindness he exhibited towards me in affording me every facility for carrying out my operations. My acknowledgments are also due to Mr. Nelson, nurseryman, of Horfield Road, to whose skill and attention I have great pleasure in bearing testimony.

CHEMICAL TABLES. COMPILED BY THORNTON J. HERAPATH.

No. I.—DENSITIES OR SPECIFIC GRAVITIES.

(Continued from page 141.)

When an * is attached to a name, it must be understood that the *preceding* numbers have been merely copied from some standard work, and are not the result of the Author's experiments.

Substances.	Density at 60° F.	Authority.
Charcoal, oak	0·332	Kirwan
„ pine	0·280	„
Chiastolite	2·04 to 3·09	Bunsen.
Chiolite	2·72	Hermann.
Chloral	1·502	Brande.*
„ mesitic	1·330	Dumas.
Chloropal, earthy	1·727 to 1·870	„
Chlorophæite	2·020	McCulloch.
Chlorophane	3·094	Haüy.
„	3·1400	Haidinger.
„	3·177	Th. Herapath.
Cholera, vomit.	1·012 to 1·0169	„
„ dejections	1·016 to 1·0185	„
Chondrodite	3·180	D'Ohsson.
„	3·199	Haidinger.
„	3·157 to 3·228	Seybert.
Christianite	2·201	Descloizeaux.
Chrome-iron	4·321	Thomson.*
„	4·552	Th. Herapath.
Chromium, oxide of	4·909 to 5·21	
„ sesqui-sulphide of	4·092 at 39°·1	Playfair & Joule.
Chromic acid	2·676	„
Chrysoberyl	3·508 to 3·597	Seybert.
„	3·711 to 3·733	Thomson.
„	3·754	Haidinger.
Chrysolite, Brazilian	2·692 to 3·340	Cresy.
„ Oriental	3·410	Haidinger.
„	3·362	Stromeyer.
Chrysophrase	2·608	Hoffman.
„	3·250	Klaproth.
Chrysotil	2·219	Delesse.
Chyle	1·0215 to 1·022	Marcet.
Cider	0·9876	Ure.*
„	1·018	Moseley*.
„ Devon.	1·0002	Th. Herapath.
Cimolete	2·000	Cresy.*
Cinnabar, native	8·098	Thomson.
„ crystals	10·218	Cresy.*
Cinnamene, or Styrole	0·928	Kopp.
Cinnamic acid	1·246	„
Cinnamole	0·880	Marchand.
Citraconic acid	1·247	„
Citric acid	1·0345	Gray.*
„	1·617	Ure.*
Clausthalite	7·187	H. Rose.
Clay, potters'	1·800 to 2·085	Kirwan.
„ common	1·919	Belidor.
Clinker, English	1·653	Tredgold.
„ Dutch	1·482	„
Clink-stone	2·575 to 2·620	Cresy.*
Cluthalite	2·166	Thomson.
Coke	0·744	Kirwan.
Coal, Bovey	1·129	Vaux.
„ Caking	1·269	Thomson.
„ „	1·274 to 1·280	Richardson.
„ cannel	1·265	Thomson.
„ „	1·276	Vaux.
„ „	1·318 to 1·319	Richardson.
„ Kilkenny	1·526	Kirwan.
„ Newcastle	1·264 to 1·288	Vaux.
„ Oregon	1·578	„
„ Splint	1·29	Thomson.

Substances.	Density at 60° F.	Authority.
Coal, Splint	1·302 to 1·307	Richardson.
Coal-gas, charcoal	1·800	Brande.*
Coal-naphtha	0·82 to 0·86	,,
Cobalt-glance	6·298	Thomson.
,, ochre	2·200	Breithaupt.
,, pt.-oxide	5·597 to 5·75	Playfair and Joule.
,, peroxide	4·814	,,
,, ,,	5·322	W. Herapath
,, proto-peroxide	5·833	Brunner.
,, ,,	6·090	Berzelius.
,, ,,	6·296	Haüy.
,, pyrites	5·450	Middleton.
,, radiated white	6·5 to 7·131	Varrentrapp.
,, bisulphide	4·269	Playfair and Joule.
,, sesqui-sulphide	4·049	,,
,, white ore of,	6·466	,,
Cobaltic galena	8·440	Du Menil.
Cochineal	1·250	Ure.*
Colophanite	3·610	Richardson.
,,	3·871 to 3·965	Thomson.*
Colophen	0·940	Deville.
Columbite, America	5·323	H. Rose.
,, Bodenmais	5·704	,,
,, Finland	7·236 to 7·655	Berzelius.
,, Tamela	7·264	Nördenskiold.
,, ,,	7·64 to 7·651	Damour.
,, ,,	7·963	Ekeberg.
Commingtonite	3·2014	Thomson.*
Comptonite	2·427	,,
Condurrite	4·20 to 4·29	Blyth.
,,	5·2054	Phillips.
Coniine	0·878	Blyth.
,,	0·890	Geiger.
Copper, blue ore of,	3·831	Thomson.*
,, dark grey	4·7 to 4·9	H. Rose.
,, glance	5·702	Thomson.*
,, grey ore of,	4·798 to 5·104	Haidinger.

Substances.	Density at 60° F.	Authority.
Copper, liver-colored ore of,	5·003	Thomson.*
,, mica	2·548	Bournon.
,, native	6·125	John.
,, ,,	7·6 to 7·8	Cresy.*
,, ,,	8·508	(?)
,, ruby ore	5·8 to 6·0	Brande.*
,, prot.-oxide	5·9 at 39°	Playfair and Joule.
,, ,,	6·13	Boullay.
,, ,,	6·40	Brande.
,, ,,	6·401	W. Herapath.
,, ,,	6·822	Filhol.
,, sub-oxide	5·746	Playfair and Joule.
,, ,,	5·751	Karsten.
,, pyrites	4·160	Thomson.*
,, schaum	3·098	,,
Copper-salts, &c., acetate	1·780	Gray.
,, chloride	3·054	Playfair and Joule.
,, dichl.	3·376	,,
,, nitrate	2·047	,,
,, sub-nitrate	2·765	,,
,, sulphate crys.	2·242 to 2·2901	,, and Filhol.
,, ,, anhyd.	3·53	Filhol.
,, ,, sat. sol.	1·15 at 42°	Cresy.*
,, sub-sulphate	3·082	Playfair and Joule.
,, sulphide	3·80	Wächtner.
,, sub-sulphide	5·695	Mohs.
Copper ore, lenticular	2·78	Thomson.
,, ,,	2·882	Bournon.
,, ,,	2·926	Haidinger.
Coprolites:—L. Regis	2·644 to 2·700	Th. Herapath.
Coprolites, pseudo:—Suffolk	2·7216 to 2·85	,,
,, powdered	2·961	,,
Coracite	4·378	Le Conte.
Coral, white	2·857	Moseley.*
,, red	2·570	,,
Corundellite	3·000	Silliman.
Corundum	3·710	Moseley.*

Substances.	Density at 60° F.	Authority.
Corundum, Indian	3·71 to 3·875	Cresy.*
,, Chinese	3·981	,,
Cotton	1·470	?
Cotumnite	1·897	Thomson.*
Cream	1·0244	Berzelius.
Creasote, pure.	1·037	Reichenbach.
,,	1·060	Christison.
Crocoisite	6·100	Vauquelin.
Cronstedite	3·348	Thomson.*
Crucite	3·579 to 3·8095.	,,
Cryolite	2·957	Cresy.*
,,	2·949	Thomson.
Cryptolite	4·600	Wöhler.
Cumidine	0·9526	Nicholson.
Cyanite	3·517 to 3·622	Cresy.*
Cyanol	1·028 at 68°	Hofman.
Cyanogen liq.	0·900	Brande.*
Cymol	0·857	Noad.
Cyprine	3·2278	Richardson.
DATHOLITE	2·99 to 3·346	
Davidsonite	2·362	Thomson.*
Dechenite	5·810	Bergmann.
Delvauxene	1·850	Dumont.
Deweylite	2·247	Thomson
Dialogite	3·592	Haidinger.
Diamond, Brazilian	3·444 to 3·55	Moseley.
,, Oriental	3·521 to 3·55	,,
Diaspore	3·4324	Haüy.
Diopside	3·2374	Tonnellier.
Dipyre	2·63 to 2·84	Cresy.*
Dolomite	2·815 to 5·284	Thomson.*
,, ,,	2·71 to 2·85	R. D. Thomson.
Dragon's Blood	1·196	Ure.*
,, ,,	1·204	Moseley.*
Dreelite	3·300	Dufresnoy.
Dysclasite	2·280	L. Gmelin.
Dysclasite	2·362	Thomson.*
Dyslnite	4·551	,,
EARTH, common	1·520 to 1·984	Tredgold.
,, loamy	2·016	,,
,, loose or sandy	1·520	,,
Earth (the earth, our planet)	4·715	Schehallier.
,, ,,	4·750	Hutton.
,, ,,	4·950	Carlini.
,, ,,	5·448	Cavendish.
,, ,,	5·507 to 5·847	Baily.
Edwardsite	4·2 to 4·6	Shepard.
Elaeolite	2·546 to 2·618	Hoffman.
Elæolite	2·600 to 2·617	Scheerer.
Elathine	1—	Zeise.
Embolite	5·806	Plattner.
Emerald	2·58	Thomson.
,, ,,	2·732	Haidinger.
,, Brazilian	3·15 ?	Cresy.*
,, Pseudo	2·701	,,
Emery	4·000	Tennant.
,,	3·74 to 4·31	L. Smith.
Ephesite	3·15 to 3·20	,,
Epidote	3·425	Haidinger.
,,	3·436 to 3·440	Thomson.
,,	3·46	Descotils.
Epistilbite	2·25	G. Rose.
Epsomite	1·751	Thomson.
Erinite, of Thomson	2·040	Thomson.
,, of Haidinger	4·043	L. Gmelin.
Erlanite	3·0 to 3·1	Gmelin.
Erythrite	2·541	Thomson.
Esmarkite	2·709	Erdmann.
Essonite, or Cinnamon stone	2·600	Cresy.*
,,	3·631	Lehunt.
Euclase	2·907	Lowry.
,,	3·0625	Haüy.

Substances.	Density at 60° F.	Authority.
Euclase	3·098	Haidinger.
„	3·300	Moseley.*
Eudnophite	2·270	Weibye.
Eudyalite	2·9035	Stromeyer.
„	2·9036	Thomson.
Euphyllite	2·9630	Silliman.
Eupion	0·655	Thomson.
„	0·740	Brande.*
Euxenite	4·600	Scheerer.
FAHLUNITE	2·620 to 2·790	Wachmeister.
Fatty acids	1 —	
Fat or tallow	0·936	Ure.*
Fayalite	4·138	Gmelin.
Felsite	2·545	Kerndt.
Felspar, potash	2·395 to 2·574	H. Rose.
Fergusonite	5·8 to 5·83	
Fibroferrite	2·500	Prideaux.
Fibrolite	3·214	Bournon.
Firestone	1·800	Tredgold.*
Flax	1·500	Ure.
Flesh	0·923 to 0·934 (?)	Moseley.
Flint	2·58 to 2·63	Thomson.
Formic acid, bihyd.	1·1168	Ure.
„ monohyd.	1·2353 at 53°·5	Liebig.
Fornacite	2·85 to 3·11	Riensch.
Fousel oil	0·812	Graham.
„	0·8137	Otto.
Franklinite	4·257	Thomson.*
„	4·870	Berthier.
Fuchsite	2·860	Schaffheutl.
Fuller's-earth	1·82 to 2·19	Ure.*
„	2·445 to 2·517	Thomson.*
Furfurol	1·168	Fownes.
GABRONITE	2·740	Ure.*
„	3·000	Thomson.*

Substances.	Density at 60° F.	Authority.
Gadolinite	4·028 to 4·030	Rinmann.
„	4·0497	Haüy.
„	4·223	Geiger.
„	4.23	Gmelin.
Gahnite	4·238	Haidinger.
Galena	7·536 to 7·652	Thomson.
Galligenstein	2·036	Haidinger.
Garnet, common	3·57 to 3·68	Cresy.*
„ „	3·90 to 4·26	Thomson.*
„ „	3·70	Moseley.*
„ brown	2·829	Thomson.
„ black	3·157 to 3·73	„
„ green	3·372 to 3·640	Klaproth.
„ precious	4·230	Moseley.*
„ „	4·208 to 4·357	Thomson.
„ „	4·085 to 4·352	Cresy.*
Gases and vapors (*See* Note)		
ACETIC ACID	2·7700	Dumas.
„	2·7565	T. H. cal.
Acetone	2·019	Dumas.
„	1·9984	T. H. cal.
Acroleïne	1·897	Redtenbacher.
Æther, acetic	3·067	Dumas.
„ „	3·0321	T. H. cal.
„ methylic	1·617	Dumas.
„ „	1.5849	T. H. cal.
„ nitrous	2·626	Dumas.
„ „	2·5842	T. H. cal.
Æthyl	2·046	Frankland.
„	1·9995	T. H. cal.
„ oxide	2·586	Dumas.
„ „	2·5497	T. H. cal.
„ bromide	3·9750	Dumas.
„ „	3·6874	T. H. cal.
„ chloride	2·2190	Thenard.
„ „	2·2396	T. H. cal.

NOTE.—Atmospheric air, at 60° F. and 30 inches. Bar. pressure, equal to 1.

Substances.	Density at 60° F.	Authority.
Gases and Vapyors.		
Æthyl, iodide	5·4750	Gay Lussac.
„ „	5·3407	T. H. cal.
Alcarsin	7·5550	Bunsen.
„	7·5803	T. H. cal.
Alcohol	1·6130	Gay Lussac.
„	1·5599	T. H. cal.
Aldehyde	1·5320	Liebig.
„	1·5161	T. H. cal.
Ammonia	0·5901	Davy.
„	0·5967	Biot and Arago.
„	0·5857	T. H. cal.
Ammonium, chloride	0·8900	Bineau.
„ „	0·9130	T. H. cal.
Amyl	4·899	Frankland.
„	4·8955	T. H. cal.
„ oxide of	5·490	Cahours.
„ „	5·378	T. H. cal.
Amylic alcohol	3·147	Dumas.
	3·137	Apjohn.
	3·043	T. H. cal.
Antimony	17·887	Gmelin, cal.
„ terchl.	7·800	Marchand.
Arsenic	10·600	Mitscherlich.
„	10·4747	T. H. cal.
„ terchl.	6·3006	Dumas.
„ „	6·3399	T. H. cal.
„ teriodide	16·100	Mitscherlich.
„ „	15·6432	T. H. cal.
Arsenious acid	13·8500	Mitscherlich.
„	13·7825	T. H. cal.
Arseniuretted hydrogen	2·6950	Dumas.
„	2·8254	T. H. cal.
Benzoic Acid	4·270	Dumas.
„	4·2036	T. H. cal.
Boron	0·7487	Gmelin, cal.

Substances.	Density at 60° F.	Authority.
Gases and Vapors.		
Boron	0·7580	T. H. cal.
„ fluoride	2·3100	Dumas.
„ „	2·3709	J. Davy.
„ „	2·2208	T. H. cal.
Bromine	5·540	Dumas.
„	5·3751	T. H. cal.
Bromoboracic gas	8·6443	Poggiale.
„	8·4643	„ cal.
Bromoform	8·6300	Cahours.
„	8·7847	Thomson.
Butyric acid	5·500	Pelouze.
Cacodyl	7·101	Bunsen.
Camphene	4·763	Dumas.
„	4·830	Cahours.
„	4·6860	T. H. cal.
Camphor	5·468	Dumas.
„	5·2373	T. H. cal.
Caoutchine	2·000	Dalton.
Capric acid	5·980	(?)
Carbon	0·4215	Graham, cal.
„	0·4160	Gmelin, cal.
„	0·41375	T. H. cal.
„ bromide	2·436	Poselger.
„ „	2·3660	T. H. cal.
„ chloride	5·820	Regnault.
„ „	5·7886	T. H. cal.
„ bisulphide	2·6690	Mitscherlich.
„ „	2·6447	Gay Lussac.
„ „	2·6380	Couerbe.
„ „	2·6124	T. H. cal.
Carbonic acid	1·4994	Lavoisier.
„	1·5191	Buff.
„	1·5196	Biot and Arago.
„	1·521	Lieb. & Redtenbach
„	1·524	Allen and Pepys.

Substances.	Density at 60° F.	Authority.
Gases and Vapors.		
Carbonic acid	1·5245	Berzelius & Dulong
"	1·5926	Saussure.
"	1·51607	T. H. cal.
" oxide	0·9400	Dalton.
"	0·9678	Cruickshanks.
"	0·9698	Thomson.
"	0·9727	Dulong & Berzelius
"	0·96477	T. H. cal.
Carburetted hydrogen	0·5590	H. Davy.
"	0·5513	T. H. cal.
Chloral	5·1736	Thomson.
"	5·1339	T. H. cal.
Chlorine	2·3400	Dalton.
"	2·395	H. Davy.
"	2·424	Gay-Lussac.
"	2·47	Thenard.
"	2·50	Thomson.
"	2·4808	T. H. cal.
" oxide	3·00	Gay Lussac.
" "	3·0321	T. H. cal.
" peroxide	2·360	H. Davy.
	2·3480	T. H. cal.
	3·4604	Thomson.
Chlorocarbonic acid gas	3·6808	J. Davy.
"	3·4456	T. H. cal.
Chlorochromic acid	5·900	Walter.
"	5·500	Dumas.
"	5·5117	Gmelin, cal.
Chlorocyanic or Phosgene gas.	2·1400	Dumas (?)
	2·1712	T. H. cal.
Chloroform	4·1999	Dumas.
"	4·1998	T. H. cal.
Coal-gas	0·407 to 0·659	Dr. Ure.
Cyanogen	1·8064	Gay-Lussac.
"	1·8190	Gmelin, cal.
"	1·7917	T. H. cal.

Substances.	Density at 60° F.	Authority.
Gases and Vapors.		
Fire-damp	0·6 to 0·966	Turner.
Fluosilicic acid	3·574	J. Davy.
"	3·600	Brande.*
Fluosilicic acid	3·5834	T. H. cal.
Fluorine	1·292	Graham, cal.
"	1·3093	T. H. cal.
HYDRIODIC ACID	4·3757	Thomson.
"	4·4429	Gay Lussac.
"	4·3759	T. H. cal.
Hydrobromic acid	1·9060	Bineau.
"	2·7220	T. H. cal.
Hydrochloric acid	1·2300	Dalton.
"	1·2470	Biot and Arago.
"	1·2555	Buff.
"	1·2780	Gay Lussac.
"	1·2748	T. H. cal.
Hydrocyanic acid	0·9476	Dumas.
"	0·9303	T. H. cal.
Hydrofluoric Acid	0·6829	Gmelin, cal.
"	0·6819	T. H. cal.
Hydroselenic acid	2·795	Bineau.
"	2·8425	T. H. cal.
Hydrosulphuric acid	1·106	Kirwan.
"	1·1791	Thomson.
"	1·1912	Gay, Lussac.
"	1·1967	H. Davy.
"	1·236	Thenard.
"	1·1715	T. H. cal.
Hydrogen	0·0688	Berzelius & Dulong
"	0·0693	Thomson.
"	0·0732	Biot and Arago.
"	0·0769	Lavoisier.
"	0·0920 (?)	Cavendish.
"	0·0692 to 0·0696	Dumas.
"	0·06891	T. H. cal.

On the AURORA.* By REUBEN PHILLIPS.

[Being a continuation of former papers. *See* "THE CHEMIST," for October, and November, 183.]

234. It has been shewn in my paper on the subject ("*Phil. Mag.*" S. 4, vol. 4, p. 126), that the atmosphere consists of two great spherical strata oppositely electrified. I also noticed the circumstance, that the positive electricity of the lower orb, would gradually rise in the atmosphere, owing to the mutual attraction between it and the negative orbicular stratum. The negative electricity must for a similar reason travel downwards. Negative electricity, generated in the air-channel of the thunder-cloud ("THE CHEMIST," Nov. 1853, p. 67), is no doubt carried by the ascending current to great elevations; further, as a general rule, the more violent the ascending wind, the more will it become electrified, and the greater the altitude it will ultimately attain. The negative orb is also neutral, as a whole, with regard to the exterior surface of the atmosphere; for, if an electrified body be enclosed within another equally and oppositely electrified, the arrangement is neutral to external objects (*See* Faraday's paper "Experimental Researches," vol 2, p. 279). Consequently, the induction of the negative orb is entirely exercised in the direction of the earth. Let us now assume that the thickness of the negative orb and its distance from the positive are such, that the vertical component of the electrical attraction of any particle varies approximately with the total electric attraction of that negative particle; then, the electricity of the superior portions of the negative orb, will, in virtue of its greater intensity, travel downwards faster than the electric charge of the inferior particles. Thus the electric charge becomes condensed. Also, since the air is more expanded and therefore generally rises in the warmer latitudes, the more elevated portions of the atmosphere go towards the poles. Let us now make another assumption, namely, that by the time the successive portions of the negative orb arrive at those latitudes favored with the aurora, that the above electrical condensation has so far proceeded, that the electricity, aided by the abounding mist of those regions, can force its way by the disruptive discharge either to the lower atmospheric strata, or to the earth, whence it would be readily dissipated into the positive orb; and the result would doubtless be an aurora. The evidence in favor of the foregoing assumptions,—that is, independently of their explaining the electric condensation at the poles,—is of the negative sort. They are not improbable, nor, so far as I know, at all opposed to observed phenomena.

235. As before remarked ("Phil. Mag.," S. 4, vol. 4, p. 126), the air is condensed by the electric attraction between the two electrified strata of the atmosphere; and this condensation of volume will be greater, the rarer the air becomes through which this attraction takes place. The experiments of Becquerel and of Faraday shew, that the magnetism of a volume of oxygen or atmospheric air, is greater with dense than with rarefied air. Consequently, since these electric strata

* Having been received by the Royal Society, November, 1853.

contain, and consist of, condensed air, and since this condensation varies with the electric attraction, which from the experiments of Harris, is as the square of the amount of the electricity—the two strata in travelling towards a pole have their parts attracted by the terrestrial magnetism, with a force varying with the square of the electricity. Now, bearing in mind the height of the auroral arch, the great rarity of the air of the region, and, as a consequence, the facility with which a volume of such air is moved, I think it may be fairly concluded, that the portions of the atmospheric strata will be gathered about the magnetic poles much in the order of their electric intensity; therefore, the lines of equal electric intensity lie perpendicular to the magnetic meridian. If, then, the electricity forces a disruptive discharge from one stratum to the other, the discharge will spread, or be simultaneously produced, across the heavens east and west along the line of equal electric intensity; and the appearance will be that of an arch resting on the horison and bisected by the magnetic meridian. After the air has discharged its electricity, the power which caused the condensation under consideration being thus removed, the air must expand and ascend. It may be well to observe, too, that Dr. Faraday has given it as his opinion, that very possibly masses of air at different temperatures, may be moved by the magnetic force of the earth. ("Phil. Mag." S. 4, vol. 1, p. 74.)

236. With regard to the advance of the arch along the magnetic meridian, and which is generally directed from the pole, that is, generally from a colder region, it appears to me to result from a falling temperature. Thus, if at a given northern latitude, at sunset, these higher parts of the atmosphere contain so much thin cloud, that the electricity can force its way from one stratum to the other, then, as the temperature falls during the night, portions of the atmosphere more to the south acquire increased conducting power, owing to the diminished temperature producing mist.

237. The electricity of the charged portions of the heavens near to an auroral arch, will travel with a speed depending on the intensity of the electricity and the conducting power of the atmosphere towards the arch, which is the seat of the discharge. This will produce more or less luminous effect; but that part of the heavens beyond the arch—supposing the arch advancing towards the spectator—consisting only of air which has been discharged by the progress of the arch, cannot be thus luminous. Consequently, inside the bow the cloud appears dark, while on the outside, the heavens are bright. After the arch has passed the zenith, the greater part of the heavens then consists of air which has discharged its electricity; and the brilliancy of the aurora, therefore, usually diminishes.

238. M. Biot and M. Lottin have both noticed the frequent parallelism of the auroral rays with the dipping-needle. I advance the following explanation with much hesitation, as it needs the light of further experiments. However, if we take an electric current flowing along a straight wire of considerable length, and then present to either side of the current a magnetic needle, perfectly free to turn on its

centre of gravity, the needle will place itself so, that a line along the length of the needle, will form a right angle with a line drawn from the point of suspension of the needle to the electric conductor; and the line along the length of the needle will also lie in a plane which intersects the conductor at a right angle; also let a circle be described on this plane, having its centre in the electric conductor, and the distance between it and the magnetic needle for a radius. An electric current flowing along the circumference of this circle, neither approaches towards, nor recedes from, the rectilinear current. It is, therefore, free from the mutual magnetic actions of currents discovered by Ampère. Further, if a straight electric conductor be made to subtend an arc of this circle, the arc and the chord may be considered as coincident, if the radius is sufficiently long in comparison of the chord; and, consequently, such electric currents lying parallel with the magnetic needle are neither attracted nor repelled. We may now, according to Ampère, substitute for the foregoing long straight current the resultant, at the magnetic equator, of the electric currents which produce terrestrial magnetism. Now, Faraday, long ago found that a moveable conductor, when placed parallel with the line of the dip, was not moved by terrestrial magnetism. ("Researches in Electricity," vol. ii., p. 154.) Currents, then, such as auroral rays, when lying parallel with the dipping-needle, are in the position in which there is least action between them and the magnetism of the earth; from which it seems at least highly probable, that the power required to develope the magnetism of a current is less in this position than in any other. And since the magnetism of a current is developed simultaneously with the current, this latter will tend to place itself so that its magnetic power may be most easily developed. If, now, the two atmospheric strata, which by discharging to each other produce the auroral arch, are so electrified and so placed one above the other, that, within limiting angles of inclination on each side of the line of the dip, it is, so far as electric induction is concerned, indifferent what path the discharge shall take, then the discharge will form in a line parallel with the line of the dip, which, as just shewn, offers least resistance to the discharge.

Postscript. The motion of auroral streamers, east or west, results from their position with regard to the line of the dip. Thus, let the dotted line *SN* represent the line of the dip, and the dark line a streamer, lying in the plane of the magnetic meridian. Since the streamer does not lie parallel with the dip, its path must either recede from, or approach towards the terrestrial magnetic currents. The electric current of the aurora is directed from below upwards, and consequently, in the above figure, the path of the current in receding from the line of the dip, also goes from the earth's electric currents,

which currents, moreover, travel from east to west; and such a streamer, therefore, goes to the west. If the streamer is in the position of the dotted line on the left-hand side of *SN*, it for the same reasons goes towards the east. The rapid motion of streamers is against the supposition that the conducting matter of the streamer is itself translated. Probably the position of the discharge is the only thing that moves.

26, *Brunswick Place, Ball's Pond Road*, December 13th, 1853.

THE TORBANEHILL MINERAL.

Mansion House, Old Park, Bristol, Dec. 1st, 1853.

"My Dear Sir,—In common, doubtless, with the majority of your readers, I have perused with great interest your Report of the chemical evidence given on the celebrated Torbanehill Mineral Case. Having been called upon in the early part of last year to examine, professionally, specimens of the so-called Boghead Coal (which appears to be similar in character to the Torbane mineral), I may be allowed to offer an opinion on the matter in dispute. Before doing so, however, you will permit me to call your attention to the results of my analyses, which may not, perhaps, be considered the less worthy of confidence because they have been arrived at by one totally unconnected with either of the contending parties. My results may be thus summed up:

Chemical and Physical Properties:—

Somewhat elastic and difficultly frangible; color, brown or brownish-black; streak, fawn-colored; lustre, none; fracture, imperfectly conchoidal. The specific gravity of the two specimens examined was respectively—No. 1, 1·203; No. 2, 1·256. When heated to redness, the mineral burnt with a white smoky flame, giving out a pleasant bituminous odor, and left a white bulky ash. It did not coke. It was but slightly acted upon by boiling-water, alcohol, wood-spirit, chloroform, æther, oil of turpentine, and coal naphtha. Most of these solvents, however, acquired a pale brownish tint, when digested on the mineral, in consequence of the solution of a trace of bitumen.

Boiling caustic leys removed from the powdered coal a considerable quantity of aluminous matter, which was re-precipitated from the solution on the addition of sal-ammoniac.

When heated to redness in closed vessels, it furnished a large amount of gaseous matters, very rich in olefiant gas, &c., together with some ammoniacal liquid and tar, and other products similar in character to those yielded under the same circumstances by common pit-coal.

Quantitative Analysis:—Composition per cent.:—

		Spec. No. 1.	Spec. No. 2.
Moisture		0·45	0·64
Volatile matters		68·70	64·76
Coke	Carbon	10·29	10·51
	Ash	20·56	24·09
		100·00	100·00

The ash was found to consist of:—

	No. 1. Per cent. of Coal.	No. 1. Per cent. of Ash.	No. 2. Per cent. of Coal.	No. 2. Per cent. of Ash.
Silica	13·497	65·66	15·598	64·75
Alumina	6·360	30·93	7·860	32·63
Lime	0·126	0·61	0·201	0·83
Magnesia	traces.		traces.	
Oxide of iron	0·451	2·19	0·301	1·25
Copper	?		distinct traces.	
Chloride of Sodium, &c.	0·126	0·61	0·130	0·54
	20·560	100·	24·090	100·

Almost the whole of the alumina was dissolved out of the ash by boiling diluted sulphuric and hydrochloric acids.

Ultimate Analysis of the Dried Coal per cent.:—

	No. 1.	No. 2.
Carbon	64·31	60·71
Hydrogen	9·01	8·51
Nitrogen	0·33	0·37
Oxygen	5·79	6·11
Sulphur		0·21
Ash	20·56	24·09
	100·	100·

It is clear, therefore, to my mind, that the Boghead coal is, chemically speaking, a mineral *sui generis*, forming part of a series of substances, which graduate into each other, and of which pit-coal, anthracite, and bitumen are the principal members. Regarded in a commercial point of view, however, it is undoubtedly a coal, and consequently, in my opinion, the jury very rightly gave a verdict in favor of the leaseholders.—I remain, dear Sir, yours very faithfully,

JOHN WATT, Esq. THORNTON J. HERAPATH.

IS COAL A MINERALOGICAL SPECIES?

By SAMUEL HIGHLEY, F.G.S., &c.

IT has long been a vexed question with mineralogists as to what should be regarded as a mineralogical *species*, there being many difficulties in drawing an exact definition; they are unanimous, however, in regarding a crystal as the true natural history *individual;* but as minerals more frequently occur in a massive state, they have extended the idea to those homogeneous bodies which exhibit the same chemical constitution throughout their mass. From the time of Werner to the latest works on mineralogy, Coal, though never found otherwise than in a massive state, has been classed in all systems as a mineralogical

species. Glocker, in his SYNOPSIS *, the latest attempt at a precise systematic arrangement of the Mineral Kingdom, divides Coal into three species—*the non-bituminous* or *Anthracites*, and *the bituminous*, including two species the *Black Coals* and *Brown Coals*, or *Lignites*. In his arrangement of the *varieties* there is a gradual passage from turf and semi-fossilized wood to the most perfect forms of coal.

Professor Quekett has examined, by the aid of the microscope, a vast number of sections of coal from various localities, as well as the Torbanehill mineral; and in concluding his paper at the last meeting of the Microscopical Society,† December 21st, he proves, that whilst the Torbanehill Mineral is a resinous-looking body, nearly homogeneous and devoid of vegetable structure, Coal, on the other hand, is almost entirely made up of woody fibre, and not homogeneous in aspect, the interstices between the fibres being filled with a resinous body similar in appearance to the Torbane mineral. Having examined many varieties of coal taken at random from Professor Quekett's extensive series of horizontal and vertical sections, I come to the conclusion that Coal is nothing more than fossil wood mineralised by bituminous or resinous matter, and that it holds the same relation to such bodies as wood opal does to *Opal*, and is in fact similar in character to any organic remains mineralized by silex, carbonate of lime, or other substances. Viewed in this light, *Coal has no claim to a place in any Mineral System as a species;* if this be admitted, it will prove the necessity of submitting massive minerals to microscopical examination before their true character can be acknowledged, and before they can be admitted into any natural history classification.

The Torbane mineral seems, from the homogeneous nature of its structure, to have greater claims to a position in a Mineral System, and so far as our present knowledge as to its other characters, extends, is probably a mineral species *sui generis*.

As it is well known that some minerals contain a vast amount of extraneous matter in *mechanical* combination, for example, the so-called Fontainebleau Sandstone, which consists of rhombohedral crystals of *Calcite* greatly impregnated with sand, may not the Torbane mineral be a true mineral species, whilst ordinary coal is the same, mechanically mixed with woody matter? Under the microscope, the eye cannot detect any difference between the matter of which the Torbane mineral is composed and that which occurs between the interstices and pores of the woody fibres of coal. I incline, however, to the former opinion, that coal is nothing but a fossil wood, therefore *not a mineral species*.

Fleet Street, December 22nd, 1853.

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* "*Generum et Specierum Mineralium secundum ordines naturalis digestorum Synopsis*," &c., by E. F. Glocker. Halle, 1847.

† For a full report of Professor Quekett's paper with colored illustrations, *see* the "*Quarterly Journal of Microscopical Science*," part VI., January, 1854.

On the PURITY *of* CERTAIN CHEMICALS.* By JOHN WILLIAMS.

MY attention having been called, by various circumstances which have occurred within a short time, to the examination of certain chemicals of considerable importance in commerce and medicine, I hope that the notes I have thrown together on this occasion in the form of a paper may not prove unacceptable.

The first article to which I have to direct attention is the *Fer Reduit*, or Pulvis Ferri of the Ph. D., sometimes also known as Quevenne's Iron. This substance is made by passing a stream of dry hydrogen over dry peroxide of iron, at a dull, red heat. Some difficulty often occurs in the manipulation; but the article, when properly prepared, is a perfectly impalpable powder, of a slaty black color, and permanent in dry air.

It has lately been asserted that the scales which fall at the smith's anvil, ground to an impalpable powder, have been substituted for the genuine article. These scales, so levigated, are in color somewhat blacker than the reduced iron, but might, from a superficial observation, be readily mistaken for it, although, of course, quite different in their chemical nature, being magnetic oxide of iron, while the reduced iron is the pure metal. I find that the following test readily distinguishes the two articles:—

A few grains of the iron to be examined are put into a test-tube with a very small quantity of sulphocyanide of potassium and some distilled water; to this, two, or at most three, drops of strong nitric acid are added, and the whole is well agitated; if the iron be the magnetic oxide it will gradually dissolve, forming the characteristic blood-red solution of persulphocyanide of iron; if, on the contrary, the powder under examination be metallic, sulphuretted hydrogen will be evolved with effervescence, and the liquid will remain perfectly colorless. The evolution of sulphuretted hydrogen may also be easily detected by damping a piece of blotting-paper with diacetate of lead, and holding it over the mouth of the tube, when, in the case of the reduced iron, it is rapidly blackened; whereas, should the substance under examination be magnetic oxide, no such effect takes place. It is proper that I should mention, that if the colorless solution produced by these means be exposed to the air for some hours, it will also become peroxidised, and consequently red.

The next article I have to mention is cyanide of potassium, which is met with in commerce of variable quality and under different names. It has lately been publicly asserted that all who wish to obtain a genuine cyanide, and especially photographers, should purchase a black article, and not on any account use the white, which is stated to be much more impure, whereas the difference of color is merely accidental; but as it is of great importance that every one should be able to test the real strength of the cyanides of commerce, and thus to

* Read before the "Chemical Discussion Society," December 8th, 1853.

ascertain, if possible, which is the most economical and advantageous article for use, I would recommend Liebig's process, which is extremely easy and well adapted for general practice, and which, in few words, is as follows:—

A given weight of the cyanide of potassium to be examined is dissolved in water, and to the solution is added a large excess of a solution of caustic potassa (say ten grains of cyanide in two ozs. water, and 1 oz. liq. potassæ, Ph. L.); into this a solution of nitrate of silver of known strength is slowly poured, with constant agitation, until a permanent precipitate begins to be formed. The quantity of nitrate of silver used being thus determined, every atom (or 170 grammes) used is to be calculated as equivalent to two atoms (or 130 grs.) of cyanide of potassium in the sample under examination. The following were the results actually obtained by experiment on fair samples of the cyanides of commerce:

The medicinal cyanide, in crystals (obtained by dissolving the gold cyanide in alcohol, and evaporating) and perfectly dry, yielded 99·02 of real cyanide per cent.

The so-called gold cyanide of commerce, quite white, and used principally by electro-platers for gold work, yielded 92·43 per cent.

The so-called silver cyanide of commerce, also quite white, and principally used for the purpose of silvering, yielded only 49·74 per cent.

These results are quite independent of the presence of carbonate, or cyanate, or any other impurity likely to be met with, and shew that, although white, a very high degree of purity can be obtained in the fused cyanides of potassium of commerce.

The last body to which I have to allude, is the so-called Hastings' naphtha, or pyroacetic spirit of the pharmaceutist.

This liquid was originally supposed by Dr. Hastings to be pure acetone, but it was afterwards proved that wood naphtha was used in its preparation, and it was supposed to be the pure pyroxylic spirit, or methylic alcohol of chemists,—the crude naphtha being deprived of an irritating oil, which prevented its being mixed with water without becoming milky. But even this purified naphtha, upon examination, proves to be of very variable constitution.

When chloride of calcium, in fine powder, is added to the Hastings' naphtha to be examined until the liquid is saturated, a variable quantity of oil rises to the surface after a short time; and this, I find, occurs even when the naphtha mixes perfectly with water without turning milky. If, now, the oil be separated, and the saturated liquid introduced into a retort placed in a water bath, and subjected to distillation, a (generally) large quantity of a naphtha liquid passes over; this is known to chemists under the name of lignone; if a small quantity of water be now added to the residuum in the retort, and the distillation repeated, another naphtha spirit passes over, which is the true methylic alcohol or pyroxylic spirit, and which has a perfectly different constitution and properties from the lignone produced in the first part of the distillation. It is important to know that these two liquids occur in very variable proportions in every sample of Hastings'

naphtha examined: some of the samples, with the exception of a small trace of oil, were pure lignone; others contained not more than from five to ten per cent of methylic alcohol; whilst other samples contained from 65 to 70 per cent. of methylic alcohol to about 30 per cent. only of lignone. So long as this uncertainty of constitution exists in an article of frequent medicinal use, we cannot wonder at contradictory results being recorded, and at the remedy consequently losing the confidence of the profession. It would be extremely interesting and important to know which of the two bodies, lignone or methylic alcohol, ought to be used in dispensing prescriptions wherein Hastings' naphtha or pyroacetic spirit is ordered, as an uncertain mixture, such as I have described, cannot certainly with propriety be employed in medicine.

TRANSLATIONS AND ABSTRACTS.

METALLURGY.

On ALLOYS, *considered in Relation to their* CHEMICAL COMPOSITION.

By M. A. LEVOL.

(Concluded from page 151.*)*

Study of the Alloys formed of Silver and Lead. — In industrial processes, silver and lead very frequently form alloys in extremely various proportions, in consequence of different metallurgical operations, and although it appears that these alloys are very generally regarded as not very homogeneous, and as their tendency to liquation is well-known, I have thought that it might be interesting to study them according to the method which I had employed in the examination of the foregoing alloys; with this view, I examined in the same way twelve spheres formed of the alloys:—$Ag^{10} + Pb$; $Ag^{6} + Pb$; $Ag^{4} + Pb$; $Ag^{3} + Pb$; $Ag + Pb$; $Ag + Pb^{2}$; $Ag + Pb^{3}$; $Ag + Pb^{4}$; $Ag + Pb^{10}$; $Ag + Pb^{15}$; $Ag + Pb^{20}$; $Ag + Pb^{100}$.

The following are the results obtained:—

Alloy $Ag^{10} + Pb$, in 1000 parts . . { Silver 912·5 / Lead 87·5

Very white, with a grey fracture; only slightly malleable; it contracted very much in solidifying.

This alloy gave the following cyphers:—

A drop		914·00
Superficial parts .	Above . .	912·50
	Below . .	908·00
	Lateral parts .	912·00 / 910·50
Central part		915·25
Internal excentric parts	Above . .	910·50
	Below . .	912·00

Alloy $Ag^{6} + Pb$, in 1000 parts . . . { Silver 862·0 / Lead 138·0

This alloy was of a greyish white; its color singularly resembled

that of platinum, even in the fracture, which was in fine grains; it contracted very much in solidifying, and was rapidly altered in damp air.

Assay of the drop			863·00
Superficial parts	Above		856·50
	Below		856·50
	Lateral parts	Silver	851·50
		Lead	852·00
Central part			866·00
Internal excentric parts	Above		855·00
	Below		857·00
Alloy $Ag^5 + Pb^9$ in 1000 parts		Silver	839·1
		Lead	160·9

Greyish white, grey fracture, contracting very much in solidifying. When heated to a sufficient degree, in contact with the air, it acquires a very beautiful violet blue color, which is not the same as that presented by pure lead in the same circumstances. At a temperature approaching fusion, this alloy swells up considerably, and, by increasing in bulk, produces a spongy mass whose form resembles that of the cauliflower. Every assayer in passing silver assays through the cupel, has witnessed a phenomenon caused by the sudden cooling of the muffle, when it is effected at the moment when the assays are near completion; from spheroïdal, the surface of the bath suddenly becomes horizontal, and soon after the alloy rises and presents the form of a spongy mass analogous to that formed by the alloy, which we expand when it is heated towards its point of fusion. Berzelius mentioned this phenomenon in his *Traité de Chimie*, he says (French edition, 1833, vol. iii., p. 154):—"In extracting the silver from the galena found in the copper mine of Fahlun, it sometimes happened that the silvêr congealed towards the end of the operation, and produced an excrescence resembling the cauliflower in form, and which it was necessary to cupel with a fresh portion of lead; it has also been observed in this case that some silver penetrated into the cupel. Having examined a sample of silver that congealed, I found that it contained bismuth."

I have made some experiments with the view of finding the explanation of the production of this singular phenomenon; and, considering that the analysis of the product might put me in the way of it, I first examined its composition; I did so with several samples obtained by causing, designedly, and on a sufficient scale, the circumstances which caused its formation.

The composition, in hundredths of this matter, according to my analyses, which with various products gave the same results, was as follows:

Silver	83·13
Protoxide of Lead	13·50
Metallic Lead	2·30
Loss	1·07
	100·00

The following is the detail of my experiments :—

1. When we introduce into a bath of pure silver, the surface of which is beginning to solidify, a certain quantity of lead under the solid form, this metal immediately enters into combination with it, the surface of the bath at once becomes horizontal, flat and wrinkled, then the excrescence in the form of a cauliflower is produced, with development of heat and light.

This matter afterwards requires, in order that it may enter into fusion, a higher temperature than the composition of the alloy represented by the two metallic elements of which it is formed would suffer, which is readily explained by the difference of fusibility which exists between lead and litharge; moreover, when we bring an ignited match near the matter at the moment when the phenomenon is produced it does not inflame, as is the case, on the contrary, during the fluxing of the silver; the cause of this phenomenon is not due, therefore, like that of the fluxing, to a disengagement of oxygen; this is, morever, superabundantly proved by the development of heat and light which accompanies it, and the presence of metallic lead in the product.

2. Instead of operating thus, if we fuse in a crucible, in contact with the air, a certain quantity of silver, during a sufficient space of time to enable it to absorb in these conditions as much oxygen as possible, and if we thus pour into the bath oxide of lead in the state of fusion, in such a manner as entirely to cover it with it, and allow it to cool, the same phenomenon will not be produced, the silver will remain separated from the litharge, and it will only be observed that the oxygen, in being disengaged, will raise up the fused oxide in light micaceous bracteæ. I may add, in passing, that in this condition the silver does not flux, probably because the litharge which covers it controls its cooling: minium is not formed in this case.

The facts above detailed appear to point out, as the cause of the phenomenon, the absorption of oxygen, under certain conditions, by the lead forming part of the alloy; but why is it not produced with any alloy of silver and lead subjected to the same influences? Unless I am mistaken, it is because, in the particular compound in which the two metals are found combined in the proportion of Ag^4 to Pb, or thereabout, the lead exists in a state of division favorable to the production of the phenomenon, with this additional circumstance, that the temperature at which it is accomplished is insufficient for the oxide of lead formed to enter into fusion.

The peculiarity which I have just mentioned, and which belongs to the alloy $Ag^4 + Pb$, but especially the constant composition which the products which I have analysed presented, led me to hope that the alloy $Ag^4 + Pb$, the proportion of whose silver very nearly corresponded with that of this product revivified, might be homogeneous, but it was not so, as we shall see: a sphere formed of this alloy gave, indeed, as its results :—

Drop	840·5

Superficial parts	Above		835·5
	Below		841·0
	Lateral parts		837·5
			838·5
Central part			859·0
Internal excentric parts	Above		841·0
	Below		842·0
Alloy $Ag^3 + Pb$, in 1000 parts		Silver	675·9
		Lead	324·1

This alloy was very ductile and capable of being flattened and rolled, but it then retained very little tenacity; its color was bluish grey; hydrosulphuretted compounds and humid air decomposed it in a short time. It fused at a temperature approaching cherry-redness.

The following are the cyphers obtained with the sphere formed of this alloy:—

Drop			676·5
Superficial parts	Above		672·5
	Below		670·5
	Lateral parts		677·5
			678·0
			679·0
			679·0
Central part			720·0
Internal, excentric parts	Above		711·0
	Below		673·5
	On the same horizontal line as the centre		674·0
			676·0
Middle and centre of the jet			676·0
Alloy Ag + Pb in 1000 parts		Silver	510·5
		Lead	489·5
Drop			516·6
Superficial parts	Above		512·5
	Below		513·5
	Lateral parts		520·0
			518·0
			512·0
			511·0
Central part			577·5
Internal, excentric parts	Above		535·0
	Below		512·5
			512·0
Middle and centre of the jet			511·5
Alloy $Ag + Pb^3$, in 1000 parts		Silver	342·8
		Lead	657·2
Drop			347·5
Superficial parts	Above		340·5
	Below		340·0
	Lateral parts		344·0
			351·0
			347·0
			349·5

Central parts				349·0
Internal, excentric parts	Above			347·0
	Below			344·5
Middle and centre of the jet				344·0
Alloy $Ag + Pb^5$, in 1000 parts		Silver		258·0
		Lead		742·0
Drop				262·0
Superficial parts	Above			255·5
	Below			268·0
	Lateral parts			259·0
				257·0
				256·5
Central part				267·0
Internal, excentric parts	Above			263·5
	Below			262·5
Middle and centre of the jet				256·0
Alloy $Ag + Pb^4$, in 1000 parts		Silver		206·8
		Lead		793·2
Drop				206·0
Superficial parts	Above			202·0
	Below			201·5
	Lateral parts			204·5
				203·5
				203·5
				205·0
Central part				207·0
Internal, excentric parts	Above			208·0
	Below			206·0
Middle and centre of the jet				205·5

No longer hoping to obtain a homogeneous atomic alloy of silver and lead, I should have confined my work to alloys of this nature; but, on reflection, I thought that it might be useful to go further and study in the same respect alloys still poorer in silver; most frequently, indeed, the assay of the metallurgical or docimasic products which do not contain nearly as large proportions of silver as exist in the alloys which we have just examined; and, to mention a case of it, in the assay of silversmith's ashes, sometimes very heavy residues of argentiferous lead are obtained, and of which some people, apparently supposing them to be homogeneous, content themselves with cupelling only a portion, in order to conclude from the product which they obtain, the contents of the whole. Is their supposition correct? This had to be answered by experiment; there was, therefore, much interest in extending this work further, and this is what I did in examining also the alloys $Ag + Pb^{15}$; $Ag + Pb^{30}$; $Ag + Pb^{100}$.

Alloy $Ag + Pb^{15}$ in 1000 parts		Silver	65·0
		Lead	935·0
Drop			67·25

Superficial parts	Above		62·25
	Below		67·50
	Lateral parts		66·25
			66·50
			66·75
			67·00
Central part			73· 5
Internal, excentric parts	Above		68·00
	Below		68·25

This alloy was, moreover, the subject of two experiments, one of which I shall now detail; the other will be mentioned when I come to speak of the compound $Ag + Pb^{100}$.

Having fused and well minted about 200 grammes of the alloy $Ag + Pb^{15}$, and having run them into an U tube of refractory glass, in which I kept the alloy in fusion without agitation for an hour, and allowed it to cool tranquilly, I found this curious result, that the alloy was found to liquate as follows:—Cyphers of the assays of the cooled matter, extracted at different heights from the upper part of one of the branches of the U tube, making abstraction of the upper extremity which was a little oxidised

		101·50
		77·50
		78·00
Assays taken in the same manner from the other branch		68·50
		84·00
		63·00
Assays taken in the part corresponding to the middle of the curve of the U tube		26·50
		26·50

Alloy $Ag + Pb$, in 1000 parts		Silver	49· 4
		Lead	50· 6
Drop			46·00
Superficial parts	Above		56·00
	Below		46·00
	Lateral parts		44·00
			44·25
			45·00
			45·00
Central part			46·50
Internal, excentric parts	Above		45·75
	Below		46·60
Alloy $Ag + Pb^{100}$, in 1000 parts		Silver	10· 3*
		Lead	989·7
Drop			9·75
Superficial parts	Above		10·00
	Below		10·00
	Lateral parts		10·00
			9·75
			10·00
			10·00

* The latter alloys present nothing striking in their external properties; the last four might be easily confounded, as regards their physical characters, with pure lead.

Central parts		9·75
Internal excentric parts	Above	9·75
	Below	9·75

The alloy $Ag + Pb^{100}$ may, therefore, be considered, if not perfectly homogeneous, at least, as not giving considerable differences in the assays; its cypher may also be established from the production from any part whatever of the mass, which, as we have seen, the various preceding alloys would not admit of; it may then be said that argentiferous leads may be analysed as we have just indicated, beginning with the quantity of about 10,000ths, but scarcely beyond. For greater certainty, however, I repeated with the alloy $Ag + Pb^{100}$ the experiment which I had made on the alloy $Ag + Pb^{15}$, in order to ascertain whether, in the same circumstances, it was susceptible of experiencing the same effects; I even repeated the proof, by operating under the influence of a Bunsen's battery, each pole of which terminated by a wire of pure copper, dipped into the bath of argentiferous lead in fusion, by the branches of the U tube, and stopped at a distance of a few centimetres from the opposite pole, but I was not able to cause in it any effect of liquation. I may here add, that having tried to liquate the alloy $Ag + Pb^{15}$ under the influence of the battery, I found sensibly the same results as I had obtained without having recourse to this influence.

I will terminate this second part of my work with a table presenting the maximum differences of cypher observed for each alloy of silver and lead experimented on.

Formulæ.	Cypher.	Differences.
$Ag^{10} + Pb$	912·5	7·5
$Ag^{6} + Pb$	862·0	14·5
$Ag^{5} + Pb$	839·1	23·5
$Ag^{3} + Pb$	675·9	49·5
$Ag + Pb$	510·5	66·5
$Ag + Pb^{2}$	342·8	11·0
$Ag + Pb^{3}$	258·0	13·0
$Ag + Pb^{4}$	206·8	6·5
$Ag + Pb^{10}$	94·4	19·5
$Ag + Pb^{15}$	65·0	7·5
$Ag + Pb^{20}$	49·4	2·5
$Ag + Pb^{100}$	10·3	0·25

It is almost unnecessary to remark that these differences present nothing absolute; they depend on circumstances which have presided over the experiment; thus, for example, as I have stated above, in abandoning the alloy $Ag + Pb^{15}$ to very slow cooling, the maximum of the differences was 75-1000dths, whilst it here amounts to only 7·5; but we must not lose sight of the fact that, in the latter case, the difference relates to the flowing of the alloys into a cast-iron mould, and, consequently, subjected to rapid cooling; it is therefore indispensable, in speaking of the phenomena of liquation, to specify the circumstances in which they are produced, since they may have a

great influence on the results, and it follows from these considerations that all the various alloys comprised in this table having been solidified in similar conditions, it may be admitted that the differences which affect them are perfectly comparable with each other.

I shall continue this memoir on the alloys, by examining, always in the same point of view, principally those which are employed in the arts, and for most important use, as bronze, type metal, brass, &c. The interest attaching to the discovery of homogeneous alloys of this nature will readily be understood.

"*Annales de Chimie et de Physique*," October, 1853.

ORGANIC CHEMISTRY.

On the ACIDS *contained in some* CHAMPIGNONS. By M. DESSAIGNES.

M. BRACONNET discovered, in champignons, two acids, which he designated by the names of *boletic* and *fungic acids*. In the Autumn of last year, I prepared these acids for the purpose of analysing them, and although M. Bolley had previously made known the composition of boletic acid, it may not be useless to publish the results which I obtained.

I extracted the boletic acid from the *Boletus pseudo-igniarius*, a champignon in which M. Braconnet discovered this acid; but I have also found it in small quantity in the *Amanite*, and in the *Deadly Agaric*. This acid is very easy purified, owing to its sparing solubility in water. The comparative examination of it, and also of fumaric acid, which I made, left no doubt as to the perfect identity of these two acids. I also analysed boletic acid isolated from its silver salt. I obtained by the combustion of the dried acid *in vacuo*, with oxide of copper and oxygen O—41·85; H—3·73; the calculation for the formula $C^8H^3O^8$, which is that of fumaric acid, gives C—41·38; H—3·45. Boletate of silver, dried at 212°F., and then calcined, contained in 100 parts, 65·01 of silver. Calculation requires 65·45.

The crude acid resulting from the above-named three champignons, and from which I had removed the boletic acid by concentration and crystallisation, was neutralised with ammonia, and then precipitated with chloride of calcium; I thus eliminated a considerable quantity of phosphate of lime. The filtered liquor was heated; it suddenly deposited a white crystalline powder. This salt of lime, washed, and then dissolved in weak nitric acid, did not give crystals; the nitric solution was precipitated with acetate of lead. The lead salt did not crystallise. I boiled it several times in water which extracted from it a small quantity of a soluble salt of lead. The portion insoluble in boiling water was finally decomposed with sulphuretted hydrogen. I obtained, by the evaporation of the filtered liquor, concentrically grouped prisms, which, after some days, were changed into large isolated crystals. These crystals burned without residue. All their chemical properties completely agreed with those of citric acid; besides, I burned the silver salt, dried at 212°F., with oxide of copper, and I

estimated the silver in the form of chloride. I obtained from 100 parts of the salt C, 13·96; H, 1·21; Ag., 62·67. The calculation for the formula of the citrate of silver, $C^{12}H^{10}O^{14}$, 3Ag., gives C, 14·03; H, 0·98; Ag., 63·15.

The liquid from which the citrate of lime had been precipitated by heat was treated with acetate of lead, and then with sub-acetate of lead, in order to extract the fungic acid. The lead salt, left in a drying stove, crystallised to a great extent. I separated the crystals by decantation, from a lighter, non-crystalline powder, and I extracted, by means of sulphuretted hydrogen, from the crystals thus purified, a colored acid which did not crystallise, which I half saturated with ammonia. I thus obtained a salt crystallising in the same form as the bimalate of ammonia, which it was easy to purify by crystallisation. Acetate of lead precipitated from the aqueous solution of this pure salt, a salt of lead which crystallised entirely, from which I extracted, by means of sulphuretted hydrogen, a colorless acid, crystallising confusedly *in vacuo*, and deliquescent. This acid presented all the characters of malic acid. When heated for a long time it was converted into fumaric acid. When almost entirely neutralised with lime, and heated to ebullition, it deposited a pulverulent salt of lime, which, dissolved in weak nitric acid, gave crystals like those of bimalate of lime. The ammoniacal bisalt, heated to 356°F., produced that sparingly soluble matter which bimalate of ammonia gives when treated in the same way. Finally, I analysed the silver salt, dried at 212°F., and I obtained C, 13·59; H, 1·58; Ag, 62·13; the calculation for the formula $C^8H^4O^{10}$, 2 Ag, which is that of malate of silver, gives C, 13·79; H, 1·15; Ag, 62·57.

Fungic acid appears to me, therefore, to be only malic acid mixed with citric and phosphoric acids.

Comptes Rendus, November 21st, 1853.

On the Proximate Principles *of* Wheat Bran.

By M. H. M. Mouriès.

Bran contains starch, nitrogenous matters, and a colored pellicle, which is regarded as ligneous.

It is known that crude flour, from which the bran has not been separated, furnishes a bread which many physicians now prescribe against habitual constipation and disposition to cerebral congestion. Magendie has shewn, also, that dogs live on bran bread, whilst they die if fed on white bread alone.

Why this difference in the effects of the two aliments? How does bran act in nutrition?

It cannot be only by the nitrogen of its proximate principles; for the latter exist in it only in small quantity compared with that which forms a constituent part of white flour. M. Mouriès has ascertained that the internal surface of bran contains several nitrogenous principles which remain to be isolated and characterised as species. But

the whole of these principles, which warm water dissolves, possess, like diastase, the remarkable property of liquifying starch, changing it into dextrine and sugar; it is then, especially by acting in this manner as a ferment, that bran acts in panification, and, consequently, in digestion.

Let a certain quantity of starch paste, heated to from 104° to 113°F., be divided into two equal portions; add to the former bran water prepared with warm water, and to the second a quantity of distilled water equal to that of the bran water; the first portion of starch will liquefy to a great extent, whilst the second will undergo no change. An aqueous solution of iodine will color the latter blue, and the former purple.

100 parts of starch reduced to paste with 1500 parts of water mixed with 100 parts of bran water prepared warm with 208 parts of bran, are liquified after 20 minutes at the temperature of 104°F.; after two hours, the solid residue is about 15·13, and the water, on evaporation, leaves 85 of dextrine and sugar.

The active matter of bran water differs from the active matter of barley or diastase, in that its activity is destroyed when it is precipitated with alcohol, whilst that of diastase is not so; and in that a temperature of 167°F. suffices for the same effect, whilst diastase requires from 208° to 212°F.

The effect of bran in bread is in conformity with the foregoing reactions; for 130 parts of this bread, supposed in the dry state, bruised with 520 parts of water, divide with facility, and after three hours exposure to a temperature of 104°F., the mixture has a milky appearance, and may be filtered.

This bread may be represented by:—

Soluble matter, dried at 212°F. . .	59·35
Insoluble matter	69·75

130 grammes of white bread, supposed dry, bruised with 520 grammes of water, form, by long trituration and at the temperature of 104°F., only a half solid mass represented by:—

Soluble matter	9·03
Insoluble matter	120·25

It would appear that the effect of bran on white flour commences in the confection of the paste, is propagated during the commencement of the baking, but it is accomplished only in the stomach.

Now it is easy to explain why a temperature superior to 167°F., does not destroy the activity of the ferment of the bran, when it is known that solid albumen may be exposed for a very long time to a temperature of 212°F., without being baked.

The experiments made by M. Mouriès explain, therefore, the difference existing between brown and white bread, by the influence, on the starch, of the bran which exists in the former and not in the latter.

Comptes Rendus, November 21st, 1853.

ANALYTICAL CHEMISTRY.

On a Method of VOLUMETRICAL ANALYSIS *of very general applicability.*

By R. BUNSEN.

(*Continued from page* 165.)

VII. *Determination of Sulphurous Acid and Sulphuretted Hydrogen.*

It has already been mentioned that the determination of sulphurous acid by means of iodine cannot be effected with accuracy when the aqueous solution contains more than 0·04 per cent., and this is likewise the case with sulphuretted hydrogen. When, therefore, a stronger solution is to be examined, it must be diluted to within this point with water free from air. The total volume of liquid is represented by P, a volume p measured off, mixed with solution of starch, and the quantity of iodine a t required for its decomposition determined volumetrically. The quantity of anhydrous acid contained in the volume P is :—

$$x = \frac{P\,SO^2}{p\,I}\,a\,t$$

The determination of sulphuretted hydrogen is effected in exactly the same manner, by means of the corresponding equation :—

$$x' = \frac{P\,HS}{p\,I}\,a\,t$$

As an instance of the application of this method, I will give the results of a determination of the density of sulphurous acid. The experiment was conducted in a manner similar to the chlorine determination given above, with the exception that the tube containing the sulphurous acid was inserted into pure water instead of a solution of iodide of potassium. The absorption was rapid, and with the exception of a small bubble that could easily be deducted, complete.

The volume of sulphurous acid at 33°·9 F. and $0^{m}{\cdot}7507$ barom. was 90·699 cub. cent.; P = 100 ; p = 194·0 ; t = 76·95 ; a = 0·0025387.

Hence the specific gravity of the gas,—

Calculated.	Found.
2·211	2·190

VIII. *Determination of Chromates.*

When a chromate, for instance acid chromate of potassa, is heated with an excess of concentrated hydrochloric acid, chlorine is disengaged in the proportion of three equivalents for every two equivalents of chromic acid. The decomposition is rapid and complete. When this chlorine is passed into a solution of iodide of potassium, an equivalent quantity of iodine is liberated. If therefore the quantity of iodine a (n t—t′) thus liberated indirectly by a weighed quantity of the chromate A is determined, the quantity of chromic acid x in the salt may be ascercertained by the equation :

$$x = \frac{2\,CrO^3}{3\,I}\,a\,(nt—t')$$

or the per centage by

$$x' = \frac{200\,CrO^3}{A\;3\,I}\,a\,(nt—t')$$

$$\text{When } A = \frac{200\,CrO^3}{3\,I}\,a$$

the difference of the two volumetrical determinations (nt—t′) gives directly the per centage of chromic acid in the substance examined. In the same manner this difference would give the per centage of pure chromate of potassa for

$$A = 100\left\{\frac{KO+CrO^3}{3\,I}\right\}a$$

and the per centage of pure chromate of lead for

$$A = 200\left\{\frac{PbO+CrO^3}{3\,I}\right\}a$$

The operation may be easily conducted in the following manner:— The weighed quantity of chromate is introduced into a blown-glass flask of 36 or 40 cub. cent. capacity, *a* Fig, 1.* This is about two-thirds filled with fuming hydrochloric acid, the delivery-tube connected with the neck by means of thick vulcanised caoutchouc, which closes perfectly air-tight without ligatures. There is no fear of a loss of chlorine, as the evolution does not commence until the temperature is raised. The small glass bulb *c*, with a sealed stem, is then introduced as a valve into the end of the delivery-tube, which is then inserted into the neck of the retort *d d d*, filled with solution of iodide of potassium. This retort has a capacity of about 160 cub.-cent., and is furnished with an expansion in the neck, to receive any of the solution which may be forced up. After about two or three minutes boiling, the whole of the chlorine is disengaged, and an equivalent quantity of iodine liberated from the solution of iodide of potassium. The contents of the retort are then transferred to a beaker-glass, and the liberated iodine a (nt—t′) determined in the usual manner. For the purpose of testing the method, very carefully prepared bichromate of potassa was selected: 0·7116 grms. of the salt, heated nearly to the melting point, lost 0·0016 grms. of water; 1·0230 grms. reduced with hydrochloric acid, and precipitated by long digestion with ammonia, gave 0·5327 grms. of ignited oxide of chromium.

The volumetrical analysis gave:—

* The drawing for the wood-cut which should have appeared here is unfortunately mislaid, and, at the last moment, we find ourselves compelled to go to press without it. We hope to rectify this omission in our next. The blame does not rest with the Editors.

A = 0·2379; n = 3; t = 5; t′ = 81·6; a = 0·0025389;
and in another instance:
A = 0·2943; n = 3; t = 103·7; t′ = 1·60; a = 0·0025387:
This corresponds to:

	Weight determination.	Volumetrical analysis.	
		I.	II.
Chromic acid*	68·18	68·32	67·95
Potassa	31·60	31·47	31·83
Water	0·22	0·22	0·22
	100·00	100·00	100·00

Another quantity of salt, consisting of 0·4632 grms. of chromate of potassa, dried at 302°F. and 1·27 grms. sulphate of potassa, gave:
A = 1·7332; t = 101·7; t′ = 382; n = 5; a = 0·0025387;

	Taken.	Volumetrical determination.
Chromate of potassa	26·72	26·91
Sulphate	73·28	73·09
	100·00	100·00

The volumetrical analysis of pure bichromate of potassa furnishes the most simple means of estimating the value of a, or the quantity of pure iodine corresponding to the weight of impure iodine contained in a degree of the burette. This value may be ascertained by means of the equation:

$$a = \frac{3\,I\,A}{(KO + {}^{3}CrO^{3})\,(nt - t')}.$$

A solution of iodine, for which the value of a was found by weight determination to be 0·002543 grms. pure iodine in a burette degree, gave, by the volumetrical examination with bichromate of potassa,—
A = 0·3637; n = 6; t = 65·0; t′ = 24·2.
These data lead to the almost perfectly identical value 0·002545 grms. for a.

IX. *Valuation of Chlorates.*

By the reaction of warm concentrated hydrochloric acid and chlorates, a reduction of the chloric acid is brought about, which, as there is not any evolution of oxygen, may consist in any one of the following decompositions:—

$$ClO^5 \left\{ \begin{matrix} ClO \\ ClO^3 \\ HO \end{matrix} \right. \quad HCl \qquad ClO^5 \left\{ \begin{matrix} 3ClO \\ 2HO \end{matrix} \right. \quad 2HCl$$

$$ClO^5 \left\{ \begin{matrix} 2\,ClO \\ 2\,Cl \\ 3\,HO \end{matrix} \right. \quad 3HCl \qquad ClO^5 \left\{ \begin{matrix} ClO \\ 4Cl \\ 4HO \end{matrix} \right. \quad 4HCl \qquad ClO^5 \left\{ \begin{matrix} 6Cl \\ 5HO \end{matrix} \right. \quad 5HCl$$

* The atomic weight of chromium = 335·09.

Whichever of these changes really takes place, whether only one or all of these decomposition products are formed, cannot be determined; but however that may be, they all agree in effecting the liberation from a solution of iodide of potassium of 6 atoms of iodine for every atom of chloric acid in the chlorate. Therefore, when a weight x of a chlorate RO,ClO^5 is distilled with concentrated hydrochloric acid and iodide of potassium, the iodine liberated $= \frac{6\,I}{RO,ClO^5}\,x$

When this quantity of iodine is determined volumetrically, as a (nt—t'), we obtain for x:

$$x = \frac{RO,ClO^5}{6\,I}\,a\,(nt—t')$$

and the percentage of chloric acid = x' contained in the quantity of chlorate employed = A, thus:

$$x' = \frac{100\ ClO^5}{A\,6\,I.}\,a\,(nt—t')$$

An experiment made with pure chlorate of potash, as in the above chlorine determination, gave,—

A = 0·0889; n = 3; t = 74·1; t' = 7·2; a = 0·002578

	Calculated atomically.	Volumetrical determination.
Chloric acid . .	61·57	61·83
Potash . .	38·43	38·17
	100·00	100·00

X. *Valuation of the Superoxides of Lead, Manganese, Nickel, Cobalt, &c.*

The percentage of oxygen in brown superoxide of lead, may be ascertained by means of the formula:

$$x = \frac{2O}{AI.}\,a\,(nt—t')$$

Pure superoxide was prepared from red lead, by continued boiling with acetic acid. Of the thoroughly washed and well dried powder 0·7048 grm. gave off on ignition in a stream of dry air 0·004 grm. water and 0·043 grm. oxygen.

By the volumetrical analysis of 0·7402 = A grm. there were obtained: n = 5; t = 58·8; t' = 9·8; a = 0·002578. These data lead to the composition,—

	Atomic calculation.	Weight determination.	Volumetrical analysis.
Lead . .	86·12	87·23	86·95
Oxygen . .	13·31	12·20	12·48
Water . .	0·57	0·57	0·57
	100·00	100·00	100·00

In order to ascertain the percentage of superoxide of manganese = x in a quantity of native oxide = A, the equation

$$x = \frac{100\ MnO^2}{A\ I}\ a\ (nt—t)$$

may be employed.

In an experiment with oxide of manganese, obtained from pure protocarbonate by ignition in the air, 0·4839 grms. gave,—

A = 0·4839 ; n = 3 ; t = 78·3 ; t′ = 16·4 ; a = 0·0025387,

and, in a second experiment,—

A = 0·3725 ; n = 3 ; t = 75·7 ; t′ = 59·4 ; a = 0·0025387.

	Atomic calculation.	Volumetrical analysis.	
		I.	II.
1 atom superoxide of manganese .	37·98	39·37	39·25
2 atoms protoxide „ .	62·02	60·63	60·75
	100·00	100·00	100·00

From these two experiments it would appear as if the composition of the oxide formed by ignition in the air, does not strictly agree with the formula assumed for it. If this is the case, it would be easy to do away with the error arising from this source in manganese determinations, by making in each instance a volumetrical examination of the ignited manganese precipitate.

(To be continued.)

CONTRIBUTIONS *to the* HISTORY *of* IODINE. By M. CH. BARRESWIL.

FOR some years past the attention of chemists has been, so to speak, concentrated on investigations concerning iodine. This metalloïd, which was found merely as an exception in nature, and the reason of whose existence was scarcely comprehended, has become, by degrees, of an importance which is inferior to no other simple body. If we look at the analyses which each periodical publishes monthly, iodine is the body the most frequently treated of; it will soon become the most essential element, the principle *par excellence*. When we reflect that the processes by means of which iodine is sought for, are exactly the same as those employed twenty-five years ago, namely, the blue coloration produced by the action of iodine on starch, iodine being eliminated from its combinations by hyponitric acid, we are astonished that a body which is so easily known should be so frequently mistaken; and we are led to ask whether such unexpected results are not produced in certain very different cases, by one cause against which we are not always sufficiently on our guard; I speak of the impurity of the reagents.

During an investigation some time since, I found iodine in the nitrate of soda of commerce. This discovery led me to think that I should find iodine in the various reagents derived from nitrate of soda or nitric acid. Consequently, I sought for iodine in nitric acid

of various degrees, in purified nitrate of soda, in the caustic potassa produced from American potash, purified by fusion with nitrate of soda, in sulphuric and hydrochloric acids, *reagents* which have all been recommended and employed in the search for iodine. Now it happened that sometimes I found iodine in all, sometimes in some only, and sometimes I did not find the smallest trace. It was especially in nitric acid that I have seen it the most regularly and in the largest proportions.

Nothing can be more simple than ascertaining the presence of iodine in nitric acid: the latter is to be diluted with an equal volume of water, it is introduced into a flask provided with an abductor tube, it is heated, and the vapors which are disengaged are received in a small test-glass containing very clear starch paste, made with starch well washed in cold water. Nitric acid proceeding from nitrate of soda is not the only kind which is iodised; that produced from saltpetre likewise contains iodine. But it is well known that saltpetre is now prepared by double decomposition, by means of chloride of potassium, or even of carbonate of potassium and nitrate of soda.

Iodine will be detected in saltpetre or nitrate of soda, by adding to them a little nitric acid, free from iodine, and heating as above described. This method of analysis may be recommended as a process for the purification of nitrate of soda for the necessities of the arts.

It is sufficient to pour on the nitrate of soda 5 per cent. of its weight of ordinary nitric acid, to completely remove the iodine.

The presence of iodine in sulphuric acid may be ascertained in an analogous manner. The iodine in this case proceeds from the nitric acid which has been introduced into the leaden chambers.

Hydrochloric acid doubtless derives its iodine from sulphuric acid, at least when it is not produced from salt derived from the manufacture of the sodas of varechs or from the preparation of saltpetre. To detect iodine in hydrochloric acid, it is advisable to saturate it previously; the investigation is in other respects as easy as in the preceding cases.

The iodine found in potassa proceeds doubtless from the nitrate of soda which has been used to purify the potassa from ammonia, at least when it is not inherent in the potassa itself, especially when it is produced from the salts of varechs.

Nothing is more simple, moreover, than to obtain pure potassa; it is sufficient to purify nitrate of potassa by several crystallisations, whence baryta is made, and sulphate of potassa, which, with pure baryta, gives potassa free from iodine.

By using pure acid and pure potassa, we are certain when we find iodine, and the search for it is very easy if iodine really exists in the substances on which we experiment. The process to which I allude, that of MM. Colin and Gaultier de Claubry, is the most sensitive and exact. That founded on the insolubility of iodide of silver in ammonia is far from infallible; and I have proved that bromide of silver precipitated in the presence of nitric acid, acquires so powerful a cohenon, that it dissolves in ammonia with great difficulty; so that I was

disposed, when I observed this fact, to take the insoluble precipitate for iodide of silver; and to assure myself that I was dealing with bromine, I was obliged to decompose the salts of silver with zinc and sulphuric acid. I must observe in all cases that the presence of bromine is no obstacle in the search for iodine by the other method.

If, for example, we propose to seek for iodine in bromine, it may be easily done by the following method:—the bromine supposed to contain iodine is poured into a saturated solution of sulphate of iron in great excess; the bromine soon disappears; nitric acid, free from iodine, is then added, until the liquor is of a deep brown, and the operation is continued as though seeking for iodine in nitric acid. This investigation requires attention for several minutes.

Without doubt the preceding reflections, and the experiments on which they rest, are known by many. I cite them merely as preliminary to a work which I shall shortly publish, in consequence of some *negative* results in the search for iodine in several substances in which its presence appeared sufficiently demonstrated.

Journal de Pharmaciee et de Chimie, November, 1853.

On the PRESENCE *of* NICKEL *and* COBALT *in some* FERRUGINOUS WATERS, *and on a* PROCESS *for* ISOLATING *them.* By M. OSSIAN HENRY.

A PHARMACEUTIST of Valence, department of the Drôme, *M. Mazade*, has communicated to the *Academy of Medicine* and the *Institute*, the discovery which he has made in the ferruginous waters of Neyrac (Ardèche) and in their ochreous deposit, of several substances hitherto unknown in mineral waters. Amongst the chief of these, he mentions *oxide of titanium, zirconium, cobalt* and *nickel.*

I was requested by the Academy to test the facts announced by M. Mazade, and I, therefore, treated the mineral water itself and the ochreous deposits furnished by it. The results of my experiments clearly prove the presence of nickel, cobalt, and oxide of titanium, and that of zirconium, although in a less distinct manner.

This work led me to think that, by reason of the concomitance so frequent between certain bodies, it might perhaps be with nickel and cobalt, with respect to iron, as it is with arsenic for example. In fact, we now know, that the presence of one leads us to suspect that the others accompany it, and the search for them is consequently less difficult.

I then sought for a process by which I could obtain nickel and cobalt in a very certain manner. I first used a mixture of pure protosulphate of iron and very small quantities of the nitrates of cobalt and nickel. I should observe, before proceeding further, that I had previously ascertained that pure water charged with carbonic acid in Briet's apparatus does not dissolve hydrated sesquioxide of iron, but is easily charged with the hydrated oxides of nickel and cobalt; that the carbonates of lime and magnesia are also more or less dissolved, and that it is the same with the carbonates of baryta and strontia when

recently precipitated. This proved, I added to the solution of sulphate of iron and the compounds of nickel and cobalt, a decided excess of purified carbonate of soda. The deposit thus formed was subjected to the prolonged action of the air, by shaking it briskly in a bottle until it had completely assumed the color of rust; then being properly washed with distilled water, it was treated in a flask with a large proportion of water containing carbonic acid. To the filtered liquid was added a slight excess of hydrosulphuric acid, or of hydrosulphate of soda, and immediately a turbidness ensued, at first greyish, then black, which after several days and repeated agitation collected at the bottom of the vessel.

In this precipitate the nickel and cobalt were found in the state of sulphurets; they were washed in a capsule with care, and subjected to the action of aqua regia. The solution being completed, they were evaporated to dryness, taken up again by water, to separate the fragments of sulphur, then again concentrated to dryness, so as to remove as far as possible any excess of acid. In this state, again dissolved in water, pure carbonate of soda was again added, to transform the metals into carbonates, and then by oxalic acid into oxalates, according to Laugier's method, which is here of the greatest use. The oxalates of nickel and cobalt are almost insoluble when the liquors are not acid (should they be so, they can be saturated with bicarbonate of potassa or soda). To these oxalates was added a considerable excess of ammonia; it was filtered and left to spontaneous evaporation in a warm atmosphere. The ammonio-oxalate of nickel separated as a dirty apple-green powder on losing the excess of ammonia; there then remained the double salt of cobalt, which forms the rose-colored or reddish liquor, when the metal is in rather considerable proportion.

To complete the operation, I decompose each of these oxalates separately by means of an alkaline carbonate and heat; I then treat each precipitate with borax, and subject it to strong calcination or fusion. The nickel gives a glass of a dirty greenish color; the cobalt glass is either blue, violet, or rose-colored, when there remains a trace of iron.

To apply this method to the search for nickel and cobalt in natural ferruginous mineral waters or their ochreous deposits, the following plan must be adopted:—

A slight excess of carbonate of soda is to be added to a large quantity of mineral water; the deposit must be allowed to form in the air, so as completely to peroxidise the iron. This deposit, or that obtained from the basins of the springs, is then to be employed.

These deposits are to be treated with pure hydrochloric acid, and the oxides of titanium and zirconium are to be eliminated by suitable evaporation, as well as the greater part of the sand and silica. The dilute solution contains principally alumina, lime, magnesia, iron, manganese, and the metals cobalt and nickel, which concern us here.

Carbonate of soda is again to be added, so as to obtain a deposit which is agitated a long time in the air in a great quantity of water. When washed and well peroxidised, it is to be put for some time in

contact with distilled water charged with carbonic acid, which only dissolves sensibly the earthy carbonates and those of nickel and cobalt. It is filtered; a current of pure hydrosulphuric acid is passed through it, or hydrosulphate of soda is added.

The black or blackish precipitates of the sulphurets of nickel or cobalt often manifest themselves very slowly. When these metals exist in very small quantities, they appear with a dirty greyish tint, which is soon developed in the liquid. After a certain time the precipitates are to be carefully collected and washed with a little water charged with carbonic acid, then treated with aqua regia, and the operation is continued as above described.

By following this method, I have found, in the ochreous deposits of many mineral springs, the more or less manifest presence of nickel and cobalt, one frequently more apparent than the other. I think that these elements, first discovered in mineral waters by M. Mazade, will often be found with iron and manganese. This leads me to repeat that there are in the productions of nature certain very frequent concomitances by which the presence of one element leads us to anticipate certain others. Numerous examples of this are well known in arsenic and manganese, in relation to iron; iodine, and bromine with chloride of sodium; in the sulphates of lime, soda, and magnesia, likewise in carbonate of lime, which is always associated in greater or less proportion with that of magnesia, &c.

Journal de Pharmacie et de Chimie, Nov. 1853.

EXAMINATION *of* BASSORAH GALLS. By Dr. L. F. BLEY.

THE author has examined the so-called Bassorah galls. They were treated consecutively with alcoholic ether, cold water and hot water.

The galls lost 12 per cent. of water in drying. The alcoholic etherial extract left a residue equal to 17 per cent. on the weight of the galls, of which water dissolved 14 per cent. The portion which was not soluble in water was shaken with ether, which took up 3 parts of a yellowish fatty oil of a mild taste. The portion insoluble in ether was a brown solid resin; it had a shining appearance, was hard and brittle, burnt when heated over flame, fused, and left a small residue of earthy ashes. The alcoholic solution gave a grayish-white flocculent precipitate with water; it dissolved in caustic alkalies with a fine red color, and in fatty etherial oils. Concentrated sulphuric acid dissolved the resin, forming a reddish-brown fluid. Nitric acid gave a yellowish solution, The portion of the galls not taken up by ether was exhausted with alcohol, the alcohol distilled off, and the residue treated with water, by which 5 parts of the resin above-mentioned were separated. The tannic acid was precipitated by solution of gelatine; in this manner 60 parts of tannic acid and 8 parts of gallic acid were obtained; these were determined by their behaviour with salts of oxide of iron.

Adding the constituents contained in the watery extracts, the Bassorah galls consist of—

Tannic acid	130 parts
Gallic acid	8 „
Fatty oil	3 „
Resin	17 „
Extractive with salts	10 „
Lichen-starch with small portions of common starch and albumen	42 „
Fibrine	230 „
Water	69 „
	500 parts.

Archiv. der Pharm. and Chemical Gazette, lxxv. p. 138.

AGRICULTURAL CHEMISTRY.

Manufacture *of* Guano *from* Fish.

We extract from the "*Agricultural Gazette*" the following Report of a Meeting, held at the Royal Dublin Society House. The subject is the more important from the fact that, as we learn from the same Number of that most useful periodical, the ordinary sources of our guano supply will not last, at the present rate of consumption, many years longer.

G. Sanders, Esq., in the Chair.

Dr. William Barker said,—The subject is one which was brought under my notice not many days since, and which seems to me of such importance that I do not think it will need any apology in bringing it before the Society, especially as my object in doing so is to subject it to full investigation, and endeavor, by eliciting opinions, to prove its commercial value. We are all aware of the extent to which the importation of guano has been carried for more than ten years past. The importations from Africa, South America, and Australia have varied from 100,000 to 200,000 tons per annum; and if we take an average price of £9 per ton, we find that the money value of a year's import has exceeded in some years one million sterling. It is therefore palpable that any substitute that could be found for this substance would prove a valuable adjunct to our wealth; and if we could direct a portion of this expenditure to this part of the British Empire, we should be conferring a substantial benefit on the country; and still further, if we could, in producing this effect, give employment to a branch of industry deserving of encouragement—we should be doing an amount of benefit to the country only limited by the extent to which it was employed. Now these benefits would, in my opinion, be all obtained were we to find that there were a source of a similar manure to be found around our own coasts, which, with a slight expenditure of capital and labour, could produce a profitable investment, and secure to our own country the money expenditure at present bestowed upon Africa, Australia, and South America. That such might be done, will be at once apparent, if we can shew that we have around our seaboard in Ireland a reservoir of similar products to those of the places mentioned, and if we could, instead of trusting to the chemical agency of birds, convert the fish which swarm upon our coasts into as profitable a manure as they (the birds) produce. I think that could be accomplished, provided we could procure the supply of fish and a chemical agency—equal in efficiency to that of birds. Now, with regard to the latter part, I feel no doubt that the invention which has become the object of a patent by Mr. Pettit, effects this. He has patented a plan for producing an artificial guano, by subjecting fish and all their parts to a process analagous to that which fish undergoes in the stomach of a bird. The fish, either fresh or dried, is treated with a small quantity of sulphuric acid, by which it is reduced to a pulpy state. In this state it is

dried either by the direct application of heat, or by the admixture of substances capable of absorbing moisture. This, then, forms an artificial guano, which, if tested by the usual criterions of manure, may be considered equal, if not superior, to any imported We all know that animal matter is the best manure, owing to its presenting azotised principles to the soil. Dung, urine, blood, animal remains are, as we know, the most powerful fertiliser of soils, and that fish ranks at least equal to any of these. Owing, however, to the facility of its decomposition, and the consequent difficulty of transport, it has heretofore been unemployed for this purpose, except on the coasts where it is found; but by means of this process it may be converted into a dry, inodorous, and easily transported article of commerce. With respect to the sufficiency of the supply on our coasts, papers read in this Society prove the enormous supplies of fish on our coasts, and especially of fish not available for human food. Skate, dog-fish, congers, fishing-frogs, &c., may all form a good manure. Prawns and small crustacea may be all converted into valuable manure. Every fisherman around the coast will testify that from one-half to two-thirds of the fish is lost or thrown overboard, which 2*l.* per ton would ensure being brought to land. The decrease of our fisheries is probably owing to the large ratio between maketable and unsaleable fish. The decrease of boats and men since 1852 was, 1254 boats and 8482 men. Would this diminution have taken place had we a sure and regular market for all fish, whether edible or inedible? 2nd. Of the mercantile value of manure, I could not speak; but, taking the tables of Boussingault and Payen as the criterion, and without special analysis, I should say that Pettit's patent manure must be a valuable article. By their experiments it appears that the value of manure depends on the slow decomposition of azotised substances. Now, the fish manure afforded by any dried white fish gave 16 per cent. of nitrogen, whilst blood gave but 12 per cent., marine plants but 2 per cent., guano 14 per cent., and farm-yard manure but 2·0 per cent. If this be the case with fish in its normal state, how much more should we expect from fish carefully prepared, without the loss of its elementary constituents. Here are the general results of the chemical analysis of artificial manure by Professor Way:—

Moisture	4·93
Organic matter	88·36
Sand, &c.	1·35
Earthy phosphates	4·06
Alkaline salts, &c.	1·30
	100·00
Ammonia	16·78

Other analyses were made by this gentleman, shewing that this manure contains a greater percentage of ammoniacal salts than the best guano imported from Ichaboe or Peru. I am disposed to believe that taking into account the cost of manufacture and all the incidental expenses, this artificial manure could be manufactured at a lower price than 9*l.* per ton, the present price of the best guano. On referring to Mr. Sinclair's statement, we find that sprats are occasionally sold round the coast at less than 1*l.* per ton, and waste fish at 1*l.* or 1*l.* 10*s.* Now, when we remember that one-fourth of all the fish taken on the coast, or at most two-thirds, is saleable, we are enabled to form some idea of the amount of the supply of refuse fish that could be obtained for the purpose of this manufacture. Moreover, what is called saleable fish, taking all descriptions together, is seldom sold at a higher rate, taking an average of the whole coast sales, than from 5*l.* to 8*l.* per ton, and even in the neighbourhood of Dublin it is, I have been informed, purchased at that rate by the wholesale dealers. The next point to be considered is, would it be possible to get a sufficient supply of fish round the coasts of this country? To this question there is only one answer to give. The quantity of fish in the sea, in spite of what the coast fishermen may say, is really illimitable. I believe that there is no limit whatever to the supply of fish which we may obtain from the ocean, and that it only depends on energy and on the exercise of skill to supply any quantity that may be required; and it must be remembered that the dog-fish, the whale, the porpoise, fishing-frog, &c., and other inhabitants of the ocean, which consume our edible fish, may be converted into this artificial guano. I would also

advocate the establishment of such manufactories on the ground that they would provide an inexhaustible field for the employment of our population, and stimulate us at the same time to use the means which nature has placed at our disposal for the augmentation of the national wealth.

Mr. JEFFERS. With respect to supply of fish, I have written to some friends, from whom I have received information of value with reference to this point. A gentleman, connected with the Dublin Steam Packet Company, writes from Kilrush to the following effect:—"With reference to the supply, no doubt this coast abounds with inferior fish, such as dog-fish, skate, star-fish, &c.; and if there were a price offered for that sort of commodity, I am convinced that a quantity would be taken." I have also a letter from a person who was acting as agent for the Irish Fisheries Company, and he says—"I have no doubt that if there were a market, the take would be enormously increased; for all, except the ling, cod, and hake, taken here, are thrown away; the farmers refusing to use it as manure, although they are ready to admit its good qualities for that purpose." With reference to the supply likely to be obtained in our own neighbourhood, I have consulted some of the fishmongers of Dublin, and they all told me that refuse fish (the scourings of their shops) could be afforded in their large quantities at a cost, covering the expense of collecting it, of 30*s.* a ton on the average.

Mr. ANDREWS, Secretary to the Royal Irish Fisheries Company. The question of supply is, in my opinion, one of difficulty. Having been engaged in the fisheries on the south-west coast of Ireland for some years past, I have taken pains to ascertain the quantities of fish brought in from time to time, and I have found that the average quantity of refuse fish taken by a trawl boat of 45 tons burden is from 6 cwt. to 8 cwt., consisting of skates, dog-fish, ray, frog-fish, &c. The supply from trawl boats would be small indeed. The canoes and other shore boats which work in Dingle Bay reject the refuse fish, and it is not possible for them to collect any great quantity of it, because the fishermen always cut them away from their lines. It appears that the average quantity of refuse fish taken at Dingle during the year was about 1 ton per week. I do not mean to say, however, that with a greater number of boats a larger quantity could not be brought in; but I think there would be difficulty in obtaining a regular or sufficient supply for the purpose of the proposed manufacture.

A MEMBER. Does the gentleman mean a ton a week on the whole of the south-west coast?

Mr. ANDREWS. I say that the offal would be a ton a week altogether. Dr. Barker stated that the value of the prime fish was about 5*l.* a ton. Now, on that point he is quite misinformed, for so far from such being the case, the prime fish amounted at times to 30*l.* a ton.

Dr. BARKER. I know that turbot, for instance, would sell at a much higher rate, but I believe that on the average around the coast the price would be about 5*l.* a ton.

Sir JAMES DOMBRAIN, late Inspector-General of Coast Guard. After what has fallen from Mr. Andrews, I think it right to state that I have also made some inquiries, and with your permission I will read a letter which I received this morning, the contents of which are in direct opposition to what Mr. Andrews has advanced. It is from Mr. Daith, who writes from Kinsale, in reply to inquiries which I made as to the quantities of refuse fish that could be procured in that locality:—

"Upper Cove, 24th November, 1853.

"My dear Sir James,—Dog-fish and skate can readily be purchased here at 1*l.* 10*s.* per ton (and I think I may include congers): the weekly average catch of the above fish, from the month of March to August, is eighty tons or thereabouts: but it must be understood that the present system is not to bring more than one-tenth of the catch to the shore, in consequence of there being so little demand for it. But if 1*l.* 10*s.* per ton was offered, the whole would be brought to land instead of nine-tenths of it being thrown back into the sea. From the month of August to March, I say the average weekly catch would be about eighty tons, as only the row boats fish with long lines during the latter months. And as for sprats they are very fluctuating. In 1846 Kinsale harbour was full of them from the month of October till February 1847, at which time I saw three boatsful of sprats, each

containing three tons, sold for 10*s.*; but since that period there has not been any very great abundance. There are three other fish-curing establishments, with minor ones, exclusive of Morley, in Kinsale. Morley carries on full one-third of the business, and I think somewhat over. I will now, Sir James, give you my candid opinion upon this subject (you of course being well aware that I have no interest in the matter, or in Kinsale); but Kinsale will produce more refuse fish and offal for the purpose required than any other port in Ireland. W. DAITH."

Now, in corroboration of these statements, I may state that about the same period I was in the harbour of Glandore, and so great was the quantity of sprats in the harbour that I passed a boat myself which was loaded with five or six tons of them, and when I inquired the price of the whole cargo, I was told that I might have them all for 10*s.* I think that even setting aside the regular fishing stations altogether, the preparation of this artificial guano might be extended to every little fishing-boat on the coast of Ireland. There are numbers of fishermen who go out in small boats and take large quantities of dog-fish, skate, &c., and if a market was open they would find it worth while to dry these fish, and preserve them for the purposes of this manufacture.

Mr. BARRY, one of the commissioners of the fisheries, said—I have with me here a letter addressed to Sir James Dombrain by a fish-curer in Kinsale.

"Dockyard, Kinsale, November 21st, 1853.

"Hon. Sir,—I most respectfully beg to report for your information that it appears by my account book, from 20th April to the date hereof, the number and description of fish cured by me is as follows: Also offal from same:—Herrings, 55,000; hake 24,000; cod and ling 2500; small fish 2200; conger eels 500; the offal 36 tons, which I have sold for manure at 2*s.* 6*d.* per ton, amounted to 4*l.* 10*s.*

"JAMES MORLEY."

I have ascertained from several respectable persons that they found it difficult to dispose of them even at that price. There are two establishments at Kinsale which have been much longer in business, and I have reason to believe that a fair estimate may be formed of what may be done in the article of offal alone, when I find that one of these men produced since April last no less than 36 tons weight, which if multiplied by three will give 108 tons within six months. I have no doubt that a large quantity of inferior fish may be added to that, for the offal merely includes the entrails, the bones, and other refuse matters, and if you take into account the amount of fish obtained in a state unfit for human food, and unmarketable fish, the quantity will be considerably increased. I can confirm what Sir J. Dombrain told you with regard to the extraordinary influx of sprats which takes place from time to time. I have seen Glandore harbour literally a living mass of sprats. It is a remarkable fact that they came into that harbour on the 8th of August, and did not leave it until the 25th of December. But during that period the harbour was full of sprats, and being a time of distress, people came in numbers and bought the fish for literally almost nothing. I have seen them offered for 2*s.* 6*d.* per boat-load, each boat containing not less than three tons, which were bought for the purpose of being used as manure. I am inclined to think that if individuals entered into a trade of this kind they would succeed to their satisfaction; but I fear that a company would, from the expensive machinery they would have to employ, hardly be enabled to make their arrangements to get a sufficient supply to render the speculation remunerative.

The CHAIRMAN. With regard to establishing the manufacture in remote places, one difficulty would be to obtain a supply of sulphuric acid, which, having to go by sea and not by land, would be an expensive thing to transport to the western and south-western coasts of Ireland.

Dr. BARKER observed that with regard to the supply of sulphuric acid, he did not conceive for the quantity required for this manufacture the transport would present any difficulty.

Mr. GOSLETT. I will confine myself to its eligibility as a means of ameliorating the condition of the fisheries in this country and their collateral branches of industry. The state of the fisheries in Ireland has been considered a subject of interest and national importance by successive governments; laws have been enacted for their regulation; bounties granted for their encouragement; commissioners ap-

pointed to watch over them; and I am sorry to add, with little useful practical result. And why? because fishermen pined for a market to absorb the fruits of their toil. Governments, in coming forward with protection and aid, could not supply a remunerative outlet. Since the cessation of bounties the fisheries have been declining, and the boats have been employed in the coasting trade. Under the inducement held out by government, I find that from 1822 to 1829 the men employed in the fisheries increased from 36,159 to 63,421; and of boats from 7655 to 13,119; in other words, nearly double; whilst the decrease in fishing-vessels since 1846, is 9431, and within the last year there has been a decrease of 1254 vessels and 8482 men. I mention these facts to shew that a useful stimulus would undoubtedly improve the state of our fisheries, and I suggest that the manufacture of the superabundant fish, most of it useless as human food, into guano would supply that stimulus. That a substitute for guano would be in demand there is not a doubt. There is a want for more guano than Peru can supply; the demand is increased and the supply is falling off. All the new discoveries are phosphatic and not ammoniacal manures. In 1846 the amount of guano imported into Liverpool alone was 115,120 tons; in 1851, 92,593 tons; last year, 22,722 tons; while the stock of Peruvian in this country is now stated to be almost nil. Mr. Caird, of Baldoon, puts forward the value of guano as a manure in the following interesting shape: -"A ton of guano is the equivalent of 10 qrs. of wheat, a cargo of 1000 tons to 10,000 qrs. In the United Kingdom we grow annually 5,000,000 acres of wheat. We have imported annually, for the last four years, about 5,000,000 qrs. of wheat and flour. This is exactly one quarter per acre more than our home produce, and that quarter, I am persuaded, might be got by the simple means I have pointed out. I do not under-estimate the many other means at our command for increasing our corn and green crops; and among these I would place the fish guano, patented by Mr. Pettit, which I had lately an opportunity of examining, as likely to become a valuable substitute for imported guano." The next question that arises is, can the supply of fish be had? On that head I think there can be little doubt—my own conviction is, that there will be an ample supply. I am not surprised at objections being raised on the score of supply, as I believe none but men engaged in the fisheries can afford us satisfactory information. Mr. Andrews has, I think, accounted for the small supply of waste fish that comes to shore, by stating that the fishermen cut away all the useless fish; but if any inducement was held out, all that fish would be brought on shore. I have received from Donegal the following letter from Mr. Sinclair, a gentleman to whom I was referred by the Commissioners of Fisheries, as an authentic source of information. Writing on the 5th of October last, the writer says:—"On the branch of Donegal Bay on which my property is situated, the sprat fishery has been going on for the last three months, and the average has not exceeded 2*l.* per ton; although in the earlier part of the season fish was scarce. Yesterday morning the price was about 1*l.* 5*s.* per ton, and from the present appearance there is every probability of an improving take on the same coast. There is a very considerable fishery of hake and cod—in the curing of which about two-fifths of the gross weight is available for manure, while vast quantities of skate and dog-fish are thrown overboard. Two years ago I was residing in that part of the country during the sprat fishery, and made a large quantity of oil from them. The process was simply roasting in large pans of sheet iron, and pressing with a screw press; by this process the fish were reduced to a state in which they would have been easily acted on by a chemical agent, being deprived of a great part of the moisture. I have at present two boats and fourteen men at the sprat fishery, and two with ten men at the round fish. All have paid well so far, at the price I mentioned, and with increased demand there would be increased supply, as the fish are really inexhaustible. Should it, therefore, appear to you worth while to adopt my suggestion as to a trial, I can give you such facilities as few other people can offer." I have received from Mr. W. J. Barry, of Glandore House, the following reply to queries put by the commissioner, as to the probability of a supply which they doubted at the time could be obtained, and as to the price at which it could be had:—

"Glandore House, 14th Nov., 1853.

"My Dear Sir,—I received your note with enclosed very interesting pamphlet. I have read it with much attention, and I am perfectly satisfied of the project being

practicable, on very remunerating terms, to the manufacturer, and of the great advantage which would result from the establishment of markets on this coast I need not dwell; the supply can be had, and the only apprehension that I have is, that it will take some time to bring the manure into general use—in other words, to create confidence as to the article being genuine, as we farmers look with great suspicion on the preparation of artificial manures. I shall feel much obliged for any further information you may favor me with on the subject. The estimated cost of fish on this coast would not average one pound per ton. W. J. Barry."

I am happy to state that Mr. James Redmond Barry, her Majesty's Commissioner of Fisheries, to whom I am indebted, as well as to Mr. Ffennel, for their kindness in affording me information and assistance, has stated that notwithstanding his doubts on the subject he finds that a market for offal, damaged, and inferior fish, is of all things the great desideratum in the south of Ireland. On the east coast my information has been collected personally, aided by the kind assistance of the Coast Guard Officer at Skerries. The information received from the trawl owners, amongst whom I may mention Mr. Bartlett, often quoted by the commissioners of inquiry into the fisheries as an authority, is that at each shot of the trawl upwards of 5 cwt. of animal matter is thrown overboard as utterly useless, independent of the broke fish, or coarser sort of eadible fish that is laid alongside for the long shore men, and with which the trawlers pay the services of boats taking their fish ashore. They consider that a certain market for their refuse and broke fish would be deemed a great boon, and would tend to the more perfect development of the fisheries. Mr. Bartlett, who is the first of the colonists from Torbay, and has been for forty-five years employed in catching fish, considers that if an arrangement were made with the crews of the trawlers now engaged in Skerries Bay (about sixty in number) the owners would hail it as a boon; that sixty tons per week of fish and marine animal matter now thrown overboard could be supplied at Ringsend, and that the broke fish would amount to nearly the same quantity; and can safely say that every week he has thrown away more than a ton of fish useless to any one, and has frequently thrown overboard three or four tons of large ray, monk fish or mollygoons, and large skate. He adds that none but a fisherman can tell the take of a trawler. I find in the statement of the British Channel Committee that "Brixham has 150 trawls, which might be drawn forty-eight times in four hours. Allowing one haul per hour, would be 3600 hauls in one day, every time loaded with quantities of fish—young turbot, brills, soles, flounders, &c., returned dead to the sea." When I mention that a cod produces more than three millions of eggs, a ling nine millions, and other fish several hundred thousands, I think the question of supply, were common industry, patience, and perseverance employed in securing this vast wealth that exists on our coasts, must be set at rest for ever. Ireland is decidedly a favoured country in the riches of its waters: the Irish fisherman is enterprising and industrious, when a prospect of reward for his toil is held out. I entreat the most earnest and searching inquiry into this matter, and if it offers an opportunity of improving the condition of the working classes in this country, let it not be foolishly neglected.

After some discussion as to the possibility of obtaining a regular and sufficient supply of fish for the purposes of the proposed manufacture, the Meeting adjourned.

On the Culture of Maize, *or* Indian Corn. By Berkeley Hill.

The following has been transmitted to us by Mr. T. J. Herapath, of whom the author was a pupil:—

After so general a failure in the crops of the last two years, partly owing to the wet Summers, and partly to the want of dry weather for sowing, &c., I think the public will be interested by a few remarks on the culture of maize, or Indian corn, on which I have been making some experiments for the last two years.

The sort I have tried is the one recommended by Mr. Keene, which he calls the "Forty-day Maize," from its flowers appearing forty days

after sowing. Mr. Keene introduced this variety from the northern side of the Pyrenees, where it is cultivated, and where the Summers are short and wet as in our own country.

Mr. Keene directs the seed to be sown about the 24th of May; if sown earlier, the crop is liable to be checked by late frosts, and it is a plant which does not easily recover itself after injury. A sure proof of the earth being warm enough for its reception, is the appearance of the ground-beetle, or cockchafer. Mr. K. says, "when the cockchafer appears, then sow your maize."

I find that if the weather be mild, it is advisable to sow earlier, although it may even be sown later, as some I sowed as late as the 1st of June ripened its seed by the 12th of October; but it required to be dried artificially before stowing away.

In April, 1852, I determined to try Mr. Keene's maize, as a friend living near London had succeeded in ripening some in an unsheltered spot. I therefore procured one cobb, which was sown in the open air on 24th of May of that year, and it was with the seed obtained from that crop I tried my experiments this year.

The piece of ground I selected was situated in a garden with a south-western aspect: though not under a wall, or other protection, it was warmer than the open field would be. The whole piece measured two perches. My first sowing was on the last day of April, dibbling in the seed at two inches apart in rows three feet asunder. This, however, I found was a great waste of seed, as it became necessary, as the plants grew, to thin out, from time to time, the weakest, until the plants remaining were a foot or eighteen inches apart. Half of these were manured with superphosphate of lime, dibbled in with the seed, and half with guano. The superphosphate manured appeared above ground in about ten days, while the guano manured did not appear for more than a fortnight; those manured with superphosphate, moreover, were finer and more vigorous plants, ripening their seed sooner.

My next sowing was about the 24th of May, without manure, though the ground was tolerably good. I dibbled these two inches apart in rows, a space of eighteen inches being left between the rows. These I subsequently thinned by degrees to eighteen inches between each plant. These came up seven days after sowing. When the plants were three inches to six inches high, I divided the plot of ground into about four equal parts, which I top-dressed with the following manures:—1st portion, with guano; 2nd, with superphostate; 3rd, with defecated sewage (Herapath's patent); 4th, peat charcoal saturated with the drainage from a stable;—giving each portion an equal bulk of manure. A fifth portion was left unmanured.

I found also that the plants manured soon recovered from the attacks of slugs, &c., while the unmanured were greatly retarded by their ravages.

My third and last sowing was on the 1st of June, the plants being treated in the same manner as the second. These, however, I manured with superphosphate and peat charcoal manure, applying the superphosphate first when sowing, and the peat charcoal as a top-dressing when the plants were three inches high.

When the plants were six inches high they were ridged up with earth, in the manner followed in ridging potatoes; and the operation was repeated when they were a foot or eighteen inches high. This was necessary, to prevent their being blown down, as they are very top-heavy when in flower. If the soil is drawn up round the roots, they soon become firmly established, as new roots spring from each joint when the joints are covered.

As the cobbs, or female flowers, sprouted from each joint, I stripped off all but one or two, generally leaving the highest, as too many cobbs weaken the plant.

When the tassels of the female flowers become withered and dry, I cut off the male flower, or feather at the top, leaving one leaf above the cobb to draw up the sap.

During this time I kept the ground clean by frequent hoeing, and cleared away all shoots from the roots; and this was all the attention I paid it until the seed was ripe.

The crop may be left out till the end of November without taking any injury, as the cobbs are so thickly enveloped with leaves that no rain or frost can penetrate to them.

The shoots stripped off are nauseously sweet, tasting very like liquorice, and cattle are greedily fond of them. I gave a small quantity daily to carriage horses in full work, with good effect, as they are not so washy as vetches and green oats.

In America many favorite dishes are made from the green cobbs. One prepared by frying them in butter is, I am informed, a very good one, though I have not tried it.

If this cereal can become acclimatized, which I see no reason to doubt after its having ripened last wet Summer, I should think it would be valuable in places where owing to a wet Winter the land is not cleaned soon enough for Spring wheat. It will doubtless be found to be a serviceable addition to our green crops, as it may be cut in six or eight weeks after sowing, and as it also comes in during the hay harvest before grass is plentiful. It must be moreover observed that it makes capital hay when dried like meadow hay.

A gentleman well known among agriculturists, Mr. Proctor, of Stoke Gifford, has kindly promised to try some next year in the open field, where I hope it will be as successful as in garden ground.

Having a tolerable quantity of seed (a bushel and a half), I intend making some more experiments, next year, on its value as a green crop, and as hay made from it; the results of which I shall communicate to the public at the earliest opportunity.

Stapleton, Bristol, Dec. 1853.

FORENSIC CHEMISTRY.

Examination of TRACES *of* BLOOD *on a* KNIFE *covered with* RUST.

By M. DAUBRAWA.

SOME time ago the author was directed, by the judicial authorities, to examine whether a knife which, it was suspected, had been used in the perpetration of a crime, was stained with blood. The blade of the

knife, which had remained for a long time in a damp place, was rusty; however, some brilliant and dark stains, very distinct from the rust, were observed. On heating the point of the knife, these stains scaled immediately, whilst the rust remained perfectly adherent; again, on steeping the knife in dilute hydrochloric acid, the shining stains were not altered, whilst the rust readily dissolved.

It was probable that these were blood-stains; but as non-nitrogenous organic acids may produce similar stains, the scales obtained by heating the blade were put into a small test-tube and heated to dryness over a spirit-lamp; reddened litmus-paper, slightly moistened, held over the mouth of the tube, immediately turned blue, owing to the disengagement of ammoniacal gas resulting from the hematine of the blood; for greater security, the entire blade was immersed for a very long time in distilled water. The water acquired a reddish coloration, and, by means of a microscope, in place of the shining stains, fibrine still adhering to the metal was readily observed.

The solution, treated with ammonia, gave no precipitate; with nitric acid, a white precipitate was formed; when heated, it became turbid; on the addition of chlorated water, it at first acquired a green color, and then was suddenly decolored, depositing whitish flakes; these various liquids, evaporated to dryness and incinerated, gave a residue which was treated with hydrochloric acid, in the filtered solution the presence of iron was demonstrated by the usual reagents.

Journal de Chimie Médicale, December, 1853.

Contributions to the HISTORY *of* BLOOD, *considered in a* CHEMICO-LEGAL *point of view. By* M. MORIN, *Professor of Chemistry at the School of Medicine, Rouen.*

THE existence of blood on the clothes of an assassin constitutes one of the most important problems of judicial chemistry. The actionof reagents and the power of the microscope, in many cases, demonstrate the presence of this organic liquid, provided that the criminal has not washed his clothes with the precautions which science might place at his disposal, or else, that the blood deposited on the bodies which have served him as supports, has not undergone putrid fermentation, so as to destroy its characteristic materials.

The assassin, in his haste to destroy that which is frequently an essential portion of the evidence against him, washes his clothes with boiling water, sometimes even with the addition of soap, with a view of hastening the disappearance of this indisputable evidence of his crime; whence results the fixation of certain matters of the blood on the tissue. This liquid, thus concreted, gives to the stained part a greater consistence than that of the tissue itself, and forms stains of a more or less deep brown color.

These stains are of two kinds. Sometimes they arise from a jet of blood; at others, they are imbibed. Sometimes the former have, so to speak, a spheroïdal form, and this happens when the blood falls on a tissue provided with small fibrils which retain it, and seem to be

opposed to its juxta-position, by favoring its coagulation. If, on the contrary, the tissue is free from nap, the vital fluid, by retaining for a longer time its temperature, and, in some measure, its vitality and its fluidity, forms stains by imbibition.

Whatever may be their state, when they are washed at a temperature higher than that at which albumen coagulates, they present the same color.

The ordinary means of demonstrating the presence of blood on a tissue, consist in the immersion of the stains in distilled water, with the view of obtaining a solution of the coloring matter with some proteïc elements; but this means of investigation is impracticable in the case of stains which have been washed in boiling water.

To solve this problem, we have made some experiments, which we now communicate. We received human blood, issuing from a vein, on linen; the tissue was immediately saturated with that liquid, and after some hours' exposure to the air, stains were obtained which were more consistent than the other portions of the tissue. They were washed with water at a temperature above that required for the coagulation of the albumen. The stains thus acquired a duller tint than they had previously. After this washing in ordinary water, they were subjected to the action of boiling water containing soap, and finally to that of cold water, until the liquor was no longer milky. In this state, and after desiccation, they were examined; their consistence was always superior to that of the tissue itself; the washing had not perceptibly dissolved the elements of the blood. We then detached, with the scissors, some of these stains, which we reduced into small strips, which were suspended in distilled water. After long immersion, the liquid was not colored, and agitation did not develope in it any streak which could render evident the solution of a body of greater consistence than the liquid employed. The application of heat did not determine any opalescence indicating the solution of the least traces of albumen.

The course to be followed for demonstrating the existence of this organic liquid coagulated on the tissue, owing to the washing performed by the criminal, consists in putting the stains in contact with a weak solution of pure potassa, and, after reacting for some time, a liquor is obtained which is precipitated white with nitric acid or with pure hydrochloric acid, which indicates the solution of one or more of the matters of the blood.

By this alkaline treatment, the stain loses some of its color; but what then is the body which is found in some measure indelibly fixed on the tissue? To solve this question, it is only necessary to put the stained tissue in contact with pure hydrochloric acid which dissolves the matter of the stain, and forms a solution which, carefully reduced to dryness, furnishes a residue having the property of acquiring a very clear blue color with ferro-cyanide of potassium, and a blood-red color with sulpho-cyanide of potassium.

These characters evidently demonstrate in washed blood-stains the presence of iron, which is always present in blood. From the fore-

going, when the chemist is enabled to establish in washed blood-stains the simultaneous existence of iron and a proteïc element of blood, it furnishes to the prosecution a new element of proof of guilt.

Journal de Chimie Médicale, December, 1853.

PHYSICS.

THERMO-CHEMICAL *Investigations Concerning* COMBINATIONS *Formed in* MULTIPLE PROPORTIONS. By M. P. A. FAVRE.

(Continued from page 182.)

Chloric Acid.—For determining the quantity of heat brought into play during the formation of this acid, it is sufficient to operate as already indicated, and to pass the dry chlorine into a concentrated solution of potassa, previously saturated with chlorate of potassa and chloride of potassiun,* by means of a great excess of these salts in powder, in order to effect the complete precipitation of the chlorate of potassa and chloride of potassium formed during the reaction.

This reaction is thus formulised :—

$$6\,Cl + 6\,KO + x\,Aq = 5\,ClK + ClO^5, KO + x\,Aq.$$

The elements of the calorific equivalent of chloric acid will be given by the equation :—

$$R = A + x + x' - (D - x').$$

R, heat collected in the calorimeter by the reaction of the chlorine on the concentrated potassa.

A, heat disengaged by 5 equivalents of chlorine for forming 5 equivalents of chloride of potassium which are precipitated.

D, heat absorbed by the decomposition of 5 equivalents of potassa in the state of dilute solution.

x', heat disengaged by the dilution of 6 equivalents of concentrated potassa employed for the reaction.

x'', heat disengaged by the combination of chloric acid and of the potassa in the state of dilute solutions for forming chlorate of potassa, supposed entirely crystallised.

x, heat disengaged by the combination of the chlorine with the oxygen, for forming chloric acid in the state of dilute solution.

A and D are known by previous experiments, x' and x'' require special determinations for arriving at the value of x.

* When the solution of potassa is of the last degrée of concentration the chloride of potassium is not dissolved in it, whereas it is dissolved in a less concentrated ley, such as was used in the experiment. As the quantity of chloride of potassium formed in an experiment exceeded that which might remain dissolved in the volume of the solution of potassa employed, it was useful to employ this alkaline solution previously saturated with chloride of potassium as well as with chlorate.

On this occasion, I proved a fact which deserves to be mentioned, namely, that the heat absorbed by the solution of chloride of potassium in the potassa ley employed does not exceed the quantity of heat which this same weight of chloride would have absorbed in dissolving in pure water.

This result shews that the solution is effected without any chemical segregation of the water and potassa; indeed, the potassa employed was such, that it continued to give a considerable disengagement of heat by its contact with water.

The foregoing seems to justify the opinion which regards the solution in water of a certain number of such salts as the chloride of potassium, &c., as a simple effect due to the destruction of the cohesion and to the change of physical state, and not to a true chemical combination between the water and the salt.

In these determinations, the potassa was employed in a state of concentration at least equal to that of the potassa employed in organic analyses; it was prepared in excess, in order not only to be able to multiply the experiments, but also in order to determine the value of x' with a solution always identical with that employed for determining the value of R. Moreover, it was important to know the strength of this potassa, that is to say, how much real potassa is contained in a certain volume. I shall now point out how this first datum was obtained.

We operated with potassa perfectly free from carbonic acid. A known volume of this dissolved potassa, the weight of which was 10·770 grs., was saturated. This quantity was equivalent to three measures made with the same pipette ($3 \times 3{\cdot}590$ grs.). The weight of chloride of potassium obtained after saturation with hydrochloric acid and evaporation, was 6·585 grs.; or 2·195 grs. of chloride of potassium each pipette. Consequently each measure of the pipette contained 1·385 of real potassa.

There was therefore 8·3 equivalents of water to 1 equivalent of real potassa in the solution employed.

The result agreed with an alkalimetrical assay previously executed, but on which we did not wish to rely exclusively.

After having introduced 25·130 grs. of this solution into the calorimetric eprouvette, an excess of chlorate of potassa and chloride of potassium was added. The current of dry chlorine was afterwards directed into the solution, and in such a manner as to saturate in the experiment only a very small fraction of the total quantity of potassa; consequently, the solution of potassa remained always concentrated. The weight of the chlorine was given by weighing the eprouvette twice, once before, and once after the operation. The abductor tube of the gas was weighed with the eprouvette.

1 grain of chlorine reacting on this solution disengaged:

I. . . .	521·44
II. . . .	519·57
III. . . .	525·44
Mean.	522·15 units of heat.

By multiplying this mean by 213, the weight of six equivalents of chlorine which acted, we have

$$R = 111218 \text{ units of heat.}$$

It remained to determine x' and x''.

Now, for x', it was proved that 3·590 grs. of the concentrated solution of potassa measured with the pipette disengaged by mixture with an excess of water:—

I. . . .	59·34
II. . . .	60·07
Mean.	59·70 units of heat.

Now, 1·385 grs. of real potassa, contained in the solution of potassa employed and combined with 8·3 equivalents of water, having dis-

engaged 59·70 units of heat, it follows that 1 gr. of real potassa would have disengaged 43·11 units. Six equivalents of real potassa, or 282 grs. would disengage therefore

$$282 + 43{\cdot}11 = 12157{\cdot}02 \text{ units of heat.}$$

This cypher requires a correction, which we shall make immediately. Indeed the six equivalents of potassa which constituted the chloride of potassium and chlorate of potassa, liberated 49·8 equivalents of water, which, by their contact with the portion of the solution of potassa left undecomposed, must have disengaged heat.

In an experiment made for establishing the value of R, and in which 0·660 grs. of chlorine were made to react on 25·130 grs. of alkaline solution, it is easy to see that the quantity of water suddenly liberated must be 1·391 grs. I, therefore, caused 1·391 grs. of water to react in the calorimetric eprouvette on 22·865 grs. of concentrated solution of potassa; for 22·865 grs. = 25·130 grs.—1·391 grs.—0·8738 grs. (0·8738 grs. being the potassa which entered into combination with the chlorine.)

The heat disengaged was 54·06 units of heat.

By referring this number to $6 + 8{\cdot}3 = 49{\cdot}8$ equivalents of water, which correspond to six equivalents of real potassa, we have

$$(54{\cdot}06 + 9 + 49{\cdot}8) : 1{\cdot}391 = 17419 \text{ units of heat.}$$

Correcting, according to this, the value of x', originally found, we have

$$x' = 12157 + 17419 = 29576 \text{ units of heat.}$$

In order now to determine x'', or the heat disengaged by the chloric acid in combining with the potassa, I made some special experiments.

1 gr. of dilute potassa treated with dilute hydrochloric acid (the chlorate of potassa formed remaining in solution) gave

I. . . .	322·16
II. . . .	324·12
Mean.	323·14 units of heat.

Consequently, one equivalent of potassa = 47 grs., would disengage 15187·6 units of heat.

The liquors were sufficiently dilute to prevent any precipitation of chlorate of potassa.

In order to bring the determination of x' to what it should be in the reaction as it was effected in our experiment for the determination of R, it is necessary to correct the preceding number of the heat absorbed by the solution of the chlorate of potassa.

Now, we find that :—

1 gr. of chlorate of potassa, in dissolving, absorbs

I. . . .	69·64
II. . . .	71·70
Mean.	70·67 units of heat.

Multiplying by 122·5, the equivalent of chlorate of potassa, we have 8657·1 units of heat.

This number, added to 15187·6, gives 23844·7 units of heat to ex-

press the value of x', or the formation of the chlorate of potassa in the crystallised state.

We sought to verify this last number by more direct experiments, by causing a normal solution of potassa (diluted to the strictly necessary degree) to react on a solution of chloric acid previously saturated with chlorate of potassa. The chlorate of potassa was thus obtained directly in the solid state.

I thought I might neglect the small error due to the solution of a small quantity of chlorate by reason of the water brought by the solution measured in the pipette.

We had for each gramme of potassa,

I.	. . .	494·0
II.	. . .	495·6
Mean.		494·8 units of heat;

and for 1 equivalent of potassa = 47 grs., we leave 23256·1 units.

I have used this last number (which, however, differs very little from the preceding) in the calculations.

The values of R, x' and x'' being determined, as we have besides

$$A = 100960 \times 5$$
$$D = 76238 \times 5$$

then follows, after substitution of the various numbers,

$$x = R - A - x'' + (D - x') = -65234 \text{ units of heat.}$$

Such is, in fine, the heat absorbed by the union of chlorine and oxygen for forming chloric acid, remaining in dilute solution.

Oxygenous compounds of Phosphorus.

I have explained in detail the experiments relative to the determination of the heat brought into play in the formation of hypochlorous acid: the numbers which result from these experiments were important in my eyes, not only in themselves, but also because they must serve as the basis of operations for determining the heat of oxidation of a very great number of metalloïds, such as phosphorus, arsenic, &c.

In all the experiments in which recourse was had to hypochlorous acid as an oxidising agent, care was taken previously to saturate the hypochlorous acid with chlorine after its introduction into the eprouvette placed in the muffle of the calorimeter, before operating the chemical reaction. In this fashion we escaped the cause of error resulting from the incomplete expulsion of the chlorine liberated in the reaction.

Hydrated Phosphoric Acid (by red phosphorus).—The combustion of phosphorus in the state of phosphoric acid, operated under the influence of an excess of hypochlorous acid in solution, was executed by employing at first phosphorus under the red modification obtained in the state of purity by M. Schrœtter. I am indebted to the kindness of M. Dumas for the sample of red phosphorus which served for several experiments, and which had been prepared by M. Schrœtter himself. The employment of phosphorus in this amorphous state admits of direct, accurate, and easy weighing; and on account of the division of the matter, the chemical action is almost instantaneous.

The phosphorus was weighed in a small glass spoon, and then introduced into the hypochlorous acid of the eprouvette; it was agitated, and the indications of the mercurial column were noted.

This experiment presents two very distinct phases of a very unequal duration in regard to the heat disengaged. Thus it was necessary for the second phase, which lasted about an hour (whilst the other continued only four or five minutes) to guard against errors arising from cooling, by means of Rumford's precaution. A preliminary rough experiment indicated the initial temperature to which the calorimeter should be reduced below the surrounding atmosphere, in order that the heat afterwards developed by the first phase of the reaction might be as much lower than that of the surrounding air, as it should exceed it at the end of the second phase.

It is easy, moreover, to conceive the cause of these two distinct phases in the disengagement of heat, the phosphoric acid not passing immediately to the state of trishydrated phosphoric acid.*

The reaction by which the calorific effects are calculated, is

$$Ph + 5\,ClO + x\,Aq = PhO^5 + x\,Aq + Cl^5.$$

We have, therefore, for determining the value of x, or the heat of formation of 1 equivalent of trishydrated phosphoric acid, dissolved,

$$R = x - D,$$

D expressing here the quantity of heat disengaged by the decomposition of 5 equivalents of dilute hypochlorous acid.

We obtain for the reaction of 1 gr. of phosphorus,—

I.	. . .	6821
II.	. . .	6809
Mean.		6815 units of heat.

Whence

$$R = 32 \times 6815 = 218080 \text{ units of heat;}$$

and by substituting for D its value $5 \times 7370 = 36850$, we have for x, heat disengaged by the formation of 1 equivalent of dilute phosphoric acid

181230 units of heat.

Hydrated Phosphoric Acid (by ordinary phosphorus).—The phosphorus was employed in small fragments of 3 to 5 centigrammes,

* The test of the acid liquor after combustion constantly indicated a little monohydrated phosphoric acid; it was not to be hoped that the calorific effect due to its total hydratation could be ascertained; the greater part, nevertheless, arrived at the state of complete hydratation, as was proved by the reagents. It is impossible to completely avoid this cause of error which, moreover, is almost always reproduced in whatever conditions the anhydrous phosphoric acid, either ready formed, or nascent, is found in contact with the water.

I do not think that this cause of error can have any great influence on the actual number which expresses the heat of the combustion of the phosphorus passing to the state of trishydrated phosphoric acid in solution, in being referred to the phenomena proved in the calorimeter. This complementary calorific effect which might have escaped can evidently constitute only a very small fraction of the total heat observed. At all events the *relations* between the calorific numbers in which the formation of phosphoric acid intervenes, cannot undergo any sensible modification by the adoption of these numbers.

previously quickly dried by means of blotting paper. In order to ascertain the weight of the phosphorus employed, a ground flask, filled with carbonic acid and containing a certain number of pieces of dry phosphorus, was tared; the desired quantity was removed by means of a pointed platinum wire, and quickly introduced into the calorimetric eprouvette containing the hypochlorous acid; the flask, was instantly closed again, and immersed in a wide-mouthed vessel, into which passed a continuous current of dry carbonic acid. The difference of the two weighings gave the weight of the phosphorus used for the experiment.* The quantity of phosphorus burned in each experiment was between 0·03 and 0·05 grammes.

I found also that 1 gramme of phosphorus would give by the reaction of the hypochlorous acid,—

I. . . .	7788·0
II. . . .	7607·4
Mean.	7697·7 units of heat.

By making the calculation as above for red phosphorus, we find for the calorific equivalent of phosphoric acid in solution, produced from ordinary phosphorus, 209476 units of heat.

Now, in the case of red phosphorus, we arrived at the number 181250 units of heat.

The difference is 28246 units of heat; a number which would be only attributable to the latent heat of constitution of ordinary phosphorus compared with red phosphorus.

This difference is considerable, and of a nature to attract the attention of physicists; it exceeds the anticipations suggested by the comparison of the specific heat of ordinary phosphorus with that of red phosphorus recently determined by M. Regnault.

The following, indeed, are the results:

	Calorific equivalent.		
Red phosphorus .	181230	0·16981	between 57 and 208° F.
Ordinary phosphorus	209476	0·1887	„ 53 „ 65° F.

It is evident that the specific heat of red phosphorus is considerably greater than that of ordinary phosphorus. This result is in the direction of the phenomena observed with regard to carbon and sulphur in various states. But, in the case of phosphorus, the difference of the heats of combustion is greater than the differences which correspond to the modifications of the carbon, although this last body presents great differences in respect to specific heats.

Anhydrous Phosphoric Acid—A gramme of anhydrous phosphoric acid, in dissolving in water, disengages:—

I. . . .	265·5
II. . . .	262·0
Mean.	263·7 units of heat.

* The manipulation for determining the exact weight of ordinary phosphorus, was analogous to that which M. Silbermann and I followed for weighing potassium.

This number multiplied by 72 (the equivalent of anhydrous phosphoric acid) gives 18985 units of heat for the hydratation of anhydrous phosphoric acid.

If we subtract this latter number from 209476 which expresses the equivalent of hydrated phosphoric acid, the difference 190490 units of heat will express the calorific equivalent of anhydrous phosphoric acid resulting from ordinary phosphorus.

This number is much higher than the number 181408 deduced from the experiments of M. Abria; it is also higher than the number 183904 calculated according to Mr. Andrew's results. These physicists doubtless operated by oxidising a known weight of ordinary phosphorus; in this case, it would be easy to understand that they would have obtained too low results; if the whole of the phosphorus burned had not passed to the state of phosphoric acid.

Phosphorous Acid.—For determining the heat of formation of phosphorous acid, we relied, on one hand, on the heat disengaged by the formation of phosphoric acid already known, and, on the hand, on the heat disengaged by the peroxidation of phosphorous acid in solution under the influence of hypochlorous acid, which required a special experiment which we shall now describe. The difference of the two numbers should give the heat of combination of the phosphorus with the oxygen for forming phosphorous acid in solution, relying naturally on the principle of successive heats.

In order to ascertain the exact weight of phosphorous acid experimented on, we operated on a known weight of protochloride of phosphorus which was decomposed with an excess of water, which furnished a calculable quantity of hydrated phosphorous acid; the hydrochloric acid formed at the same time was likewise in calculable proportion, which enabled us to take account of the effect produced by hydrochloric acid mixed with phosphorous acid on one part of the hypochlorous acid employed in the calorimeter.

We have, indeed, for the reaction giving the weight of the phosphorous acid,—

$$Ph\ Cl^3 + 3HO + x\ Aq = PhO^3 + 3HCl + x Aq;$$

and for the calorimetric equivalent,

$$PhO^3 + 3HCl + 5ClO + x Aq = PhO^5 + 3HO + Cl^3 + x Aq.$$

It was not necessary to reproduce for each experiment a new quantity of phosphorous acid derived from a weighed quantity of protochloride of phosphorus; it was sufficient to measure the volume of the first liquor obtained, and to operate each time on a known fraction of that volume.

From the time of the conversion of the phosphorous acid into phosphoric acid, we observed in the indication of the mercurial column the same peculiarities noticed during the direct formation of phosphoric acid from free phosphorus; the same precautions must, therefore, be observed in the estimation of the heat disengaged.

The elements of the experiment are:

$$R = x + A - (D + E).$$

A, heat disengaged by the formation of 8 equivalents of water =103386
D, heat absorbed by the decomposition of 8 equivalents of dilute hydrochloric acid . . . . . =120576
E, heat disengaged by the decomposition of 5 equivalents of dilute hypochlorous acid . . . . . =36850

A weight of phosphorous acid, corresponding to 1 gramme of phosphorus, in passing to the state of dilute phosphoric acid disengages :

I. . . .	2773·2
II. . . .	2762·6
III. . . .	2771·9
Mean.	2769·2 units of heat.

Whence

$$R = 32 \times 2769{\cdot}2 = 88742 \text{ units of heat.}$$

By substituting in the last equation for the numbers A, D, and E, their numerical values, we have $x = 69082$ units for expressing the heat *disengaged* by the transformation of phosphorous acid into phosphoric acid, whence the calorimetric equivalent of phosphorous acid resulting from ordinary phosphorus

$$=209476-69081=140394,$$

(209476 being the calorific equivalent of phosphoric acid.)

Hypophosphorous Acid—To determine the heat of formation of hypophosphorous acid, we relied, on one hand, on the heat disengaged by the formation of phosphoric acid already known, and, on the other hand, on the heat disengaged by the peroxidation of hypophosphorous acid in solution under the influence of hypochlorous acid. The difference of the two numbers should express the heat of combination of the phosphorus with the oxygen, in order to constitute dissolved hypophosphorous acid.

To ascertain the exact weight of real hypophosphorous acid experimented on, I dissolved, in the eprouvette, a known weight of crystallised and previously analysed hypophosphate of baryta, then I liberated the hypophosphorous acid by means of a slight excess of dilute sulphuric acid. The eprouvette was afterwards introduced into the calorimeter, and the reaction of the hypochlorous acid on the solution of hypophosphorous acid holding in suspension sulphate of baryta was commenced.

In this reaction we observed the phenomena already noticed in the formation of phosphoric acid either from phosphorus or phosphorous acid. The operation presents two distinct phases; therefore the same precautions must be taken for estimating the heat disengaged.

The elements of the experiment are :—

$$R = x-D$$

Whence

D=29480 heat, disengaged by the decomposition of 4 equivalents of hypochlorous acid dissolved

A weight of hypophosphorous acid corresponding to 1 gramme of phosphorus, in passing to the state of dilute phosphoric acid, disengages

I. . . .	6022·50
II. . . .	5893·40
Mean. .	5957·95 units of heat,

Whence

$$R = 32 \times 5957{\cdot}95 = 190654 \text{ units}$$

and

$$x = 161174 \text{ units of heat,}$$

to express the heat *disengaged* by the transformation of hypophosphorous acid into phosphoric acid.

It follows from this that the calorific equivalent of hypophosphorous acid arising from ordinary phosphorus—

$$= 209476 - 161174 = 48302$$

(209476 being the equivalent of phosphoric acid).

(To be continued.)

PHARMACY.

Infusion of ROASTED ACORNS *associated with* SULPHATE *of* QUININE.

By M. THOULOUSE.

M. THOULOUSE published, some months ago, a note, from which it results that an infusion of acorns prepared in the same manner as coffee, and used as a vehicle for sulphate of quinine, instantly destroys the bitterness of this latter medicament, without affecting its therapeutical action. The mixture of these two substances filtered and dried, gives as a residue a brown amorphous powder, incomparably less bitter than the *tannate of quinine* of commerce.

What gave him the idea of the combination was the impossibility of procuring, in the neighbourhood, tannate of quinine. He had a patient who had attacks of intermittent fever coincident with severe gastric pains, and who dreaded sulphate of quinine. The new combination succeeded beyond all hopes; not only the attacks of fever, but also the gastralgia gave way after the first doses of the medicament.

M. Thoulouse, senior, then thought of experimenting on himself with coffee of acorns, and he observed that this preparation diminished the secretion of urine in a perceptible manner. He advised the use of it to one of his patients affected with relaxation, which nearly approached incontinence. The secretion of urine diminished to such a point that the necessity was not felt more than once or twice during the ten hours following the ingestion of the coffee-medicament. But this secretion returned to the same degree as before when these ten hours were passed. M. Thoulouse, junior, himself advised the same infusion, in analogous conditions, and his patient found great benefit from it. Fresh observations are necessary to appreciate the effect of this preparation, either in incontinence of urine arising from relaxation of the bladder, or in abnormal secretions of the kidneys (glucosuria, albuminuria).

We know from M. Desvouve's observations, that tannic acid has the property of concealing the bitterness of cinchona and its preparations; it was therefore desired to find a substance which, containing tannic

acid, should be easy of preparation and cheap, and which, mixed with sulphate of quinine, should have none of the inconveniences of the other preparations of cinchona, which cannot be endured by delicate stomachs, and which inspire children especially with insurmountable disgust. Acorn coffee appears to fulfil all these conditions. This will be perceived by the following experiments :—

1st. Litmus paper, steeped in the infusion of roasted acorns, reddens perceptibly, which proves that this infusion is acid.

2nd. By mixing 1 gramme of sulphate of quinine with about 150 grammes of concentrated infusion of roasted acorns, and filtering shortly after mixing, a residue was obtained of 2 decigrammes of an aromatic, brown, amorphous substance containing a few needles which, however, soon disappeared. A small quantity of this powder, suspended in distilled water, gave rise to a completely neutral, insipid mixture.

3rd. This powder, at first insoluble, acquired, after a few days, a slight degree of solubility, loses its aroma, and has the peculiar bitterness of the tannate of quinine of commerce. It slowly restores the color of litmus when it has been reddened by a dilute acid.

4th. A crystal of sulphate of iron steeped in distilled water, holding in suspension a little of this powder, becomes black after two or three minutes, proving that this powder contains tannic acid.

5th. When this powder has recovered its bitterness, and only then, it dissolves perfectly in citric acid.

6th. The solution of chloride of barium, put in contact with this powder dissolved in citric acid, produces a slight cloudiness ; it forms, after some hours, a proportionally small precipitate, insoluble in an excess of nitric acid, whence it follows that the sulphuric acid has been, if not entirely, at least in great part, replaced by tannic acid, and that the mixture in question is a true neutral tannate of quinine.

The infusion of roasted acorns being a medicament within the reach of every one, and adding nothing to the price of sulphate of quinine, the whole may be reduced, in the greater number of cases, to the cost of the sulphate and tannate of commerce. But M. Thoulouse thinks, moreover, that practitioners will not hesitate to give the preference to the new preparation, because of its insolubility at the moment of administration, especially when they see produced an effect which is much more durable than can be produced either by sulphate of quinine, too quickly eliminated from the economy in consequence of its solubility ; or by tannate of quinine, which, having been long prepared, and having become slightly alkaline, has acquired a solubility almost equal to the sulphate ; or again, when they wish to continue the use of a nearly inoffensive medicine, almost agreeable to the taste, in obstinate intermittent fevers, or in quartan ague.

Gaz. Med. de Toulouse.

BIBLIOGRAPHY.

THE DRUGGISTS' HAND-BOOK OF PRACTICAL RECEIPTS; A MANUAL FOR THE USE OF THE CHEMIST AND MEDICAL PRACTITIONER: Comprising—The London Pharmacopœia, in English; with the Medicines, their Uses and Modes of Preparation; and Formulæ for the Trade Preparations, Mineral Waters, Powders, Beverages, Dietetic Articles, Perfumery, Cosmetics, &c.; a Glossary of the Terms used in Chemistry and Medicine: including Old Names, Contractions, Vulgar and Scientific Denominations; with a Copious Index to all the Preparations. By THOMAS F. BRANSTON. Liverpool: Howell, 1853.

THIS is a most useful little book: its nature is sufficiently set forth in the above lengthy title—consequently, an analysis of its contents is unnecessary. The author has executed his very laborious task with much care, and has provided very fully for the wants of the class to which his work is addressed.

The Glossary will be found very useful, especially to those who may require to refer to old works or recipes. An ample Index also constitutes a very important feature in this work.

This hand-book will be especially valuable to the pharmaceutical chemist and medical practitioner, and very useful to most of the general public.

PROCEEDINGS OF SOCIETIES.

BRISTOL PHILOSOPHICAL AND LITERARY SOCIETY.

At the last private meeting of the Philosophical and Literary Society, held at the Institution on Thursday, November 25th (W. Herapath, Esq. President, in the Chair), the following papers were read and discussed:—

1.—"On the presence of fluorine in the feathers of birds, and on the existence of minute traces of this element in hair and wool," by Mr. Thornton J. Herapath.

2.—"On a new method of detecting hydrocyanic acid (*prussic acid*) in cases of poisoning, and on the conversion of that compound into hydrosulphocyanic acid in the body by the hydrosulphuric acid and sulphide of ammonium evolved during putrifaction," by Messrs. William and Thornton J. Herapath.

Abstracts of these communications will shortly be published in "THE CHEMIST."

At the close of the evening, the Messrs. Herapath exhibited to the members present specimens of various minerals, &c. that had been lately sent to them for analysis. Amongst these were samples of tin-ore (stannolite, or oxide of tin), containing rubies and sapphires, from "The Ovens' gold-diggings, Australia; Hatchettine, or mineral adipocire, from Merthyr Tydvil; native carbonate of magnesia, or massive baudisserite, from Turkey; and fine specimens of the common ruby, or specular iron-ore, and native sulphuret of antimony, from Ireland. They also exhibited two samples of sewage-manure, which had been recently prepared from town sewage by their patent process; it was quite inodorous.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

(Concluded from page 128).

On the Properties and Composition of the Coca Leaf.

By Professor JOHNSTON.

After describing the remarkable physiological properties of the leaves of this plant, the author explained that they yield to ether a peculiar volatile resinous substance possessed of a powerful odor, in which the peculiar virtues of the leaf are supposed to reside. The plant is as yet to be obtained in too small quantities in this country to admit of a complete chemical examination of the subttances which the leaves contain.

Report on the Gases evolved in Steeping Flax, and on the Composition and Economy of the Flax Plant. By Professor HODGES.

The investigations directed by the Association, at the Belfast Meeting, with respect to the gases evolved in the steeping of flax and the composition of flax straw are in progress, and will be reported at the next Meeting. The gases of the fermenting vat have been analysed by the methods of Prof. Bunsen, and have been found to consist of carbonic acid, hydrogen, and nitrogen. No sulphuretted hydrogen has, in any case, been detected. Several analyses of the proximate constituents of the dressed fibre and of its inorganic ingredients have been made, which shew that a considerable amount of the nitrogenised and other constituents of the plant are retained in the fibre even after steeping and dressing have removed the structures unsuitable for textile purposes.

CHEMICAL DISCUSSION SOCIETY, DEC. 8, 1853.

Mr. J. WILLIAMS in the Chair.

Mr. J. SPENCER read a paper on an error which has lately occurred in the Journal of the Photographic Society, respecting the method of determining the strength of the Nickel bath employed in the Collodion process. The paper was ordered to be forwarded to the Editor of the "Photographic Journal" for his use.

Mr. J. SPENCER then exhibited and explained a test for Photographic purposes, made of black calico lined with yellow, and adapted to the legs of the camera stand; it folded into the space of a small knapsack, was light and inexpensive, and sufficiently roomy for developing in.

Mr. J. WILLIAMS read a paper "On the purity of certain Chemicals," which was ordered to be printed. (*See* page 210.)

Mr. SPENCER considered that the simple addition of an acid to the reduced iron under examination would answer the purpose described in the paper.

Mr. LINFORD objected that in small qnantity it was extremely difficult to recognise hydrogen with certainty.

Mr. J. WILLIAMS stated that he meant the test to be so striking in its characters, that persons, not analytical Chemists, should be able with certainty to distinguish between the two articles.

The Meeting adjourned until January 12th, 1854, and it was announced that a paper by Mr. J. Spencer on the Iodide of Quinine would then be read.

NOTES AND QUERIES.

TO THE EDITORS OF THE CHEMIST.

GENTLEMEN,—After your kind invitation to readers of "The Chemist," in No. II., New Series, to contribute notes, or make queries, I have ventured the following from my Note Book, Glasgow Laboratory, 1849, not having had the means of further investigation since.

Platinum made red hot, and then exposed in a current of coal-gas, has the property of maintaining itself in that state, as long as the current of gas is allowed to act upon it. The following is the *modus operandi*:—

A cylinder of thin iron covered at one extremity with fine wire gauze, is placed over a gas pipe; ignite the gas on the top of the gauze, and then invert a platina crucible on the top of the gauze, in the flame. After the crucible has become red hot, turn off the gas so as to extinguish the flame, and then turn the gas on again as quickly as possible, and you will observe that the platinum begins to glow with a brighter red, and will continue in a state of incandescence as long as the gas is allowed to stream upon it.

1. What is the action? Is it chemical, by the catalytic decomposition of the gas, or any of its constituents? or simply catalytic without producing any change?

2. Would the same phenomenon be the result, if coal or other CH gas were passed through a tube or sphere of platinum made red hot? If so, the soubriquet of my note book (viz., everlasting fire) would be somewhat *à propos*.

If the 2nd query be affirmed, I see no obstacle to employing globes of platina for stoves. Perhaps the gas thus employed may be found to serve two purposes better than one, by giving a serviceable heat and a better light. So much for hypothesis.

I trust I have not been expatiating on a property of platina, that has already been noticed by abler observers than myself; if I have, it is unwittingly. J. C.

Mineralogical Cutting Forceps.—Having found the form of cutting forceps recommended by Berzelius and Plattner in some cases insufficient in strength for their purpose, I have had a pair made to the pattern given below. They

resemble in form, surgeon's bone forceps, but are smaller; from the shape of the blades they are exceedingly powerful, cutting down quartz crystals readily, and from their being placed at an angle to the handle, are admirably adapted for cutting crystals out of geodes or crevices in large specimens.

SAMUEL HIGHLEY, *Fleet Street.*

PULVIS FERRI, OR QUEVENNE'S IRON.

With reference to our notice on this subject in lact month's Number, we have been requested to insert the following statement :—

On the Substitution of Magnetic Oxide of Iron for Iron in the Metallic State, reduced by Hydrogen.

BY MR. T. N. R. MORSON.

THE preparation which is the subject of the following observations is known as *Iron reduced by hydrogen—Fer reduit—Quevenne's iron*, &c., &c., and is described in the "Dublin Pharmacopœia" as *Pulvis Ferri*. It is, in fact, metallic iron in a finely divided state, and its chemical characters are those of iron filings, from which it differs only in its state of division. If acted upon by diluted sulphuric acid it evolves hydrogen gas, and forms a proto-salt. To prepare it, however, in a state fit for internal administration is not easy, and various processes have been suggested for this purpose, the most approved of which consists in the reduction of peroxide of iron by hydrogen gas at a red heat. It is rarely obtained free from a little sulphuret and carbon; but these impurities, however desirable it may be to avoid them, do not materially diminish its therapeutic action. It has acquired some importance from its employment, with most beneficial results, in many diseases requiring chalybeate medicines, its efficacy depending on its ready solubility in the weak acids of the stomach, a property which is not possessed by the oxides of iron, and especially by the magnetic oxide.

In the ordinary course of business we have furnished this article to our friends, when required, and amongst others to Messrs. Duncan, Flockhart, and Co., of Edinburgh, a firm well known, and of most high and deserved reputation. No complaint was made of the preparation until the 24th of September last, when we received a letter from those gentlemen inclosing two samples, one consisting of

"reduced iron,' as furnished by us, the other a black powder; with a request to be informed in what respect these differed, as both were sold under the same name, and the latter had been represented as superior to ours, and supplied in Edinburgh at one-third the price we had charged. No mention was made of the source from which the black powder had been obtained, nor any further information given. Upon examination I found the black powder to be magnetic oxide of iron, and wrote to my correspondent to that effect. Messrs. Duncan and Co. were satisfied with this reply, and afterwards informed me that the black powder had been furnished by Mr. Heathfield, as *Quevenne's Iron*. I heard no more on the subject until the 19th of November, when I received another letter from Messrs. Duncan and Co., inclosing a comparative analysis by Dr. George Wilson, of Edinburgh, of Heathfield's and of Morson's *Quevenne's Iron*, and representing the former as containing even per cent. more of iron in the metallic state than the latter. Shortly afterwards, an article appeared in "THE CHEMIST," at page 192 of the number of that Journal for December, to the same purport.

Feeling convinced that I was not in error respecting the nature of the substance sent to me by Messrs. Duncan and Co., and thinking it possible that the sample analysed by Dr. Wilson as *Heathfield's Quevenne's Iron, or reduced iron*, was not the same as that I had examined, I immediately wrote to Dr. Wilson, requesting him to favor me with a small portion of the article referred to in his report, which was being circulated to my disparagement. This letter remaining unanswered at the end of a week, I wrote again, and then received his reply, in which he referred me to Mr. Robertson, of Edinburgh, as the party for whom he had performed the analysis, at the same time declining to send me the sample. I then addressed Mr. Robertson to the same purport, but with no better success.

Failing in my endeavors to elucidate the truth in these directions, and having ascertained from Mr. Robertson that he had supplied to Messrs. Duncan and Co. some of *Heathfield's Quevenne's Iron*, taken from the same parcel as the sample sent to Dr. Wilson for analysis, I requested Messrs. Duncan and Co. to have this analysed by men of undoubted competence to decide the question at issue. Messrs. Duncan and Co. had also received, direct from Mr. Heathfield, some of his so-called *Quevenne's Iron*, and a sample of this, and also of that obtained from Mr. Robertson, was accordingly sent by Messrs. Duncan, for analysis, to Dr. Gregory of Edinburgh; to Dr. Stenhouse, of St. Bartholomew's Hospital; to Dr. Williamson, of the Birkbeck Laboratory; and to Dr. Garrod, of University College. The reports thus obtained, through a perfectly independent channel, from so many competent judges, will, I trust, be considered to have satisfactorily established the correctness of my original statement, and the injustice of the imputation which has been so unwarrantably circulated to my serious injury.

Reports of Analyses of Heathfield's and of Morson's Quevenne's Iron.

"To Mr. DUNCAN, "Dec. 16th, 1853.

"MY DEAR SIR,—I have carefully analysed the three specimens of Quevenne's Iron you handed me, and with the following results:

"Of No. 1, Heathfield's obtained direct, 10 grains yielded 10·75 of peroxide of iron. Had it been metallic iron, 10 grains should have yielded 14·3 grains of peroxide; but if black or magnetic oxide, of which it has the aspect and density, it would yield from 10 grains—

" If of the formula $Fe^3 O^4$ . . .	10·3 grains
" " $Fe^4 O^5$ . . .	10·5 grains
It actually yielded . .	10·7 grains

"Hence this specimen is black oxide of iron almost pure. I have not been able to detect in it even a trace of metallic iron.

"No. 2, Heathfield's, from Mr. Robertson,—

" 12·1 grains yielded . . .	12·9 peroxide
12·1 of metal should yield . .	17·2 grains
12·1 of black oxide, $Fe^3 O^4$ . .	12·5 grains
12·1 of ditto, $Fe^4 O^5$ . . .	12·7 grains

"This is therefore identical with No. 1, and contains not a trace of metallic iron. The small discrepancy between the analyses and theory depends on the presence of a trace of some impurity too trifling to be worth notice in any way.

"No. 3, Morson's preparation. This, which has precisely the color and density proper to iron reduced by hydrogen, I have analysed for my own satisfaction. 10·3 grains of it yielded 13·9 of peroxide. 10·3 grains of pure iron should yield 14·7 of peroxide.

"It is therefore metallic iron, as it professes to be, its composition being—

"Iron	97·3
"Impurities, probably carbon, sulpher, &c., present in all iron (silica probably also)	2·7
	100·0

"It is not, I believe, possible to manufacture, on any considerable scale, iron in this shape purer than this specimen is.

"I have to apologize for the time that has elapsed, but I have found it impossible, although not a moment has been lost, to furnish these analyses in such a manner that I could pledge myself for their accuracy, sooner than this morning. The mere washing of the precipitated peroxide has required gallons of distilled water.

"The above results are as exact as I can make them, and will, I am certain, be confirmed by any chemist who shall examine the same specimens.

"I remain, yours very truly,

"WILLIAM GREGORY, M.D, F.R.S.E.,

"Professor of Chemistry."

"St. Bartholomew's Hospital, 13th December, 1853.

"To Messrs. DUNCAN, FLOCKHART AND CO.

"GENTLEMEN,—I have carefully examined the two specimens of a preparation of iron, marked respectively '℥ij Heathfield's Quevenne's Iron obtained direct,' and '℥i Heathfield's Quevenne's Iron obtained from Mr. Robertson,' which you sent me a few days ago.

"I find that neither of them contains any metallic iron, but that they consist of a mixture of the protoxide and sesquioxide of iron in nearly atomic proportions, so as to form chiefly magnetic oxide of iron. They likewise contain a very appreciable amount of carbonaceous matter.

"I remain, Gentlemen, your obedient servant,

"JOHN STENHOUSE, LL.D., F.R.S.,

"Lecturer on Chemistry to St. Bartholomew's Hospital."

"University College, London, 9th November, 1853.

'To Messrs. DUNCAN, FLOCKHART, AND CO.

"GENTLEMEN,—I had the pleasure of receiving to-day your letter of yesterday enclosing a specimen of preparation labelled 'Heathfield's Quevenne's Iron;' and in reply to your inquiry regarding the nature of the substance I have to state, that it consists essentially of sesquioxide and protoxide of iron, and corresponds in its chemical deportment, to the 'magnetic oxide of iron,' mixed with a small quantity of carbon.

"I am, Gentlemen, yours obediently,

"A. W. WILLIAMSON,

"Professor of Practical Chemistry in the Birkbeck Laboratory."

"63, Harley Street, London, December 12th, 1853.

"To Messrs. DUNCAN, FLOCKHART AND CO.

"GENTLEMEN,—I have received a sealed packet containing a black powder, and labelled 'Quevenne's Iron (Heathfield)," which I find to be the magnetic oxide of iron with a trace of sulphuret, and not metallic iron.

"It possesses the following properties :—

"1. It is attracted by the magnet.

"2. Of a black color.

"3. Dissolves in acids with but a trace of effervescence, due to the evolution of a little sulphuretted hydrogen.

"4. Its acid solutions are precipitated black with an alkali.

The Fer Réduit of Quevenne, or Pulvis Ferri of the Dublin Pharmacopœia is,

"1. Attracted by the magnet.

"2. Of a steel grey color.

"3. Dissolves with a powerful effervescence in acids, and evolves hydrogen, usually mixed with a little sulphuretted hydrogen, from the preparation containing some sulphuret.

"4. Its acid solutions are precipitated green with an alkali.

"I have extensively employed the Fer Réduit in medicine, both in private and hospital practice, and think that the black or magnetic oxide of iron should not be substituted for the metallic iron.

"I am, Gentlemen, your obedient servant,

"A. B. GARROD, M.D.,

"Professor of Materia Medica and Therapeutics at University College Hospital."

PULVIS FERRI, OR QUEVENNE'S IRON.

The following is Dr. Wilson's Analysis of Heathfield's and Morson's Quevenne's Iron alluded to in the concluding Article of our last Number.

"Laboratory, Surgeon's Hall, Nicholson Street, 14th Nov., 1853.

"From the accompanying detailed analyses it appears that neither Heathfield's nor Morson's Quevenne's Iron is pure iron, but that Heathfield's makes the nearest approximation to it.

"Quevenne's Iron, theoretically, is pure metallic iron, but in actual fact probably never makes more than a distant approximation to it. The specimens sent to me, contain, in addition to hygrometric or accidental moisture, oxide of iron, sulphuret of iron, and an insoluble, incombustible substance which has not been minutely examined, besides metallic iron, of which the specimens should alone consist.

"The moisture has no doubt been absorbed from the atmosphere by the finely divided powder. The oxide of iron results either from the hydrogen gas not being passed over the original oxide from which the substance is prepared for a sufficient length of time, or from the substance being taken out of the tube in such a condition that it can take back oxygen from the air.

"The sulphuret of iron is derived either from the ordinary commercial oxide of iron containing some sulphate of iron having been employed to yield the Quevenne's iron, or from the hydrogen passed over the oxide containing sulphuretted hydrogen.

"The insoluble matter was, I presume, purposely added, and something, I think, is prescribed to be added, by the 'Dublin Pharmacopœia.'

"So far as *relative* purity is concerned, Heathfield's specimen is decidedly the better of the two. Compared with Morson's it contains more iron, and less oxygen, less sulphur, less insoluble matter, and less water. If the amount of metallic iron present be made the standard of purity of a substance professing to be all metallic iron, then Heathfield's sample is 7 per cent. purer than Morson's.

"Analyses of two samples of Quevenne's Iron, received from J. Robertson, Esq., Druggist, George Street, Edinburgh.

No. 1. Morson's.
No. 2. Heathfield's.

	No. 1.	No. 2.
"Metallic Iron	71·35	78·55
Sulphur (as sulphuret of Iron)	0·51	0·41
Water	2·01	1·42
Insoluble in acids	2·52	1·24
Oxygen, and loss in analysis	23·61	18·38
	100·00	100·00

"GEORGE WILSON, M.D.,

"Lecturer on Chemistry."

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the ADULTERATIONS *of* OILS. By Professor F. CRACE-CALVERT.

IN consequence of the large quantities of oils used at the present day for machinery, woollen, &c., many varieties are introduced into the market, and thus much temptation exists to mix or adulterate those which are most expensive. Having been frequently called upon to examine samples of oils, I have ascertained that the known processes for the discovery of adulterations were too general in their application to enable me to obtain satisfactory results. To this class belongs the delicate process recommended by M. F. Boudet, for the detection of drying oils in olive oil by the action of hyponitric acid and M. Rousseau's diagometer, which is based on the very inferior conducting power of olive oil as compared with that of the others.

For distinguishing one class of oils from another, we have M. Fauré's method, which consists in the brown or black tinge which fish oils exclusively assume when a stream of chlorine gas is passed through them; or M. Maumené's, by which the drying may be distinguished from the non-drying oils, owing to the fact that the latter, when mixed with strong sulphuric acid, give rise to a much higher temperature; and although M. Fehling has endeavored lately to give more precision to M. Maumené's researches, even his results are yet far from being satisfactory.

There are other processes, the characters of which are not sufficiently distinct to be employed with any degree of certainty: such as M. Fauré's process, which consists in adding a given quantity of caustic ammonia to oils, and noticing, after they have been mixed, the peculiar appearance which the white or yellow thick fluid presents. The same may be said of the process proposed by M. Heidenreich with monohydrated sulphuric acid, or that of M. Diesel with concentrated nitric acid; for the chemical actions are so violent, that the characteristic colorations which are at first produced, rapidly disappear in consequence of the destruction of the oil.

These facts induced me to ascertain what would be the action, on oils, of the above acids, when diluted, and the satisfactory results obtained are hereafter described.

The marked colorations produced may be considered as derived from two distinct chemical actions:—

1st. They appear due to certain foreign matters which are dissolved in them, and which existed in the substance from which they were extracted.

2ndly. That the diluted acids have, probably, an action on the component parts of the oils themselves; for if caustic soda be added to oils so acted on, a different result is obtained than when no acid has been previously applied: this fact being clearly illustrated with French nut-oil, as it gives a semi-saponified fluid mass when caustic soda of sp. gr. 1·340 is alone mixed with it, and a fibrous mass when treated by dilute nitric acid previous to the addition of the alkali.

It may be interesting to remark, that fish oils present reactions which are quite distinct from other animal or vegetable oils, consequently, in my opinion, not only has cod-liver oil a different composition from that of other oils, as shewn by the researches of M. Winckler, but also probably sperm and seal oils. The most difficult part of my researches has been to procure oils the purity of which I could depend upon; and to arrive at this object, I was obliged in many cases to obtain samples from their sources of production on the Continent, and even then I took the precaution to ascertain their degree of purity, by applying to them the various tests which I shall describe further on. The reason I employed so many reagents is, that the adulterations which commercial interests may dictate are numerous, and that the reactions presented by organic substances, and especially oils, are exceedingly delicate. I would strongly recommend that samples of pure oil be tested comparatively with those suspected of being adulterated, and never to apply one only of the proposed tests, but all those giving characteristic reactions with a given oil.

I have great pleasure in acknowledging the intelligence, industry, and chemical knowledge exhibited in these researches by my assistant, Mr. Charles Gowe.

As the reactions presented by the various oils depend upon the special strengths and purity of the reagents, not only great care should be exercised in their preparation, but also the exact mode and time required for the chemical action to become apparent: these particulars I have taken care to give with each reagent.

Caustic Soda in solution. Sp. gr. 1·340.—The reactions given in the following table were obtained by adding one volume of the above test-liquor to five volumes of oil, well mixing them, and heating the mixture to its point of ebullition:—

Dark Colorations.				Light Colorations.			
Fish Oils.		Vegetable Oils.		Animal Oils.		Vegetable Oils.	
Sperm, Seal, Cod-liver	Red.	Hempsd.	thick brwn. yellow	Neatsft.	dirty yellowish white	Pale rapeseed, Poppy, French nut, Sesame	dirty yellowish wht
		Linseed	fluid yellow	Lard	pinkish white	Castor, India nut (thick)	white
						Gallipoli Olive	yellw.

Caustic soda of sp. gr. 1·340 is principally useful to distinguish fish

from other animal and vegetable oils, owing to the distinct red colors which the former assume, and which coloration is so distinct that 1 per cent. of fish oil can be detected in any of the others. This table should also be consulted when the question is not to discover adulteration, but to distinguish some of the oils; for instance, hempseed acquires a brown yellow color, and becomes so thick that the vessel which contains it may be inverted without losing any of its contents; whereas linseed assumes a much brighter yellow color, and remains fluid. India nut oil is characterised by giving a white mass, becoming solid in five minutes after the addition of the alkali, which is also the case with gallipoli and pale rape, to the exclusion of the other oils which remain fluid.

Although it is probable that the reason some of the oils on the application of this reagent acquire a mucilaginous appearance, whilst others become stringy or fibrous, is due to the greater or lesser facility with which they are saponified; still I regret that I have not the time to examine this point carefully.

Action of diluted sulphuric acid on oils.—As different strengths of this acid had distinct reaction on the oils I had at my disposal, and as they may be employed to discover some known commercial adulterations, I shall discuss separately each series of reactions:

Sulphuric Acid of sp. gr. 1·475.—The mode of applying this acid consists in agitating one volume with five volumes of oil until complete admixture takes place, and then allowing the whole to stand for fifteen minutes, when the appearance is taken as the test reaction.

Not Colored.		Colored.					
Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Lard, dirty	Indian nut Pale rape seed Poppy Castor	Sperm Seal	light red	Neatsft	yellw tinge	Olive Gallipoli Sesame	green tinge
		Cod liv	purple			Linseed Hemp.	green intense green
						French nut	brown.

The most striking reactions in this table are those presented by the oils of Hemp and Linseed, for the green coloration which they acquire is such, that if they were used to adulterate any of the other oils to the amount of 10 per cent., their presence would be indicated by the distinct green tinge they would communicate. The red color assumed by the fish oils with this test is also sufficiently marked to enable us to detect them in the proportion of 1 per cent. in any other oil; and it is at the point of contact of the oil with the acid on their being allowed to separate by standing, that the color is principally to be noticed.

Sulphuric Acid of sp. gr. 1·530. Having obtained, by the application of the preceding acid, a certain number of characteristic reactions, I was induced to try the influence of a stronger one, and therefore agitated one volume of it with five volumes of oil, and allowed the mixture to stand for five minutes.

Light Colorations.				Dark Colorations.			
Animal.		Vegetable.		Fish.		Vegetable.	
Lard	dirty white	Olive	greenish white	Sperm Seal	red	Gallipoli French nut	grey
Neatsf.	brownish dirty wh.	Sesame	greenish dirty white	Cod-liver	purple	Hempseed	intense green
		India nut Poppy Castor	dirty white			Linseed	dirty green
		Palerape-seed	pink				

As hempseed, linseed, fish, gallipoli and French nut are the only oils that assume with the above reagents decided colorations, they can be discovered in any of the others.

Sulphuric Acid of sp. gr. 1·635. This acid was used in a similar manner to the above, and the coloration noted after two minutes.

Not Colored.	Distinctly Colored.							
Vegetable.	Fish.		Animal.		Vegetable.	
Poppy Sesame Castor	Sperm Seal Cod liver	intense brown	Neatsfoot Lard (light)	brown	Olive (light Hemps. (intense) Linseed	green
					Gallipoli Pale rapeseed French nut India nut (light)	brown

I wish to draw especial attention to this acid, as it gives distinct reactions widely differing from those of the former acids. The colorations produced by sulphuric acid sp. gr. 1·635, are so marked that they may be consulted with great advantage in many cases of adulteration. For example, I have been able to detect distinctly 10 per cent. of rape in olive oil; of lard in poppy; of French nut in olive; of fish oil in neatsfoot.

I was much struck by the gradual increase of coloration assumed by some of the oils when treated by sulphuric acid of different strengths. Thus, I found that gallipoli which was white with No. 1 sulphuric acid, becomes brown with No. 3. Pale rape seed, which was white with No. 1 acid, gives a pink color with No. 2, and a brown with No. 3; whilst neatsfoot is of a light yellow with No. 1, but becomes brown with No. 3. These results, therefore, clearly shew the decomposing action of the sulphuric acids on oils; and that an acid of sp. gr. 1·635 is the maximum strength that can be used, for when stronger, nearly all the oils begin to carbonise, and their colorations are destroyed.

Action of Nitric Acid of different strengths on Oils. — Owing to the reasons given in the first part of this memoir, I employed diluted acid, and obtained a series of reactions, some of which will, I hope, prove useful in some special cases of adulteration, and interesting, as shewing the influence of gradual oxidation on oils.

Nitric Acid of sp. gr. 1·180. One volume of this acid was agitated

with five volumes of oil, and the appearance, after standing five minutes, is described in the following table.

Not Colored.			Colored.					
Fish.	Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Cod-liver	Lard	Indian nut	Sperm	slight yellow	Neatsf.	light yellow	Olive Gallipoli	grensh.
		Pale rapeseed						
		Poppy	Seal	pink			Hempseed	dirty green
		Castor					French nut	yellow
							Sesame (orge)	
							Linseed	

This test is sufficiently delicate to detect distinctly 10 per cent. of hempseed oil in linseed oil, as the latter assumes the greenish hue so characteristic of the former. Although olive oil acquires also a green color, still its shade is such that it is easily distinguished from that of hemp seed.

Nitric Acid sp. gr. 1·220.—I employed this stronger acid with the view of increasing the colorations of certain oils, so as to render them sufficiently marked to ascertain the presence of these oils when mixed with others. The proportions used and the time of contact were the same as above.

Not Colored.			Colored.					
Fish.	Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Cod-liver	Lard	India nut	Sperm	light yelw	Neatft.	light yelw	Poppy (yell.)	red
		Pale rapesd.					French nut	
		Castor	Seal	light red.			Sesame	
							Olive Gallipoli	greenish
							Hempsd.	greenish dirty brown
							Linseed	yellow.

The chief characters in the above table are those presented by hempseed, sesame, French nut, poppy, and seal oils, and they are such that not only may they be employed to distinguish these oils from each other, but are sufficiently delicate to detect their presence when mixed with other oils in the proportion of 10 per cent.

Nitric Acid of Sp. gr. 1·330.—One volume of this acid was mixed with five volumes of oil, and remained in contact five minutes :—

Not Colored.	Colored.				
Vegetable.	Fish.	Animal.		Vegetable.	
Indian nut	Sperm	Lard	very slight yellow	Poppy	red
Pale rapesd.	Seal	Neatsft.	light brown	French nut (dark)	
Castor	Cod-liv			Sesame (dark)	
				Olive Gallipoli	greenish
				Hempseed	greenish dirty brown
				Linseed	green, becoming brown.

The colorations here described are very marked, and can be employed with advantage to discover several well known cases of adulteration; for instance, if 10 per cent. of sesame or French nut oil exists in olive oil: as for poppy with sesame oil, the tinge produced not being so vivid as the others, an adulteration cannot be detected with certainty; and supposing that any doubt remained in the mind of the operator as to whether the adulterating oil was sesame, French nut, or poppy, he would be able to decide by applying the test described in the next table, where he will find that French nut oil gives a fibrous semi-saponified mass, sesame a fluid one with a red liquor beneath, and poppy also a fluid mass, but floating on a colorless liquor.

The successive applications of nitric acid of sp. gr. 1·330, and of caustic soda of sp. gr. 1·340, can also be successfully employed to detect the following very frequent cases of adulteration:—

1st. That of gallipoli with fish oils, as gallipoli assumes no distinct color with the acid and gives with soda a mass of fibrous consistency, whilst fish oils are colored red, and become mucilaginous with the alkali.

2nd. That of castor with poppy, as the former acquires a reddish tinge, and that the mass with the alkali loses much of its fibrous appearance.

3rd. That of rape seed with French nut, as nitric acid imparts to the former a more or less red tinge, which an addition of the alkali not only increases, but also renders the semi-saponified mass more fibrous.

The colorations which various oils assume under the influence of the three above-mentioned strengths of nitric acid, clearly illustrate the remarks made at the commencement of this paper; and that the cause of the unsatisfactory results obtained by those chemists who have preceded me in these tedious researches for distinguishing oils and their various adulterations, is, that owing to the acids they employed being highly concentrated, all the distinctive colorations were lost, the oils becoming yellow or orange; but there is no doubt that the above reagents will enhance the value of M. F. Boudet's process, as they afford very useful data to point out the special oils mixed with olive.

Caustic Soda of Sp. gr. 1·340.—The following reactions were obtained by adding ten volumes of this test-liquor to the five volumes of oil which had just been acted upon by one volume of nitric acid:—

If a fibrous mass is formed.			If a fluid mass is formed.			
Animal.		Vegetable	Fish.	Animal.	Vegetable.	
Neatsft.	white	Gallipoli	Sperm	Lard	Olive	white
		Indian nut	Seal		Pale rapeseed	white
		Castor	Cod-liv.		Linseed	yellowish
		French nut				
		Hempseed			Poppy (light)	red
					Sesame (br. liquor beneath)	red

Having given, in a previous paragraph, some of the most useful reactions contained in this table, I shall simply call attention to the following mixtures :—"Neatsfoot with Rape," "Gallipoli with Poppy," "Castor with Poppy," "Hempseed with Linseed," "Sperm with French nut," and "Gallipoli with French nut oils." It is also necessary here to mention that the brown liquor on which the semi-saponified mass of sesame swims, is a very delicate and characteristic reaction.

Phosphoric Acid.—One volume of syrupy trishydrated phosphoric acid was agitated with five volumes of oil, and gave the following results :—

Not Colored.		Colored.			
Animal.	Vegetable.	Fish.		Vegetable.	
Lard	India nut	Sperm	dark brown	Olive (slight)	green
Neatsfoot	Pale rapeseed	Seal		Gallipoli (slight)	
	Poppy	Cod liver		Hempseed	
	Sesame			Linseed (brown yellow)	
	Castor			French nut	brown yellow

The only reaction to be noticed is the dark red color rapidly becoming black, which phosphoric acid imparts exclusively to the fish oils, as enabling us to detect 1 part of these oils in 1000 parts of any other animal or vegetable oil ; and even at this great degree of dilution a distinct coloration is communicated to the mixture.

Mixture of Nitric and Sulphuric Acids.—The results given in the following table were obtained by agitating one part by measure, of a mixture of equal volumes of sulphuric acid of sp. gr. 1·845, and nitric acid of sp. gr. 1·330, with five volumes of oil, and allowing the whole to stand two minutes :

If Colored.					
Fish.		Animal.		Vegetable.	
Sperm	dark brown	Lard	brown	Gallipoli	dark brown
Seal		Neatsf. (dark)		Rapeseed	
Cod liver				French nut	
				Sesame (bec. intense red)	green
				Hemps. (becoming black)	
				Linseed (becoming black)	
				Olive (orange)	yellw
				Poppy (slight)	
				India nut (orange)	white
				Castor	brownish red

As three oils remain nearly colorless, viz., those of poppy, olive, and India nut, we are enabled to detect in them the presence of any of the others ; and when olive or poppy are adulterated with sesame, the green color at first produced is much more persistent than with

sesame alone, consequently it is necessary that the acid and the oil suspected of containing it, should remain in contact for about ten minutes, in order to obtain the ultimate brownish red color of the sesame; in fact, it is so intense, that it may be usefully employed to detect this oil when mixed with others.

Nitro-hydrochloric Acid. In consequence of the results obtained with nitric acid, I was induced to try the action of nitro-hydrochloric acid, but I found that when it was composed in the ordinary way of three volumes of hydrochloric, and one volume of nitric acid, the reactions produced nearly coincided with the last named acid. I, therefore, prepared several strengths of nitro-hydrochloric acid, in which I gradually increased the proportion of hydrochloric acid, and, after having tested them, I adopted one composed of twenty-five volumes hydrochloric acid of sp. gr. 1·155, and one volume of nitric acid of sp. gr. 1·330, and allowed it to stand about five hours. The reactions described in the following table are those which took place when a mixture of five volumes of oil with one volume of aqua regia was agitated and allowed to stand five minutes.

Not Colored.		Colored.				
Animal.	Vegetable.	Fish.		Animal.	Vegetable.	
Lard.	Olive Gallipoli Indian nut Rapeseed Poppy Castor	Sperm (slight) Seal (slight) Cod liver	yellow	Neatsf. slight yellow	French nut Sesame Lins. (green.)	yellow
					Hemps. greenish	

When the facts contained in this table are compared with those preceding it, we are struck with their uniformity, and are led to infer that no marked action had taken place; but this conclusion is erroneous, as most of them assume a vivid and distinct coloration on the addition of an alkali of sp. gr. 1·340, as seen in the following table:—

If a fibrous mass is formed.				If a fluid mass is formed.		
Animal.	Vegetable.		Fish.	Animal.	Vegetable.	
Neatsf brownish yellow	Gallipoli (yellowh) India nut Rapeseed (yellowh)	white	Sperm Seal Cod-liver	orge. yel.	Lard, pink	Olive, white
					Poppy intense pink.	
	Castor pale rose					
	French nut, orange				Sesame orange with brown liquor beneath.	
	Hempseed light brown				Linseed, orange.	

The characters presented in this table permit us to discover with facility 10 per cent. of a given oil in many cases of adulteration, for example—poppy in rape, olive, gallipoli, and India nut, as all of them assume a pale rose color; but when poppy is mixed with olive or castor oils, there is a decrease in the consistency of the semi-saponified mass. By the aid of the above reagents we can also ascertain the presence of 10 per cent. of French nut in olive or linseed oils, as the semi-saponified mass assumes the same appearance as the last, and as to French nut in pale rape, gallipoli, and India nut, it is recognised in consequence of their white masses acquiring an orange hue. With respect to linseed in hempseed oil, it is detected as it renders the fibrous mass of the latter more mucilaginous. Sesame also gives with this reagent the same characters as those which it afforded with nitric acid and an alkali; and poppy is distinguished from all other oils by giving in this case a semi-saponified mass of a beautiful pink color.

To give an idea how the above are to be used, I shall suppose a sample of rapeseed oil adulterated with one very difficult to discover; namely, lard oil. I first apply the caustic alkali test, which, in giving a white mass, proves the absence of the fish oils, together with those of hempseed and linseed, and as no distinct reactions are produced by the oils under examination, when mixed with the three sulphuric and nitric acids above-mentioned, poppy and sesame will then be thrown out as they are reddened, neatsfoot, India nut, castor, olive, and lard resting only in the scale of probabilities. In order to discover which of these is mixed with the suspected oil, I agitate a portion of it first with nitric acid of sp. gr. 1·330, and then with caustic soda, and their mutual actions exclude neatsfoot, India nut and castor oils, as the sample does not give a fluid semi-saponified mass. The absence of olive is proved by no green coloration being obtained on the application of syrupy phosphoric acid. As to the presence of lard oil in the rape oil, it is ascertained on caustic soda being added to the oil which has been previously acted on by nitro-hydrochloric acid, as the latter gives a fibrous yellowish semi-saponified mass, whilst the former yields a pink fluid.

In conclusion, I trust that the reagents described in this paper, and the new method of applying successively two of them to any particular oil, will prove useful not only to detect the numerous admixtures of oils we have noticed, but also to trace and determine in a given oil the presence of any of the others which we have examined.

I here give a general table of the preceding reactions, in order to facilitate the discovery of any adulteration :—

GENERAL TABLE OF REACTIONS.

OILS.	Caustic Soda Sp. gr. 1·340	Sulphuric Acid Sp. gr. 1·475	Sulphuric Acid Sp. gr. 1·530	Sulphuric Acid Sp. gr. 1·635	Nitric Acid Sp. gr. 1·180	Nitric Acid Sp. gr. 1·220	Nitric Acid Sp. gr. 1·330	Caustic Soda Sp. gr. 1·340 +	Phs. Acd syrupy	Sul. Acid + Nit. Acid	Nitrohydrochlor. Acid	Caustic Soda Sp. gr. 1·340 +
Olive	slight yellow	green tinge	greenish white	light green	greenish	greenish	greenish	fluid white mass	slight green	orange yellow	0	fluid white mass
Gallipoli . .	ditto	ditto	grey	brown	ditto	ditto	ditto	fibrous ditto	ditto	dark brown	0	fibrous yellowsh ish white mass
India Nut .	thick and white	0	dirty white	light brown	0	0	0	ditto	0	orange white	0	fibrous white mass
Pale Rapesd	dirty yellowish whte	0	pink	brown	0	0	0	Fluid white mass	0	dark brown	0	fibrous yellowish white mass
Poppy . . .	dirty yellowish whte	0	dirty white	0	0	yellowish red	red	light red fluid mass	0	slight yellow	0	fluid intense rose-col. mass
French Nut	ditto	brownish	grey	brown	yellow	red	dark red	fibrous red mass	brown yellow	dark brown	yellow	fibrous orange mass
Sesame . . .	ditto	green tinge	greenish dirty white	0	orange yellow	ditto	ditto	fluid red mass br.liq. beneath	0	green bec. inten. red	ditto	fluid orange mass br. liq. beneath
Castor . . .	white	0	dirty white	0	0	0	0	fibrous white mass	0	brownish red	0	fibrous pale rose-col. mass
Hempseed .	thick brown yelw	intense green	intense green	intense green	dirty green	greenish dirty brown	greenish dirty brown	fibrous light brown mass	green	green bec. black	green	fibrous light-br. mass
Linseed. . .	fluid yellow	green	dirty green	green	yellow	yellow	green becoming brn	fluid yellow mass	brown yel. green	ditto	grensh yellow	fluid orange mass
Lard	pinkish white	dirty white	dirty white	light brown	0	0	very slight yellow	fluid mass	0	brown	0	fluid pink mass
Neatsfoot. .	dirty yellowish whte	yellow tinge	brownish dirty white	brown	light yellow	light yellow	light brown	fibrous white mass	0	dark brown	slight yellow	fibrous brownish yellow mass.
Sperm . . .	dark red	light red	red	intense brown	slight yellow	ditto	red	fluid mass	dark red	ditto	ditto	fluid orange yellow mass
Seal. . . .	ditto	ditto	ditto	ditto	pink	light red	ditto	ditto	ditto	ditto	ditto	ditto
Cod-Liver	ditto	purple	purple	ditto	0	0	ditto	ditto	ditto	ditto	yellow	ditto

Method *of* Electrotyping Fishes, Ferns, *or other* Objects *of* Natural History.*

By James How.

In order to obtain perfect copies of fishes by the electrotype process, the moulds should be taken while they are yet alive, for as soon as they are dead, the scales drop, presenting a perfectly flat surface, so that however beautifully developed its scales may be during life, no accurate copy can be obtained. The fish is therefore taken as soon after it is caught as possible, and laid upon a slab of plate glass, a piece of slate, or any polished or smooth surface, and plaster of Paris mixed to about the consistence of cream, poured upon it, adding successive layers of the plaster until the mould becomes from $\frac{3}{4}$ to $1\frac{1}{2}$ inches in thickness, care being taken to allow the plaster to run a certain distance round the sides of the specimen so as to form a margin or base of a width proportionate to the size of the fish to be copied; the thickness of the mould must also be regulated by its dimensions; for instance, for a small trout or perch, the thickness of $\frac{1}{2}$ inch would be sufficient; whereas, for a large jack or small salmon, even two or three inches would be required, otherwise the mould when finished would be too liable to fracture. I have generally found it advisable to keep the glass on which the specimen is laid in constant motion while the plaster is being poured on, to prevent the formation of air-bubbles, which would inevitably spoil the mould.

Should any finished moulds be found to contain air-bubbles, or any imperfections in casting, it is advisable at once to reject them, as it is scarcely possible to obtain good electrotypes from them. As soon as that portion of the mould resting upon the glass is quite dry, it may be very readily removed without danger of breaking by slight pressure of the hand. The mould should now be laid upon its back or convex surface, in order to remove the fish; and as the back and belly of fishes are not level with the inner edges of the mould, but what is generally termed by electrotypists "undercut," it is found necessary, in order to get them out without spoiling the matrix, to cut the body with a sharp knife from head to tail, and remove the entrails, roe, &c., so that, by gently pressing both the edges towards the centre, it will give way, and the whole may be taken out without injury to the mould. These precautions will be advisable in practice, especially when very rare specimens are to be operated upon, as it is sometimes requisite to make several moulds before a perfect copy is obtained, although the second and third copies of the same are neither so sharp, nor so deeply marked in detail as the first, still with due care tolerable results may be arrived at.

To proceed with the preparation of Moulds, &c.—After having taken the fish from the mould of plaster, and having found it free from air bubbles and quite perfect, it should either be exposed to the sun or put

* Read before the "Chemical Discussion Society," July 28th, 1853.

into an oven or hot-air chamber of moderate temperature until perfectly dry; it is then to be laid in a vessel containing three parts white wax and one part stearine, melted together, which must be kept perfectly hot and fluid until the mould is thoroughly saturated, when it should be taken out and placed before a fire (resting its back on coarse blotting paper), to remove the superfluous wax, and so render sharp the fine lines, scales, fins of the fish, &c. A conducting surface is now given to the mould by brushing over it plumbago, or that which I have generally found a better conductor for all electrotyping purposes, precipitated silver (made by simply immersing a piece of clean zinc in a solution of nitrate of silver, and the precipitate thus formed well washed upon a filter and dried); either of these substances, however, should be well brushed over the surface to be coated, and the mould then attached to the conducting-wires, or copper-ribbon, which is, perhaps, better for these purposes, and especially for very large fishes, as there is not sufficient conducting surface in wire for large objects), and attached to the battery in connection with the precipitating trough. In addition to these ribbons or wires, it will be found necessary to use "guiding-wires," as they are termed, leading from the battery to the inner parts of the mould, the fins, eyes, mouth, &c., or any deeply-cut parts which the mould may contain.

Having now obtained a coating of sufficient thickness (which should be proportionate to the size and length of the specimen, as very large objects require a stout coating of copper to stand cleaning), it may be removed by cutting the plaster mould through the back in several places with a very fine saw, taking great care not to cut more than about half through the mould, and then breaking or chipping off the rest. By a careful operator, a dozen or more of these moulds may be removed from the electrotypes in the course of an hour without injury to the copy. After removing the mould the copy should be brushed with a good hard brush with soft soap and powdered rotten-stone; and sometimes it is advisable to use a little very dilute nitric acid, in order to get the electrotype perfectly clean.

Very pretty groups of this kind may be made for the cabinet by tastefully arranging several small specimens upon blades of grass, leaves or ferns, and taking a perfect copy of the whole, as before described. Should it be considered dangerous to pick out any remaining portions of leaves, which often adhere to the mould, they may safely be displaced by pouring into the mould a little boiling water, which will sufficiently shrink them to enable the operator to remove them with ease; this, of course, should precede the waxing process. It will be found that every sprig, and veining of each leaf, and the fishes upon them, will be accurately copied, the beauty of which may be very much enhanced by electro-plating or gilding, leaving the body of the fish in *dead* silver or gold, as it comes from the decomposition trough, and burnishing the plain surfaces, picking out with the burnisher the leaves and stalks, &c. Two or three burnishers of different shapes will be sufficient for ordinary purposes, and these can be obtained at any tool shop or ironmonger's.

CHEMICAL TABLES. COMPILED BY THORNTON J. HERAPATH.

No. I.—DENSITIES OR SPECIFIC GRAVITIES.

(Continued from page 203.)

When an * is attached to a name, it must be understood that the *preceding* numbers have been merely copied from some standard work, and are not the result of the Author's experiments.

Substances.	Density at 60° F.	Authority.
Gases and vapors:		
Iodine	8·7160	Dumas.
"	8·6829	T. H. cal.
Mercaptan	2·326	Bunsen.
"	2·1363	T. H. cal.
Mercury	6·9760	Dumas.
"	7·0300	Mitscherlich.
"	6·8912	T. H. cal.
" bromide	12·18	Mitscherlich
"	12·2665	T. H. cal.
" subchl. (calomel)	8·350	Mitscherlich.
"	8·1316	T. H. cal.
" chlor. (cor. sublimate).	9·800	Mitscherlich.
" "	9·372	T. H. cal.
" iodide	15·900	Mitscherlich.
" "	15·6054	T. H. cal.
" sulphide	5·510	Mitscherlich.
" "	5·4262	Gmelin cal.
Methylene	0·490	Dumas.
"	0·48238	T. H. cal.
Methylamine	1·130	Würtz.
"	1·075	T. H. cal.
Methyl	1·076	Kolbe & Frankland
"	1·0336	T. H. cal.
" oxide	1·570	Regnault.
" "	1·5849	T. H. cal.
Naphtha	3·400	Pelletier.
"	3·390	Gmelin.
Naphthaline	4·528	Dumas.
"	4·4104	T. H. cal.
Nicotine	5·6300	Barruel.
Gases and vapors:		
Nicotine	5·6370	Barruel, cal.
Nitric oxide	1·0388	Bérard.
" "	1·0416	Thomson.
" "	1·0960	H. Davy.
" "	1·1887	Kirwan.
" "	1·0338	T. H. cal.
Nitrogen	0·9426	Lavoisier.
"	0·9670	H. Davy.
"	0·9691	Biot & Arago.
"	0·972 to 0·9729	Dumas & Berzelius
"	0·985	Kirwan.
"	0·9647	T. H. cal.
Nitrous or Hyponitric acid	1·720	Mitscherlich.
"	1·5849	T. H. cal.
Nitrous oxide	1·3629	Berthollet.
"	1·5204	Colin.
"	1·5269	Thomson.
"	1·6140	Dalton.
"	1·51607	T. H. cal.
Œnanthole	4·0019	Bussy.
Oil-gas	0·910 to 1·175	Ure.
Oil of turpentine	4·880	Dumas.
	4·7633	T. H. cal.
Olefiant-gas	0·985	Saussure.
	0·9647	T. H. cal.
Oléine	2·942	Fremy.
"	2·8943	T. H. cal.
Oxygen	1·087	Fourcroy.
"	1·088	Allen and Pepys.
"	1·1026	Berzelius & Dulong

Substances.	Density at 60° F.	Authority.
Gases and Vapors.		
Oxygen	1·1030	Kirwan.
,,	1·1031	Buff.
,,	1·1036	Biot and Arago.
	1·1056	Saussure.
	1·1117	Thomson.
	1·1280 at 32° (?)	Davy.
Phosphorus	4·420	Dumas.
,,	4·580	Mitscherlich.
,,	4·4104	T. H. cal.
Phosphuretted hydrogen	1·146	Rose.
,,	1·1214	Dumas.
,,	1·1650	Buff.
,,	1·2059	T. H. cal.
Pyroxylic spirit	1·1200	Dumas.
,,	1·1210	Kane.
,,	1·1026	T. H. cal.
Selenious acid	4·03	Mitscherlich.
,,	3·8591	T. H. cal.
Selenium	16·5390	T. H. cal.
Silicon	1·0261 (?)	Gmelin.
Sulphur	6·5635	Dumas.
,,	6·9000	Mitscherlich.
,,	6·6156	T. H. cal.
,, chloride	3·860	Dumas.
,, ,,	3·5835	T. H. cal.
Sulphuric acid	3·000	Mitscherlich.
,,	2·762	Brande.*
,,	2·7576	T. H. cal.
Sulphurous acid	2·193	H. Davy.
,,	2·2222	Thomson.
,,	2·2277	Buff.
,,	2·2340	Thénard.
,,	2·2470	Berzelius.
,,	2·2630	Kirwan.
Tellurium	26·6227	Gmelin, cal.
,,	26·4624	T. H. cal.

Substances.	Density at 60° F.	Authority.
Gases and vapors.		
Tin	4·0905	Gmelin cal.
,,	3·9960	T. H. cal.
Urethane	3·1400	Dumas.
,,	3·1300	Würtz.
,,	3·0900	T. H. cal.
Urethylane	2·620	Echevarria.
Valeric acid	3·669	Graham.*
Water	0·6240	Brande.*
,,	0·6202	Graham.*
,,	0·6202	T. H. cal.
Gastric juice	1·005	Silliman.
Gaylussite	1·928 to 1·95 (?)	Boussingault.
,,	2·921	Barruel.
Gedrite	3·260	Dufrésnoy.
Gehlanite	2·91 to 3·02	Thomson.
Gelactine	0·969	Thomson.
Geokronite	5·88 to 6·43	Svanberg.
Gibbsite	2·091	Thomson.
,,	2·400	Torrey.
Gieseckite	2·832	Haidinger.
Gilbertite	2·648	Thomson.
Girasol	4·000	Cresy.*
Gismondine	2·180	Kobell.
Gjellebackite	2·205	Thomson.
Glass, crown,	2·520	Moseley.*
,, Bohemian	2·396	Gmelin.
,, bottle	2·732	,,
,, crystal	2·9 to 3·25	,,
,, flint	3·00 to 3·103	Bontemps.
,, for optical purposes	3·3 to 3·6	Guinard.
,, ,,	3·60	Bontemps.
,, ,,	3·77	Fraunhöfer.
,, green	2·642	Moseley.*
,, mirror	2·488 to 2·506	Gmelin.*
,, plate	2·9420	,,

Substances.	Density at 60° F.	Authority.
Glass, plate	2·7600	Tredgold.
„ „	2·249 to 2·408	Mayer and Brazier.
Glauberite	2·730	Brogniart.
„	2·897	Haidinger.
Glaucolite	2·72 to 2·90	Bergmann.
Glauconite	2·598 to 2·632	Thomson.*
Glottalite	2·181	„
Glucina	2·970	Brande.*
„	3.000	Turner.
Glycerine, concen. solu.	1·252	Thomson.*
„ „	1·270	Chevreul.
„ anhydrous	1·280	Pelouze.
Gold-ore, Californian	15·63 to 15·96	T. H. Henry.
„ Australian	14·63	Thomas.
„ melted ditto	18·83	„
Gorlandite	6·410	Gregor.
Granitelle	3·063	Cresy.*
„	2·846	„
Granite	2·622 to 2·956	„
„	2·538 to 2·999	Kirwan.
„	2·5 to 2·6	Portlocke.
Graphite or Plumbago	2·25 to 2·32	Thomson.
„ „	2·140	Fuchs.
„ „	2·273	Regnault.
„ „	2·303	Th. Herapath
Gravel	1·749	Tredgold.
Greenockite	4·80 to 4·908	Breithaupt.
Greenovite	3·440	Dufrénoy.
Greenstone	2·7 to 3·0	Murchison.
Guanite	1·65	Teschemacher.
Guano, African	1·570	Ure.
„ Bolivian	1·600	„
„ „	1·579	Th. Herapath.
„ Chincha I.	1·600	Ure.
„ Peruvian	1·700	„
„ Saldanha B.	1·799	Th. Herapath.
Gum-Arabic	1·3 to 1·5	Brande.*
Gum-Arabic	1·355	Thomson.
„	1·4606 to 1·5256	Herberger.
Gum-Bassora	1·359	Thomson.*
Gum, cherry-tree	1·475 to 1·481	Moseley.*
„ Senegal	1·436	Guerin Barry.
„ „	1·586 to 1·654	Herberger.
„ tragacanth	1·384	Thomson.
Gum-Resins :—		
„ Ammoniac	1·207	Thomson.*
„ Asafœtida	1·327	Brisson.
„ Bdellium	1 371	„
„ Galbanum	1·212	„
„ Gamboge	1·207	Ure.*
„ „	1·221	Brisson.
„ „	1·222	Moseley.
„ Myrrh	1·360	Brisson.
„ Opoponax	1·622	„
„ Scammony	1·235	„
Gunpowder,	1·836	Moseley.*
„ English	1·800	Ure.
„ French	1·793	„
Gunpowder including interstices.		
„ Pigeon and Wilkes'	0·990	Ure.
„ French	1·020	„
„ Battle	1·030	„
„ Curtis and Harvey's	1·050	„
Gymnite	2·2165	Thomson
Gurjan Balsam	0·962	Lüdwig.
Guyaquillite	1·092	Johnston.
Gypsum	2·288	Moseley.*
„	2·310	Haidinger.
„	2·322	Roget and Dumas.
„	2·288 to 2·989	Cresy.*
Hæmatite	3·922	Haidinger.
„ red.	3·605	Yorke.

Substances.	Density at 60° F.	Authority.
Hæmatite, ligniform	3·443	Thomson.
Haidingerite	2·848	Haidinger.
Harmotome	2·40 to 2·448	Thomson.
Harringtonite	2·217	,,
Hartmannite	6·450	H. Rose.
Hausmannite	4·722	Turner.
Hauyne	3·200	Cresy.*
,, soda	2·470	Varrentrap.
Hatchettine	0·9657	T. Herapath.
,,	0·983	Thomson.
Hedenbergite	3·154	H. Rose.
Hedyphan	5·460 to 5·493	Kersten.
Heliotrope	2·700	Moseley.*
,,	2·629 to 2·700	Cresy.*
Heloine	3·166	Gmelin.
Herschellite	2·110	Levy.
Hetopizite	3·523	Dufrésnoy.
Heulandite	2·200	Haidinger.
,,	2·195	Thomson.
Heveëne	0·921	,,
Hone, white razor	2·876 to 2883	Cresy.*
Honey	1·450	Moseley.*
Honey-stone or Mellite	1·666	,,
Hopeite	2·760	Brewster.
Hornblende	3·830	Moseley.*
,, common	3·60 to 3·83	Cresy.*
,, ,,	3·00 to 3·50	L. Gmelin.
Horn-mangan	3·1 to 3·5	Jashe.
,,	3·89	Germac.
Hornstone	2·81	Moseley.*
Humboldtite	2·489	Thomson.
,,	2·89	Klaproth.
,,	2·938	Haidinger.
,,	2·900	Damour.
Huraulite	2·270	Dufrésnoy.
Huronite	2·865	Thomson.
Huttenbergite	7·228	,,

Substances.	Density at 60° F.	Authority.
Hyalite	2·400	Bucholz.
,,	2·110	Cresy.*
Hyalosiderite	2·875	Walchner.
Hydroboracite	1·900	Hess.
Hydroborocalcite	1·800	Ulex.
Hydromagnesite	2·336	Brewster.
Hydrochloric acid	1·270	Graham.*
,, concent. aqueous	1·203	Thomson.
,, solution of	1·210	Brande.*
,, of L. Ph.	1·160	,,
Hydriodic acid, liq.	1·700	,,
Hydrocyanic acid, anhyd.	0·696	,,
Hydrofluoric acid, anhyd.	1·0609 (?)	,,
,, hydrated	1·25	,,
Hydrolite	2·054	Thomson.
Hydrosulphurous acid	1·789	Thénard.
Hyperchloric acid, c. aq. solution	1·704 to 1·811	Nativelli.
,, ,,	1·600	Brande.*
Hypersthene	3·338	Muir.
,,	3·355	Thomson.
Hyposist	1·526	Cresy.*
Hyposulphuric acid, con. solu.	1·347	Brande.*
Ice	0·885	Meinke.
,,	0·888 at 32°	Dulk.
,,	0·905	Kraft.
,,	0·920	Scoresby.
,,	0·927	Osann.
,,	0·9184	Playfair and Joule.
,,	0·937	Irving.
,,	0·947	Williams.
,,	0·950	Roger and Dumas.
Icespar	2·436	Thomson.
Idocrase	3·346	Varrentrapp.
,,	3·349 to 3·399	Thomson.*
,, fused	2·929 to 2·941	Magnus.

Substances.	Density at 60° F.	Authority.
Ilmenic acid	4·1 to 4·2	Hermann.
Ilmenite	4·766 to 4·808	G. Rose.
Ilvaite	3·9799	Stromeyer.
„	3·9940	Haidinger.
„	3·825 to 4·061	Lelievre.
Iodic acid	4·250	Filhol.
Iodine	4·948	Gay-Lussac and Thénard.
Indigo	1·500	Ure.
Indigotine, sublimate	1·350	W. Crum.
Inulin	1·356	Parnell.
Iolite	2·597	Stromeyer.
„	2·651 to 2·664	Thomson.
Iridium, native	19·50	Wollaston.
„ „	19·47 to 21·118	G. Rose.
Iron, apatite	3·97	Fuchs.
„ arsenical	6·13	Gmelin.*
„ hæmatitic	3·555	Johnston.
„ magnetic, ore of	5·092	Thomson.
„ „	4·9 to 5·2	Leonhard.
„ „	5·22 to 5·4	Boullay.
„ „	5·453	Playfair & Joule
„ meteoric	5·75 to 7·30	
„ phosphide	6·70	Hatchett.
„ pyrites	4·83 to 5·03	Thomson.
„ sub-sulphate of, native	2·500	Prideaux.
„ subsulphide	5·800	Playfair & Joule
„ sulphide	5·026 te 5·046	„
„ sesqui-sulphide	4·246	„
„ bisulphide	4·9 to 4·981	„
„ sesqui-arsenide	7·228	Reichenstein.
„ spathic carbonate of	3.737 to 3·829	
„ native	5·95 to 6·72	Thomson.*
„ peroxide	4·60 to 5·135	Playfair & Joule
„ scale-oxide of	5·480	Boullay.
Iron-ore, black-band	3·0553	Colquhoun.
„ brown-clay	4·3700	Yorke.
Iron-ore, clay	3·1175 to 3·3801	Colquhoun.
„ manganesian	5·076	Thomson.
„ needle	4·320	Breithaupt.
„ pitchy	3·459	Vauquelin.
„ „	3·562	Breithaupt.
„ specular	4·491 to 5·059	Kirwan.
„ „	5·251	Haidinger.
„ „	5·2998	T. Herapath.
„ yellow	2·78 to 2·90	Rammelsberg.
Iron, salts of:—acetate	1·368	Gray.*
„ „ chloride, crys.	1·926	Filhol.
„ „ phosphate	2·600	Gray.*
„ „ sulphate, dry	2·640	Brande.*
„ „ „	2·841	Filhol.*
„ „ „ crys.	1·800	Brande.*
„ „ „	1·889 at 39°	Playfair & Joule
„ „ „	1·904	Filhol.
„ „ sat. solu.	1·15 at 52°	Cresy.*
Iserine	4·491	Thomson.
„	4·5 to 4·65	Klaproth.
Isinglass	1·1110	Moseley.*
„ dry	1·2097	T. Herapath.
Ittnerite	2·3854	Thomson.*
Ivory	1·825 to 1·899	Moseley.*
„ vegetable	1·376	A. Connel.
JADE	2·801 to 2·9829	
„ oriental	2·970	Damour.
Jamesonite	5·564	Thomson.*
„	5·616	Schaffgotsche.
Jasper	2·62 to 2·816	Cresy.*
Jenite	3·8 to 4·0	„
Jeffersonite	3·510	Keating.
Jet	1·259	Cresy.*
„	1·300	Moseley.*
Johannite	3·190	Phillips.
Junkerite	3·815	Dufrénoy.

Substances.	Density at 60° F.	Authority.
Juice of apples	1·060 to 1·084	Couverchel.
Juice, cane	1·046 to 1·110	Evans.
„	1·033 to 1·106	Porter.
„ of average quality	1·07 to 1·09	
KAMMERERITE	2·640	Nordenskiold.
Kaolin, or porcelain clay	2·216	Karsten.
„ „	2·484	Thomson.
„ „	2·47 to 2·64	Laurent.
Kapnite	4·00 to 4·15	Manheim.
Karpholite	3·923 to 3·936	
Karphosiderite	2·500	Breithaupt.
Karsten	2·652 to 2·668	Köhler.
Kilbrickenite	6·407	Apjohn.
Killinite	2·711	Taylor.
Kirwanite	2·941	Thomson.
Knebelite	3·714	Döbereiner.
Kobellite	6·29 to 6·32	Setterberg.
Konichalcite	4·123	Breithaupt.
Krokydolite	3·200	Stromeyer.
Kupaphrite	3·098	Thomson.
LABRADORITE	2·690	Klaproth.
„	2·695 to 2·7025	G. Rose.
„	2·699	Thomson.
Lactic acid	1·215	Thomson.
Lamarkite	6·8 to 7·0	Brooke.
Lard	0·947	Moseley.*
Latrobite	2·800	Brooke.
Lazulite	3·057	Fuchs.
„	3·106 to 3·123	Rammelsberg.
Laumonite	2·200	Cresy.*
Laurel-turpentine	0·8645	Stenhouse.
Lead, acetate, anhydrous	2·570	Gray.*
„ „ crystallised	2·345	„
„ arseniate, native	6·410	Gregor.
„ bitelluride	7·085	Thomson.*
Lead, carbonate of, native	6·465 to 6·480	Thomson.*
„ carbonate of, artifi.	6·40 to 6·75	Brande.*
„ cupreo-sulphate of	5·30	Brooke.
„ chloride	5·823	Karsten.
„ „ fused	5·680	„
„ chromate	5·653	Playfair & Joule.
„ dichromate	6·266	„
„ sesqui-chromate	5·750	„
„ iodide of	6·028 to 6·111	Boullay.
„ „	6·384	Filhol.
„ nitrate of	4·316 to 4·472	Playfair & Joule.
„ „	4·581	Filhol.
„ sub-nitrate of	5·645	Playfair & Joule.
„ protoxide	9·250	Playfair & Joule.
„ „	9·277	W. Herapath.
„ „	9·209	Karsten.
„ „	9·361	Filhol.
„ „	9·4214	Graham.*
„ peroxide of	8·756 to 8·897	Playfair & Joule.
„ „	8·900	W. Herapath.
„ „	9·190	Boullay.
„ suboxide of	9·772	Playfair & Joule.
„ selenide of	7·187	H. Rose.
„ sulphate of	6·300	Filhol.
„ sulphato-carbon. of	6·8 to 7·0	Brooke.
„ sulphide of	6·9328	Playfair & Joule.
„ „ nat.	7·500	Karsten.
„ „	7·568	Mohs.
„ „	7·587	Brisson.
„ „	7·2200	Muschenbroack.
„ „	7·4 to 7·6	Leonhard.
„ tungstate	8·000	Breithaupt.
„ vanadiate	6·663	Thomson.
Leadhillite	6·3 to 6·5	Brooke.
Lead-ore, corneous	6·056	Chenevix.
„ heavy	9·392 to 9·448	Breithaupt.
„ white	6·465 to 6·480	Thomson.

On Iodides *of* Quinine.* By Mr. John A. Spencer.

Having for some years past prepared a true hydriodate of quinine for medicinal use, I have been led at intervals to examine some of the substances known in commerce as iodide of quinine, and also the bodies formed by the action of iodine in different forms on quinine and its salts. The subject is one which is well worthy of the attention of those who have more time and opportunity than I have to make a more extended examination of the results.

It is not my intention in the present paper to make any extensive remarks on these preparations, but rather to give a few of the notes I have made from time to time while preparing them for sale.

The true hydriodate of quinine is obtained by dissolving pure quinine in a slight excess of pure hydriodic acid, made by passing a stream of hydrosulphuric acid through water, containing iodine in suspension. On cooling, it separates in the form of brilliant yellow needles, of considerable size and great beauty. They are freely soluble in hot water, from which they separate on cooling in much smaller crystals than before, require about 18 or 20 parts of cold water for solution, and effloresce rapidly by exposure to a dry atmosphere. The watery solution, especially when acidulated with hydriodic acid and exposed to the air, becomes ropy, and sometimes deposits dark brown crystals, which I shall have occasion to notice further on.

The salt may be procured by other means: thus, by decomposing solution of sulphate of quinine in dilute acid with iodide of potassium, the yellow crystals appear after a few seconds; but as there is considerable loss in washing them from sulphate of potassa, it is more economical to prepare it in the manner I first mentioned. On boiling neutral sulphate of quinine with an excess of iodide of lead, it separates, on cooling, in the form of a pasty gelatinous looking mass, without much tendency to crystallise; but when it is redissolved, with the addition of a little free hydriodic acid, it separates again on cooling in similar crystals to those obtained by other methods. I would mention, *en passant*, that it is a salt which appears to deserve a more extensive application as a therapeutic agent, possessing as it does all the active properties of iodine, without the depressing effect so often caused by other preparations of that body.

There is a substance known in commerce as iodide of quinine, which appears as a white crystalline powder, not very soluble in water; it is prepared by decomposing the disulphate with an alkaline iodide, and washing. From what I have noticed in it, I am convinced that iodide of quinine is not a proper name for it, but that it would more properly be called dihydriodate of quinine, as it corresponds to the disulphate in the same way as the salt first described does to the neutral or soluble sulphate. It is soluble in a very large proportion of water, and on cooling separates in the form of small acicular crystals; on the addition of free hydriodic acid it becomes immediately

* Read before the Chemical Discussion Society, January 14th, 1854.

yellow and soluble, and on the cooling of a warm solution deposits crystals identical with, and in every way similar to, the true neutral hydriodate. I should mention, that I have not been successful in preparing this substance by methods analogous to those by which I obtained the former salt.

The term "iodide of quinine" was, I think, first applied by the late Dr. A. Todd Thomson, to a combination which he effected by triturating equivalents of iodine and quinine with water in a mortar, a yellowish solution is formed, and a small quantity of brown resinous looking matter remains. This solution on evaporation does not readily crystallise, in which it agrees with the hydriodate when quite free from excess of acid, but on the addition of free hydriodic acid I have little doubt that it would crystallise with no more difficulty than from the other solutions; but this I have not yet had time to try.

The brownish resinous looking substance just mentioned, seems to be identical with the ropy substance obtained by exposing an acid solution of the hydriodate to the air; at least, as far as their physical appearance and behaviour allow us to judge; they are both soluble in spirit, and deposit sometimes a few crystals, but more frequently form, on the evaporation of a spirituous solution, a resinous extract of a beautiful red brown color, which becomes after a time quite brittle; on passing hydrosulphuric acid through a solution of it, it becomes converted into the neutral hydriodate. Thus, it would appear to be a product of the oxidation of quinine, or possibly it may be quinine in which a portion of the hydrogen is replaced by iodine.

There is yet another substance having considerable interest from the beauty of its appearance, of which I should make mention. On pouring an alcoholic solution of iodine into a hot alcoholic solution of neutral sulphate of quinine a body separates, on cooling, in the form of small but beautilful crystals, having the greenish color and metallic appearance of murexide. It was examined some years ago by my friend, Mr. Nicholson, but I do not know if he ever completed his analyses and experiments upon it. I believe it to be a similar body, if not identical, with that which was stated by Mr. Herapath to be possessed of such remarkable powers of polarising light.

On the Composition *of the so-called* Simple Substances.

By W. M. Meacock.

That many of the so-called "simple" substances are in reality *compound*, has been the opinion of more than one chemist. Of course we cannot speak with certainty on this point, till the substances in question are actually decomposed into their component parts. Actual analysis is requisite before we can remove any body from the list of "simple substances" and place it in that of compound; yet may we assign to many bodies formulæ—hypothetical, indeed, but still exceedingly probable. Take, for instance, chlorine, bromine and iodine, who can doubt that they are *compounds*, alike in their composition as

they are in their properties? That composition appears to me to be this :—

Chlorine = N + C + O^2 = 36
Bromine = Cl^2 + C = 78
Iodine = Br + Cl + C^2 = 126.

It is well known that the cyanides resemble the *chlorides;* why so, we shall understand if we compare the formula of cyanogen with the hypothetical one of chlorine :—

Cy. N + C^2 = 26
Cl. N + C + O^2 = 36.

We may conceive that cyanogen is formed from chlorine by the substitution of one atom of carbon for two of oxygen. On the other hand, chlorine may be formed from cyanogen by a transformation the converse of the above.

Liverpool, Jan. 17th, 1854.

TRANSLATIONS AND ABSTRACTS.

ANALYTICAL CHEMISTRY.

On a Method of VOLUMETRICAL ANALYSIS *of very general applicability.* By R. BUNSEN.

(*Continued from page* 226.)

Bohemian pyrolusite which gave by weight determination 0·32 per cent. silica, 0·08 per cent. peroxide of iron, and 0·5 per cent. water, gave :

A = 0·3128 ; n = 5 ; t = 79·3 ; t = 44·0 ; a = 0·0025387.

Superoxide of manganese . .	98·25
Peroxide of iron . . .	0·08
Silica	0·32
Water	0·50
	99·15

XI. *Determination of Iodic, Vanadic, Selenic, Manganic and Ferric Acids and Ozone.*

As all these volumetrical determinations, as well as many others, are conducted in a manner which is essentially the same for all, and as the deduction of the equations relating to them will be sufficiently intelligible from what has preceded, it will be enough here to bring forward one example only. I select for this purpose the determination of iodic acid.

When the free or combined acid is distilled with an excess of fuming hydrochloric acid, there are disengaged for every atom of iodic acid four atoms of chlorine, while one atom of chloride of iodine remains in the liquid. The percentage of iodic acid in a saline mixture whose

weight = A, may therefore be ascertained by the method described for the determination of chromium with the aid of the formula,

$$x = \frac{100\ (RO + IO^5}{A\quad 4\quad I.} a\ (nt—t')$$

For the purpose of testing the method a mixture of 0·5321 chloride of calcium with 0·2755 iodate of baryta was taken. The volumetrical analysis of it gave,

$$n = 3\ ;\ t = 85·5\ ;\ t' = 31·4\ ;\ a = 0·002578.$$

	Taken.	Volumetrical analysis.
Chloride of calcium .	65·53	65·89
Iodate of baryta . .	34·47	34·11
	100·00	100·00

XII. *Volumetrical separation of the Oxides of Cerium and Lanthanum.*

These oxides are usually separated together as oxalates, which are dissolved in concentrated sulphuric acid and precipitated by caustic potassa. The hydrated protoxides obtained are then suspended in a concentrated solution of caustic potassa, treated with a stream of chlorine, and then carefully washed. The precipitate which contains the cerium in the state of protoxide is introduced moist into the flask above described, and covered with fuming hydrochloric acid, in which it dissolves even at the ordinary temperature with a brown color. When the temperature is raised there is disengaged for every atom of protoperoxide of cerium one atom of chlorine, which when passed into the solution of iodide of potassium liberates an equivalent quantity of iodine. If this iodine volumetrically determined is represented by a (nt—t') the quantity of protooxide of cerium contained in the precipitate may be found by the equation :

$$x = \frac{(3\ Ce + 4O)}{I} a\ (nt—t),$$

or in the state of protoxide by

$$x = \frac{3\ CeO}{I}\ a\ (nt—t').$$

In order to test this method, Kjerulf made the following experiment : a mixture of hydrated oxides of cerium and lanthanum obtained from cerite, and separated in the most careful manner from alumina, silica, lime, peroxide of iron, and the metals precipitable by sulphuretted hydrogen, that are usually present in cerite, was treated with chlorine in contact with caustic potassa, the solution containing the lanthanum separated by filtration, and the precipitate, after being seven times dissolved in hydrochloric acid precipitated by potasa and treated with chlorine, used for the experiment. An undetermined quantity of the moist yellow gelatinous precipitate gave,

$$n = 2\ ;\ t = 20·1\ ;\ t' = 106·9\ ;\ a = 0·0025387.$$

The solution of oxide of cerium remaining in the flask was neutralised, and yielded by precipitation with oxalate of ammonia 1·2127 grms. of oxalate of cerium. This salt again gave by combustion with

oxide of copper for 0·5275 grms. 0·2073 grms. of carbonic acid, and 0·0460 grms. of water. Upon the assumption that the salt is neutral, the oxygen of the oxide of cerium should amount to one-third that of the oxalic acid, and consequently the composition of the oxide of cerium would be,—

Cerium	87·918
Oxygen	12·082
	100·000

Hence it follows that the atomic weight of cerium is 727·7. Adopting this number we obtain,—

	Calculated.	
1 atom oxide of cerium .	59·45	59·13
1 „ oxalic acid . .	32·47	32·15
1 „ water . . .	8·08	8·72
	100·00	100·00

The oxide of cerium present in the total weight of oxalate of cerium 12137 contains, according to these experiments, 0·0866 oxygen. Before its reduction by hydrochloric acid it was combined with a quantity of oxygen, which, calculated from the above volumetrical data by means of the equation given, amounted to 0·0296.

Since 0·0866 : 0·0296 as 3 : 1 thus:

	Found.	Calculated.
Oxygen in the lowest oxide of cerium . .	0·0866	0·0872
Surplus oxygen in the higher oxide examined .	0·0296	0·0291.

It may be inferred that the hydrated oxide of cerium, when treated with chlorine in contact with a solution of caustic potassa, passes into oxide, having the formula $CeO + Ce^2O^3$, and that the behavior of this oxide, when heated with hydrochloric acid, may be taken advantage of for the determination of cerium, even in the presence of lanthanum.

Besides the substances above considered, and a great number of others which effect a liberation of chlorine, those substances also which are readily and completely oxidised to a higher stage by chlorine, may be determined by this method. They are heated with fuming hydrochloric acid and a weighed quantity of pure bichromate of potassa p, the disengaged chlorine passed into solution of iodide of potassium, the iodine thus liberated determined in the ordinary manner, and its quantity $a(nt—t')$ is equal to the quantity of iodine $\frac{p\,3I}{KOCrO^3}$ equivalent to the bichromate of potassa used, minus the quantity of iodine j equivalent to the oxide employed. This j is therefore :

$$j = \frac{p\,3I}{KO + 2CrO^3} — a(nt—t')$$

The weight of the substance sought may then be ascertained by

simple stochiometric calculation from the iodine equivalent as in the following instance.

XIII. *Determination of Protoxide of Iron alone or when mixed with peroxide.*

The quantity of protoxide of iron = e in a substance may be ascertained thus: let j represent the quantity of iodine required to convert the protoxide of iron to be determined into peroxide. This quantity of iodine is to the protoxide of iron as I : 2FeO. Consequently, when the above found value is substituted for j, the protoxide of iron contained in the substance examined is,—

$$1. \qquad e = \frac{6FeO}{(KO + 2CrO^3)}\,p - \frac{2FeO}{j}\,a(nt - t'),$$

and the iron e′ contained in it is,—

$$2. \qquad e' = \left(\frac{6Fe}{KO + 2CrO^3}\right)p - \frac{2\,Fe}{j}\,a(nt - t'),$$

or lastly, the peroxide of iron,—

$$3. \qquad e'' = \frac{3Fe^2O^3}{(KO + 2CrO^3)}\,p - \frac{F^2O^3}{j}\,a(nt - t').$$

The formula 1. is correct only so long as the equation

$$\frac{j}{2FeO}\,e < \frac{3\,j}{(KO\,2CrO^3)} \text{ is satisfied.}$$

This is the case when the quantity of bichromate of potassa used equals or exceeds that of the ferruginous substance examined; of course a similar limitation obtains with regard to the equations 2 and 3.

When it is required to determine protoxide of iron either alone or mixed with peroxide, the distillation flask is two-thirds filled with fuming hydrochloric acid, the atmospheric air displaced by carbonic acid by dropping a fragment of carbonate of soda into the acid; and the weighed quantity of bichromate p, together with the substance A to be examined, contained in a short wide glass tube, introduced into the acid. The delivery tube is then connected, and the operation conducted as in the valuation of chromate of potassa.

Well dried magnetic iron ore, in fine octohedrons, from the Tyrol, gave, when analysed in this manner,—

A = 0·2869 ; p = 0·4206 ; n = 6 ; t = 71·3 ; t′ = 65·6 ; a = 0·0025387,

	Atomic calculation.	Volumetrical analysis.
1 atom peroxide of iron . .	68·97	68·96
1 atom protoxide of iron . .	31·03	31·04
	100·00	100·00

When, on the contrary, it is required to determine metallic iron or peroxide, these substances are dissolved in the flask by means of hydrochloric acid, and perfectly reduced to the state of protoxide by boiling with sulphurous acid, or still better, a ball of chemically pure zinc, cast upon a thin platinum wire.

In order to prevent any access of air during this operation, the flask is fitted with a cover. When the color of the liquid indicates that the reduction is complete, the flask is cooled in water, the upper stopper raised, a few fragments of carbonate of soda dropped into the acid, the zinc ball drawn up into the tube, freed from adhering liquid by washing with a dropping-bottle, and the cover removed. The weighed quantity of bichromate is then rapidly introduced, and the operation continued as above.

For the purpose of testing the method, 0·5603 grms. of clean, thin harp string wire was dissolved in nitro-hydrochloric acid, the silica separated by evaporation and re-solution in acid, and the peroxide of iron precipitated by ammonia. After ignition it weighed 0·7977 grms.

The volumetrical determination of 0·2087 grms. of the same wire gave,—

$A = 0{\cdot}2087$; $n = 1$; $t = 68{\cdot}4$; $t' = 11{\cdot}0$; $a = 0{\cdot}0025387$.

	Weight determination.	Volumetrical analysis.
Iron	99·66	99·62
Carbon and silicium . . .	0·34	0·38
	100·00	100·00

XIV. *Determination of Arsenious Acid and its Salts.*

If the weight of the substance containing arsenious acid is represented by A, the weight of the chromate of potassa employed by p, the percentage of arsenious acid in the substance examined may be ascertained from the formula,—

$$x = \frac{100}{A}\left\{\frac{3AsO^3}{2(KO \times 2CrO^3)}p - \frac{AsO^3}{2I}a\,(nt - t')\right\}.$$

The quantity of iodine which is consumed by the arsenious acid x contained in A is $\frac{2I}{AsO^3}x$; so likewise the iodine liberated by p is $\frac{3I}{(KO + 2CrO^3)}p$. Consequently the proportion by weight of the substance containing arsenious acid to the chromate, must be such that the equation $\frac{2I}{AsO^3}x < \frac{3I}{(KO + 2CrO^3)}p$ is always satisfied under all conditions, that is for every weight unit of the substance more than 0·998 chromate must be taken. For the purpose of testing the equation, 0·2615 grms. of pure arsenious acid mixed with 0·5274 grms. of sulphate of lime and 0·4334 grms. of chromate was tested volumetrically. Experiment gave $A = 0{\cdot}7889$; $p = 0{\cdot}4334$; $n = 3$; $t = 79{\cdot}8$; $t' = 66{\cdot}4$; $a = 0{\cdot}0025387$.

	Taken.	Found.
Arsenious acid	33·15	33·14
Sulphate of lime	66·85	66·86
	100·00	100·00

As the other methods of determination of this nature may very easily be deduced from what have been above described, it would be superfluous to add any further remarks.

Heidelberg, March 29th, 1853.

On the EMPLOYMENT *of* CHLORINE *in* ANALYSES. By MM. RIVOT, BEUDANT, *and* DAGUIN.

CHLORINE has long been employed in the analysis of mineral substances. Sometimes these substances have been subjected to the action of a current of dry gaseous chlorine, sometimes the operation is conducted in the presence of water.

The first method is applicable to bodies whose chlorides are volatile, such as sulphur, arsenic, tin, and antimony, to separate them from fixed chlorides, such as those of iron, copper, nickel, cobalt, &c.

Nevertheless, as these last chlorides are not absolutely fixed, the separation thus obtained by the dry way is never perfect, and only gives an exact result when the operator is very skilful and accustomed to this species of experiment.

In the second mode of analysis, the chlorine may become an oxidising agent; it is thus that it peroxidises iron and cobalt, and changes arsenious acid into arsenic acid when dissolved in water acidulated with hydrochloric acid. It will be remembered that on this property is based the chlorometric method generally employed in manufactories, and which was invented by Gay-Lussac.

Chlorine has been long employed in a certain number of analyses both by the dry and the humid ways; but the works of which we have to give an account have the certain result of extending the use of it, and of causing it to enter into an important part of the domain of assaying.

The experiments of the authors all have relation to the employment of chlorine by the humid way in solutions either alkaline or acid.

In the first of these conditions, that is in the presence of a free alkali, the oxidising action is extremely energetic, and generally causes the bodies to pass to their highest point of oxidation; it will be at once perceived that it would be very useful to bring to the state of soluble salts the metals or oxides which give rise to acids when combining with oxygen. These salts are thus separated from the acid insoluble in alkalis.

In the second case, that is to say, in an acid medium, the oxidising action of chlorine is much less energetically manifested; it is only exerted on a very small number of metals, such as lead and manganese which pass to the state of binoxides insoluble in the acid.

To render these considerations easier of comprehension, we will proceed to give the principal results attained by M. Rivot, and his two fellow-laborers, MM. Beudant and Daguin.

First Case.

Action of Chlorine in alkaline solutions.—Sulphuret of lead is transformed into an entirely insoluble binoxide and an alkaline sulphate. Thus is resolved the difficult question of the complete separation of lead and sulphur, and at the same time the exact and rapid analysis of galena.

Oxide of lead dissolved in potassa is transformed by chlorine into binoxide, which remains in solution, forming with the alkali a soluble combination, in the presence of an excess of potassa. Thus, in two different circumstances, which may be reproduced at will, the binoxide of lead may be separated in a state of purity and become free, or may remain in solution, combined with the alkali.

Hydrated oxide of iron, natural and artificial, is rapidly transformed into ferric acid, which gives, on combining with the alkali, a solution of a very deep red color. This fact had already been mentioned by M. Fremy, in alluding to the discovery of ferric acid, which is due to that skilful chemist. Ferric acid is not produced when the alkaline liquor is below 32° F.; neither is it formed if the temperature reaches 104° or 122° F., in the presence of quartz and of some other substances, whose singular power of modifying the ordinary reactions of chlorine, we shall mention further on.

When chlorine is made to act on sulphuret of iron in an alkaline solution, the sulphur dissolves in the state of sulphuric acid, whereas the iron remains insoluble, in the state of sesquioxide; and it is only after the complete acidification of the sulphur that the oxide of iron may be dissolved in its turn, passing to the state of ferric acid.

When treating by this process well porphyrised arsenical pyrites, we may easily obtain the sulphur and arsenic dissolved in the state of alkaline sulphate and arseniate, and the iron insoluble as peroxide; this is very easily done by interrupting the action of the chlorine when the liquor begins to color, and heating it for a few moments with a little pulverised quartz.

The oxides of manganese are very rapidly transformed into alkaline permanganate, when operating in a moderately concentrated alkaline liquor, and at a temperature of from 104° to 122° F. When operating, on the contrary, below 32° the action of chlorine does not produce manganic acid. When the solution, in contact with the acid, returns to the ordinary temperature, green manganate of potassa is observed to form. Thus the action of chlorine will produce at will binoxide of manganese, or alkaline manganate or permanganate.

Oxide of copper has a great tendency to combine with the fixed alkalis: the combination is soluble and even stable at the boiling point in presence of a great excess of alkali. In the estimation of copper, precipitated in the state of oxide by potassa, a small quantity of this compound is always formed. Its presence may be, at least in great part, prevented by making the precipitation in an ammoniacal liquor. It is still better avoided by causing chlorine to pass into the alkaline liquor, not containing ammonia, until the alkali is nearly saturated.

The sulphurous copper ores, well porphyrised, may be easily analysed by the action of chlorine in the presence of an alkaline liquor. The copper and other metals remain insoluble in the state of oxide, whilst the sulphur, arsenic and antimony dissolve completely in the state of alkaline salts. The presence of blende in these ores is in general injurious to the clearness of the reactions.

The oxides of cobalt and nickel, recently precipitated by potassa and put in suspension in an alkaline liquor, pass rapidly to the state of sesquioxide under the influence of a current of chlorine. In the analysis of the ores of nickel and cobalt, great difficulty is usually found in separating these metals distinctly from arsenic and antimony; this difficulty is very easily removed by operating in the following manner:

The ore is to be attacked by nitric acid; to the liquor diluted with water is added a great excess of potassa; it is gently warmed, and a current of chlorine is passed into it until the precipitate is black. The liquor then contains the arsenic and antimony in the state of alkaline salts; the insoluble portion contains the metals in the state of sesquioxide, no longer retaining even a trace of arsenic or antimony.

Phosphoric acid may be separated, like arsenic and antimony, from those of the other metals which form sesquioxides or binoxides; for example, from iron, nickel, cobalt and lead.

The estimation of free sulphur containing organic matters, such as are used in such considerable quantities in the manufacture of sulphuric acid, presents serious difficulties when its peroxidation is attempted with aqua regia or nitre in fusion. It always succeeds very well by employing chlorine, as an oxidising agent, in a warm and concentrated alkaline solution. If it is required to estimate the sulphur contained in a sample of commercial sulphur, or in a precipitate of sulphurets of arsenic and antimony produced in an analysis, or again in the natural sulphurets, the sulphuret is to be warmed for some hours in a solution of potassa, which dissolves it completely, as well as the sulphurets of arsenic and antimony. Chlorine is then passed into the liquor; in a very short time the three bodies pass to the state of sulphuric, arsenic and antimonic acids, which remain dissolved in the state of alkaline salts. All that remains is to render the liquor acid with hydrochloric acid, and to precipitate the sulphuric acid with chloride of barium after having driven off the excess of chlorine by means of heat.

The sulphate of baryta is to be well washed, calcined and digested in hydrochloric acid and a little chloride of barium. It is only after a second washing and a second calcination, that the weight of the sulphate of barium should be taken for the estimation of the sulphur.

The necessity of these operations results from the property which sulphate of baryta possesses of removing a certain quantity of salts contained in solution, and not giving them up to washing until after calcination.

The action of chlorine in a solution of potassa aids in the estimation of the sulphur in all metallic sulphurets. For this estimation it is necessary to porphyrise the sulphuret perfectly, to cause it to digest for several hours in a solution of potassa heated to from 122° to 140°

F. and to pass chlorine into it. The sulphur oxidises rapidly, and dissolves in the state of sulphate of potassa, whilst the metals, transformed into oxides, remain insoluble. The sulphuric acid is precipitated by chloride of barium from the filtered alkaline liquor, acidified with hydrochloric acid.

This is a very advantageous method, especially for sulphurets which contain lead, because this metal remains quite insoluble in the state of binoxide. We no longer have to fear the numerous inconveniences ordinarily caused by the slight solubility of sulphate of lead.

A great many organic matters are directly soluble in potassa, or become so by the oxidising action of chlorine. In a solution thus obtained, the reactions of mineral analytical chemistry may, in general, take place in the same manner as in the absence of organic matters. The action of chlorine and the alkalis is above all useful to destroy the filtering-paper which has been used to collect the sulphurets of zinc, lead and some other more or less volatile metals. The elimination of the paper by the action of chlorine in an alkaline solution advantageously replaces the burning of the filters, which may frequently cause perceptible loss.

This method may likewise be of service in the examination of the mineral matters contained in organic substances. Burning is likewise prevented, which almost always occasions loss.

One of the matters which presents the most difficulties is vulcanised caoutchouc into which ceruse and oxide of zinc are often introduced. This matter is first subjected to the prolonged action of boiling nitric acid, which attacks and reduces it to a great state of division, water and an excess of potassa are added to the turbid liquor, then a current of chlorine is passed into the mixture; the two oxides of zinc and lead, and a white resinous matter are deposited; the sulphur remains dissolved in the state of alkaline sulphate. It is estimated as sulphate of baryta. The insoluble portion is treated with acetic acid, which dissolves the oxides of zinc and lead, which are afterwards precipitated in the state of sulphuret with sulphuretted hydrogen.

By this means the exact estimation of the mineral matters contained in caoutchouc may be made in a very short time, and this is certainly one of the most difficult analyses to make on account of the presence of an organic matter.

Second Case.

Action of Chlorine in a Liquor containing free Acetic Acid, and a certain proportion of Alkaline Acetates.—In these conditions the oxidising action of chlorine is much less energetic than in the presence of the caustic alkalis. Thus, free sulphur, the sulphur of metallic sulphurets, acidifies very slowly; the metals which do not form very stable peroxides remain dissolved or dissolve in the free acetic acid in the state of protoxide. On the contrary, those metals which form stable peroxides undecomposable by acetic acid give rise to these peroxides, and may thus be clearly separated from the other metals.

This action is most easily applicable to manganese and lead. They may thus be clearly separated from the alkalis, from magnesia, &c., and from several metals, especially zinc, copper, nickel, &c. The operation should be conducted in the following manner :—

An acetic solution is produced containing oxide of lead or of manganese, and the other oxides; to this is added a certain quantity of acetate of soda, and it is heated to 122° or 140° F. A current of chlorine is then passed in, which is stopped the moment that the binoxide is deposited, which takes place very rapidly. It is washed by decantation and filtered. The insoluble portion contains all the lead or manganese, the liquor retaining the other oxides.

This process cannot be employed for the separation of lead and manganese from iron and cobalt. In the presence of these metals the precipitation of the binoxides is difficult of completion, and the precipitate always retains a certain proportion of the oxides of iron and cobalt.

It only remains for us, in concluding this report, to draw the attention of all chemists to one point mentioned in the work of which we have presented an analysis; namely, the singular property possessed by many bodies and certain powders otherwise inert, of causing a rapid disengagement of oxygen in alkaline solutions into which chlorine is passed. This phenomenon resembles the remarkable decompositions of oxygenous water, and those of chlorate of potassa in the presence of the oxides of copper and manganese. This will be easily and certainly proved by directing a current of chlorine into a solution of potassa previously carried to from 194° to 212° F., and holding in suspension quartz in fine sand, iron pyrites, arsenical copper, copper, the oxides of copper, nickel, cobalt, &c.

When this decomposition commences it almost entirely opposes the oxidising action of the chlorine, but this may be prevented in analytical operations, by reducing into very fine powder the mineral substances whose composition it is desired to ascertain.

The disengagement of chlorine, of which we have just spoken, is sometimes remarked during the manufacture on the large scale of chlorate of potassa, either when the chlorine reaches the milk of lime at a temperature above 140° F., or when the chlorate of lime is heated with chloride of potassium.

The presence of silica in the lime is sufficient to explain this disengagement of oxygen, and the diminution of weight of the chlorate of potassa which is the consequence of it.

To sum up, the work of MM. Rivot, Beudant, and Daguin, contains many new observations, and the description of several important analytical methods.

Comptes Rendus, December 5th, 1853.

PUBLIC HEALTH.

COFFEE *considered as a* DRINK *in a* CHEMICAL *and* PHYSIOLOGICAL *Point of View.* By M. J. LEHMANN.

SINCE coffee has acquired, as a drink, so much importance in social life, it has attracted much attention from many scientific men. Some have been satisfied with forming hypotheses as to its action on the animal economy; others have endeavored, relying on facts drawn from chemical analysis, to arrive at more certain conclusions in this respect. Some years since M. Payen, founding his arguments on the richness in nitrogen of coffee, attributed a true nutritive value to the infusion of this product. He appears to have admitted the principle, that the nutritive value of a substance is proportional to the quantity of nitrogen which it contains. But since this opinion was promulgated, physiologists have modified it so far as to admit, that the proportion of nitrogen can be taken as the basis of the alimentary value of a substance only when contained in it under the form of an albuminoïd matter. Now, the legumine contained in coffee-berries only passes into the infusion in very small proportion, and it is certain that the greater proportion of the nitrogen proceeds from the caffeïne. This circumstance obliges us to modify the opinion hitherto held as to the nutritive properties of coffee.

To establish the influence which coffee exercises on the metamorphosis of the tissues, to decide whether it takes a more or less direct part in it, nothing is better than to study the modifications that the use of coffee introduces into the composition of the urine. M. Boecker has investigated this subject, and has arrived at the conclusion that the proportion of urea is diminished after the ingestion of coffee.

M. Lehmann has arrived at a contrary result; after the administration of caffeïne he has perceived that the proportion of urea in the urine has considerably increased.

In consequence of these contradictory results, the author, M. J. Lehmann, has thought fit to undertake some new experiments on this subject.

Many considerations suggested to him, *à priori*, the idea that coffee, without being itself exactly an aliment, might yet exert a considerable influence on nutrition, by, in some degree, moderating the activity of the proximate transformations, and, consequently, by permitting the organism to be satisfied with a reparation which, in ordinary circumstances would be insufficient.

It may easily be understood that thus placed, the question becomes accessible to experiment; on the other hand, the author does not wish to conceal the difficulties of such an investigation. In fact, it was necessary not only to find persons who would submit for entire weeks to a strictly uniform regimen, but it was even requisite to guard against any pathological influences which might render the results uncertain; besides which, the numerous and delicate analyses required by this species of investigation take a considerable time and require the greatest care. Fortunately the new processes introduced by M. Liebig have

materially simplified the author's task. He operated in the following manner:

He began with two men by determining the quantities of urine, urea, chloride of sodium and phosphoric acid secreted in twenty-four hours under the influence of a fixed regimen from which coffee was excluded. This regimen, described hereafter, was continued long enough for the three principles to become quite stationary.

G. M., thirty-two years of age, of a robust constitution, weighing 102 pounds, was subjected during the whole time of the experiments to the following regimen, which appeared to agree with him perfectly. In the morning he had 250 grms. of white bread and butter; at noon, 375 grms. of meat (beef steak), with 250 grms. of rice boiled in water, and the same quantity of bread, and in the evening 750 grms. of bread and butter. To drink, he had five or six glasses of water in the course of the day, and in the evening two small glasses of common beer.

This man was thus dieted for many weeks; it agreed with him perfectly, and he said that he could continue it without disgust for the entire year.

The following are the results of the experiments made on his urine:

First series of experiments on G. M. Regimen without Coffee.

Date.	Cubic cent. of urine.	Reaction.	Peculiar properties.	Phosphoric acid.	Chloride of sodium.	Urea.
May				gr.	gr.	gr.
19	1030	acid	clear; yellow	3·166	8·389	22·906
20	1130	alkaline	turbid; sediment	3·112	9·591	23·131
21	2020	alkaline	turbid; considerable sedmt.	3·732	11·448	23·664
22	1808	alkaline	*id* *id*	3·644	13·328	25·229
23	2010	alkaline	*id* *id*	4·578	9·158	26·300
24	1695	alkaline	clear; yellow	3·898	8·051	24·860
25	1284	acid	*id*	3·842	9·714	24·598
26	1376	acid	*id*	4·423	9·420	25·282
27	1856	acid	*id*	5·796	12·341	25·096
28	1458	acid	*id*	4·238	9·429	27·360
29	1532	acid	*id*	4·156	8·892	26·250
30	1453	acid	*id*	3·998	9·245	26·975
31	1312	acid	*id*	4·098	10·001	27·991
June 1	1465	acid	*id*	4·210	9·251	27·581
	7720			20·700	46·818	136·160

The second subject, H. S., subjected to an analogous experimentation, was of a strong constitution, and weighed 141 pounds. He was accustomed to a very substantial regimen, and, like the first, to the use of coffee.

The diet to which he was restricted during the whole experiments was exactly similar to the preceding, except that at noon he received 450 grms. of meat.

First series of experiments on H. S. Regimen without Coffee.

Dates.	Cubic cents. of urine.	Reaction.	Peculiar properties.	Phosphoric acid.	Chloride of sodium.	Urea.
May				gr.	gr.	gr.
19	1632	acid	clear ; yellow	3·210	7·246	25·016
20	1921	acid	*id*	3·405	8·358	24·251
21	1750	alkaline	turbid	3·756	9·462	26·343
22	2187	alkaline	*id*	4·202	8·444	26·584
23	1841	acid	clear ; yellow	3·991	8·346	28·414
24	1430	acid	*id*	4·223	10·541	26·992
25	1579	acid	*id*	4·159	11·201	29·101
26	1612	acid	*id*	4·371	9·414	29·425
27	1699	acid	*id*	4·500	10·001	31·587
28	1599	acid	*id*	3·899	9·870	31·110
29	1674	acid	*id*	4·643	9·573	30·984
30	1656	acid	*id*	4·574	10·052	31·558
31	1549	acid	*id*	4·489	9·831	31·251
	8177			22·105	49·327	156·490

The perturbatory influences which the complete change of diet, and especially the privation of the coffee, exercised on the system, were manifested by a feeling of weakness and unsatisfied hunger, as well as by the temporary alkalinity of the urine.

The mean of the products secreted by each subject were calculated from the results finally obtained, when, the considerable oscillations caused by the change of regimen having ceased, the urinary secretion had resumed its natural course.

The averages in twenty-four hours were :

For G. M. 1444 cc. of urine 4·140 PhO^5 9·363 NaCl 27·232 Ur.
For H. S. 1635 cc. „ 4·421 „ 9·865 „ 31·298 „

To study the influence exercised by coffee on the nutrition, the two subjects were continued under the diet indicated, with this difference, that in place of two glasses of water after breakfast and two glasses of water after dinner, they had each time an infusion of coffee, each portion being prepared with 45 grms. of berries.

From the first day of this altered diet, the feeling of sinking and unsatisfied hunger completely disappeared in both subjects. They were both in perfect health, and well disposed for work ; their urine remained in the normal state.

The following are the results obtained under the influence of this modified diet :

Second Series of Experiments on G. M. Regimen with Coffee.

Dates.	Cubic cent of urine.	Reaction.	Peculiar properties.	Phosphoric acid.	Chloride of sodium.	Urea.
June				gr.	gr.	gr.
3	2102	acid	yellow; clear	4·113	12·755	26·181
4	1480	*id*	*id*	3·669	9·570	24·311
5	1620	*id*	*id*	3·822	3·380	25·185
6	1500	*id*	*id*	3·782	11·543	24·000
7	1846	*id*	*id*	3·876	11·201	24·113
8	1520	*id*	*id*	2·936	8·255	23·300
9	1340	*id*	*id*	2·673	6·932	21·440
10	1620	*id*	*id*	3·572	6·285	19·440
11	1560	*id*	*id*	3·439	8·070	21·360
12	1520	*id*	*id*	2·818	6·469	20·984
13	1322	*id*	*id*	3·024	7·001	20·251
	7362			15·526	34·757	103·475

Second Series of Experiments on H. S. Regimen with Coffee.

Dates.	Cubic cent of urine.	Reaction.	Peculiar properties.	Phosphoric acid.	Chloride of sodium.	Urea.
June				gr.	gr.	gr.
3	1594	acid	yellow; clear	4·212	10·170	30·992
4	1812	*id*	*id*	4·010	11·285	28·285
5	1789	*id*	*id*	3·825	7·351	29·104
6	1921	*id*	*id*	3·154	8·411	27·243
7	1934	*id*	*id*	3·203	9·052	24·532
8	2112	*id*	*id*	3·025	8·515	22·001
9	1946	*id*	*id*	2·981	8·910	21·453
10	1989	*id*	*id*	2·899	9·102	22·211
11	2002	*id*	*id*	3·201	8·802	21·360
12	1976	*id*	*id*	2·912	8·769	21·415
	10028			15·018	44·098	108·440

The mean quantities of urine and solid products excreted in twenty-four hours by G. M. were 1522 cc. of urine, 3·105 of phosphoric acid, 6·951 of chloride of sodium, and 20·695 of urea.

By H. S. 2005 cc. of urine, 3·001 of phosphoric acid, 8·819 of chloride of sodium, and 21·888 of urea.

The numbers in the preceding Tables shew that, during the first days the infusion of coffee exercised no perceptible influence on the secretion of the solid matters of the urine. This influence was distinguishable only after the seventh day in G. M., and after the sixth in H. S. Although the quantity of urine was increased, a notable diminution of the solid materials of the urine was proved. The author thence concludes, that coffee acts on the system by diminishing the activity of the intimate reactions and the metamorphoses of the organised substances. To study the effects which would be produced by a strong dose of coffee, the author caused each subject to take twice a day an infusion of 90 grms. of this substance. The symptoms observed were the following: augmented activity of the heart, accelerated pulse, excitement, sweats, anxiety, vertigo, and afterwards prostration, and sleep disturbed by dreams.

These symptoms were much more marked in G. M. than in H. S.

Is the peculiar manner in which coffee acts on the system due to the caffeïne; to an empyreumatic oil; or to some other principle? This was the last question which the author attempted to answer.

He obtained the following results with respect to it.

G. M. having been subjected to the regimen originally described until the secretion of the materials of the urine had become regular, received morning and evening 0·2 grm. of caffeïne dissolved in water. He secreted in twenty-four hours: 1928 cc. of urine, 3·768 of phosphoric acid, 9·546 of chloride of sodium, and 24·088 of urea.

Under the influence of the normal regimen, the secretion of the materials of the urine attained its highest limit; it diminished again on the administration of the caffeïne, but not in the same proportion as under the influence of the infusion of coffee. We may thence conclude that coffee contains, independently of caffeïne, another substance which exerts a certain action on the intimate reactions.

The dose of caffeïne indicated above produced no other effects than an augmented action of the heart; but a double dose produced in G. M. a quick pulse, tremblings, continual desire to micturate, a peculiar excitement, confusion of ideas, visions,—in a word, a species of intoxication, followed by a deep sleep.

The following are the results of two series of experiments on another subject, L. B., which appear to confirm the above:

L. B., subjected to a normal regimen without caffeïne, secreted 1200 cc. of urine, 7·790 of chloride of sodium, 3·910 of phosphoric acid.

L. B., under the influence of the same diet, with caffeïne, gave 1340 cc. of urine, 6·890 of chloride of sodium, 3·705 of phosphoric acid, and 22·230 of urea.

After an interruption of eight days these experiments were renewed, and gave the following results:

Regimen, without caffeïne, 1250 cc. of urine, 6·980 of chloride of sodium, 3·855 of phosphoric acid, and 25·010 of urea.

Regimen, with caffeïne, 1276 cc. of urine, 6·790 of chloride of sodium, 3·690 of phosphoric acid, and 20·800 of urea.

The author did not stop there. He wished to study the action of

the empyreumatic oil of coffee on the system. To obtain this oil, a certain quantity of roasted coffee berries were distilled with water, until the residue became inodorous. The product obtained, true distilled coffee water, possessed exactly the odor and flavor of the decoction itself. G. M. took four glasses each day, in which was the empyreumatic oil of about 120 grms. of roasted coffee. Under the influence of this regimen, he passed on the average 1789 cc. of urine, 3·479 of phosphoric acid, 10·307 of chloride of sodium, and 20·271 of urea.

The ingestion of this distilled coffee water produced an agreeable excitement, a moderate perspiration, and the feeling of continual hunger completely disappeared. A double dose occasioned congestions, abundant sweats and sleeplessness.

The general results of the author's experiments may be expressed by the following propositions:—

1. Coffee exerts two distinct actions on the economy, which appear difficult to reconcile. It augments the activity of the vascular and nervous systems, whilst, on the other hand, it lessens the rapidity of the metamorphosis of the tissues.

2. The salutary state of excitement produced by the use of coffee is due to the simultaneous action of the empyreumatic oil and caffeïne.

3. The comparatively slow intimate decomposition must be attributable principally to the empyreumatic oil, caffeïne contributing to this effect only when acting in rather large doses.

Annalen der Chemie und Pharmacie.

On Stagnant Waters *in General, and on the* Waters *of* Ponds *in particular.* By M. Marchand.

In speaking, in my memoir on drinkable waters, of the modifications which the organic matter of running waters undergo under the influence of light, I should have noticed the production, in these circumstances, of globules of green matter, the proportion of which is always in direct ratio with the quantity of organisable matter dissolved. The animated globules which are thus generated, are known in science by the name of Priestley's green matter, from the learned observer who first proved their production.

When these phenomena of vitality are accomplished in a liquid, this liquid, under all reacting influences, contracts different properties, dissolving new principles, whose nature is worthy of being determined, for they in no way resemble those whose normal presence I noticed (in a former memoir) in natural waters, since they emanate from animal and vegetable productions which are developed in one of the three conditions, into the consideration of which I shall now enter.

First Case.

Waters exposed to the action of light.—In these circumstances they are soon covered with a green, and sometimes red, matter, the quantity of which goes on increasing; afterwards, this matter is diffused throughout the mass of the liquid, and is even deposited at the bottom of the vessel, at the same time that the water, by spontaneous evapo-

ration, diminishes in volume. When the action of light on the inferior layers of the liquid is intercepted by the colored matters resting on its surface, numerous microscopic animalculæ are developed, whose generations follow each other in rapid succession, and increase the quantity of organic matters deposited, by undergoing putrid fermentation. The water then always contracts injurious properties, for if it is filtered, it is obtained of a yellow color; it is neutral with respect to coloring matters; it has an insipid and disagreeable taste; it reduces the salts of gold, and prevents the action of iodine on starch. Analysis detects in it the presence of considerable proportions of vegetable and animal albumen, as well as of humus, sometimes in large quantity. . However, the air which it retains in solution is ordinarily highly oxygenous, for, in several experiments I found it to be formed, in one-hundred parts, of,—

Oxygen	33·5
Nitrogen.	66·5

and its proportion amounted to twenty-six cubic centimetres per litre.

Second Case.

Waters covered with vegetable species, but which do not wash any.—The phenomena of vegetation, under the influence of the solar rays, are accomplished in a regular and normal manner; and under the covering of verdure, numerous microscopical animalculæ are developed, some of which become visible to the naked eye. In time, the debris of vegetables and animalculæ accumulate at the lower part of the liquid, and contract in it, as in the foregoing case, but in a higher degree, putrid fermentation; the water is of a fawn color; it exhales an odor of organic matters in the state of decomposition, and when it contains sulphates, sulphuretted hydrogen may be exhaled. It is ordinarily slightly alkaline. Analysis always detects in it also very appreciable quantities of albuminous matters, which commonly communicate to it the property of frothing by agitation. Its color is due to the presence of dissolved humus. The proportion of atmospheric air in it amounts on the average to twenty-two cubic centimetres per litre, and it is ordinarily formed, in one-hundred parts, of,—

Oxygen	20
Nitrogen	80

Waters of this class, even more than those of the foregoing class, possess the property of reducing the salts of gold under the influence of heat, and of disguising the action of iodine on starch. Like them also, they contain little carbonic acid.

Third Case.

Waters washing, and bearing on their surface, vegetables in great number.—Like those subjected to the foregoing influences, these waters contain numerous animalculæ; but, thanks to the plants which they bathe, the phenomena of putrefaction which are developed in them are no longer appreciable by the sense of smelling. Nevertheless, the excess of the humus dissolved still communicates to the liquid a pale

yellow color, and frequently also a slightly acid reaction ; it may always be found, as also may albuminous matters, whose quantity may vary. The atmospheric air which they contain is ordinarily as abundant as in those of the last class, and as oxygenous. Their taste is insipid, and often disagreeable.

It may be conceived, that under the reducing influence of the phenomena of putrefaction above-mentioned, the nitrates are compulsorily converted into ammoniacal salts, the proportion of which, if they are not absorbed by the vegetables, must be considerably increased. This ammonia, thus produced, operates especially in effecting the solution of the humus. The dissolved salts also diminish in proportion, by passing into the constitution of organised beings.

It is evident, *ceteris paribus*, that the stagnant waters examined in the second case are the most dangerous ; they are more charged than the others with organic matters, by being subjected to the influence of matters in putrefaction, in the injurious properties of which they participate more.

When they are evaporated, all these stagnant waters leave, in contact with atmospheric air, earths impregnated with putrescible matters, which become the source of an active production of proto-carburetted hydrogen marsh-gas, with which they are saturated, contracting then still more injurious properties ; for this gas, as is known, is the principle, and the most active vehicle, of marsh miasmata. I have remarked that its production is so much the more certain as the waters contain a larger quantity of vegetable albumen.

If, now, we seek to apply the foregoing to the waters of ponds, which are in daily use on all the plains and in the rural portions of the country of Caux, we find that these waters are always turbid, thick, but slightly aërated, and, consequently, very unwholesome. When they are exposed to the action of the solar rays, and almost all of them are subject to this influence, they are rapidly covered with an organised matter, composed of numerous vegetables of the inferior classes, amongst which may be particularly distinguished the various kinds of *Lemna*, L. They ordinarily wash a great number of plants ; several classes of animals, but especially infusoria, insects and reptiles, which live in them, deposit in them their seeds and their eggs, and die in them, leaving their remains, which putrify in them. Frequently also rain waters, before reaching these reservoirs, wash lands charged with organic detritus in process of decomposition.

In these conditions, the waters of ponds often present the worst characters of stagnant waters, and their use as drink cannot be without danger ; for the albuminous principles which they contain, like all matters assimilable by the human organism, are capable of producing actual poisonous symptoms when taken into the stomach whilst in course of decomposition ; thus, the populations which are supplied with this kind of water, when they use it as drink, are liable to contract diseases in which the intermittent febrile symptoms peculiar to paludian affections, are frequently recognisable. Their employment is, therefore, so much the more dangerous, as by a long continued drought,

and as under the influence of spontaneous evaporation, the albuminoïd matters are accumulated in greater quantity.

Stagnant Waters in contact with the air and sheltered from Light. Water of Cisterns.—The following are the results of my researches on pure rain-waters, and on the same waters mixed with putrescent matter, when they are subjected to the action of the air, screened from all luminous influence.

When the alimentary waters of cisterns come into these receptacles, without containing animalculæ in suspension, none are developed in them; and this is intelligible, since the intervention of the luminous rays is indispensable for the generation of the first germs of organised matter. When, in coming into the cisterns, they contain insects, infusoriæ, &c., these may continue to be developed in them, if diffused light has only a slight access to the reservoir; if not, they are rapidly destroyed, because the complete absence of light to beings destined to live under its most active influence, is almost always a cause of death. Nevertheless, if these waters, on arriving in their reservoirs, contain Priestley's green matter, the latter, as M. G. Gros has observed, takes the animal direction, and gives rise to the various animalculæ which are derived from it, and which are always then found in this liquid.

Now the oxygen which waters left to themselves continue to absorb reacts on the constitution of the organic matters dissolved, induces their gradual division, and their final conversion into water, carbonic acid, and matters which their complete insolubility isolates from the liquid, they are precipitated to the bottom of the reservoir, where they are incapable of undergoing putrid fermentation. It is thus that in determined conditions, the two series of water to which these observations relate, were found after being left to themselves for five months, restored to a remarkable state of purity, with the exception of a few flakes of insoluble matter which remained in suspension, communicating to the waters a milky appearance, which filtering-paper alone was sufficient for separating. The greater portion of the organic matters originally dissolved were deposited and fixed, in the insoluble state, at the bottom of the reservoirs; the waters of both series were saturated with atmospheric air very rich in oxygen, for at the temperature of 54° F. they contained on the average, per litre, twenty-three cubic centimetres, composed, in 100 parts, of

Oxygen	32 to 33
Nitrogen	68 ,, 67

Comptes Rendus, November 7th, 1853.

PHYSICS.

Thermo-Chemical *Investigations Concerning* Combinations *Formed in* Multiple Proportions. By M. P. A. Favre.

(Continued from page 249.)

Oxygenous Compounds of Arsenic.—I will dispense with the details concerning these experiments, which were calculated from the opera-

tions by which I determined the calorific equivalents of phosphoric, phosphorous and hypophosphorous acids, with this difference only, that the arsenious acid was weighed directly.

The metallic arsenic was employed as pure as possible. The arsenic and the arsenious acid were oxidised with hypochlorous acid.

The following were the reactions :—

I. $As + 5ClO + x\,Aq. = AsO^5 + Cl^5 + x\,Aq.$ (for arsenic acid.)

II. $AsO^3 + 2ClO + x\,Aq. = AsO^5 + Cl^2 + x\,Aq.$ (for arsenious acid.)

The reactions terminated suddenly, and gave rise to the observation of the two distinct phases established in the formation of the acids of phosphorus.

Arsenic Acid.—1 grm. of arsenic converted by means of hypochlorous acid into arsenic acid, gave,—

I. . . .	1975·5
II. . . .	1961·5
Mean.	1968·5 units of heat.

Calculation, based on equation I., gave 110787 units of heat, as the calorific equivalent of arsenic acid.

Arsenious Acid.—1 grm. of opaque arsenious acid in powder, converted into arsenic acid by hypochlorous acid, gave,—

I. . . .	503·8
II. . . .	509·6
Mean.	506·7 units of heat.

Calculation, based on the reaction indicated by equation II., gave 35424 units of heat to express the heat disengaged by the conversion of solid opaque arsenious acid into arsenic acid in solution ; we have, therefore, for the calorific equivalent of solid opaque arsenious acid.

110787—35424 = 75363 units of heat.

In order to ascertain what the opaque arsenious acid would have given in being converted into arsenic acid, it was necessary to determine the quantity of heat brought into play by the solution of this acid in water. This quantity was moreover indispensable to be known for the accurate determination of the calorific equivalent of the chloride of arsenic according to my method.

1 grm. of opaque arsenious acid, in dissolving, absorbs 37·1 units of heat.

This number multiplied by 99 (the equivalent of arsenious acid), gives 3673 units of heat absorbed.

If we had caused hypochlorous acid to react on arsenious acid previously dissolved, the number expressing the reaction would have been augmented by the number given by the solution of arsenious acid not previously dissolved ; we should have obtained, 39097 units of heat to express the heat disengaged by the transformation of dissolved opaque arsenious acid into arsenic acid in solution.

We find, therefore, for the calorific equivalent of dissolved opaque arsenious acid

110787—39097 = 71690 units of heat.

It is known that arsenious acid may occur in two isomeric states: the vitreous and the opaque.

It is likewise known that heat tends to convert the opaque into the vitreous acid, whilst the latter passes more or less rapidly into the opaque state, according to the circumstances in which it is placed. Finally, it is known that when this last transformation is suddenly effected, it is accompanied by an emission of light.

I have thought that it would be interesting to determine the quantity of heat brought into play during these transformations.

This determination presented no difficulty: it was sufficient to treat successively the same solution of potassa at 40°, employed in great excess, with 1 grm. of each of the two forms of arsenious acid taken in the solid state. The employment of a solution of potassa at 40° allowed of a very rapid solution and combination.

The following are the results furnished by the experiment.

1 grm. of solid opaque arsenious acid, in combining with the dissolved potassa and marking 40°, disengages

I. . . .	93·26
II. . . .	93.92
III. . . .	93·55
Mean	93·57 units of heat,

therefore 1 equivalent disengages 9211 units.

1 grm. of solid vitreous arsenious acid, in combining with potassa at 40° disengages

I. . . .	105·81
II. . . .	107·16
Mean	106·48 units of heat,

or, for 1 equivalent 10542 units.

10542—9211 = 1331 expresses the quantity of heat which is found in the *latent state*, in vitreous arsenious acid compared with opaque arsenious acid, both retaining the solid state.

It likewise follows from the results furnished by the experiment, that if the two acids can enter into solution in hydrochloric acid without undergoing transformation, it is not the same when they are united with potassa; the opaque arsenious acid alone appears susceptible of entering into combination. If this explanation be rejected, it must be admitted that the vitreous acid absorbs, in entering into solution, 1331 units of heat less than its isomer. I intend to elucidate this point by experiment; for if this were so, there might exist in solution two arsenites of potassa, containing arsenious acid in different states.

By comparing together the heats disengaged by the formation of arsenious acid and that of arsenic acid, we find that the two calorific numbers are,— : : 1 : 1·54

The calorific equivalents of phosphorous and phosphoric acids present sensibly the same relation: they are,—

: : 1 : 1·49

starting from ordinary phosphorus.

The relations do not deviate from the ratio of 3 to 5, which is that of the equivalents of oxygen in the two groups of acids under consideration.

We may remark, however, that the parallel of constitution can be strictly permitted only when we know beforehand which of the three phosphoric acids in solution, may be compared with the phosphorous acid dissolved.

The researches of M. Wurtz lead us to regard phosphorous acid $PhO^3, 3HO$, as a ternary hydrated and bibasic acid.

$$PhHO^4, 2HO.$$

It would follow that the combination of the arsenites would not allow of their assimilation to the phosphites.

If we compare the calorific equivalents of the three acids of phosphorus, beginning with hypophosphorous acid, we find that they are, among themselves

$$:: 1 : 2{\cdot}91 : 4{\cdot}36.$$

I may mention that, according to M. Wurtz, hypophosphorous acid does not belong to the class of oxygenous binary acids ; it would be a ternary hydrated and monobasic acid, of the form of PhH^2O^2, HO.

Chlorated Compounds of Phosphorus and Arsenic.—The method employed consisted in effecting the decomposition of a known weight of various chlorides by water in excess, estimating the quantity of heat disengaged. By introducing into the calculation of x elements previously determined, namely, the heat of formation of oxygenous acid corresponding to the chloride employed, besides the heat disengaged by the formation of hydrochloric acid and that absorbed by the decomposition of water, we are enabled to arrive at the heat absorbed by the separation of the elements of the chloride under consideration.

Perchloride of Phosphorus.—The following is the action on which the calculations were based for obtaining the value of x :—

$$PhCl^5 + 5HO + xAq = PhO^5 + 5ClH + x\,Aq.$$

In this reaction, two phases are observed as regards the calorific effects, as might be foreseen from what has been said before relative to the formation of nascent phosphoric acid in contact with water.

The determination of the weight of the perchloride was made in the following manner :

An eprouvette, containing a great excess of water in proportion to the chloride to be decomposed, was accurately weighed before being introduced into the calorimetric muffle. The eprouvette being placed, a convenient quantity (but not weighed) of perchloride of phosphorus was introduced into it ; the reaction was effected without any disengagement of gaseous or volatile products out of the apparatus ; so that the weight of perchloride might be deduced from a second weighing of the eprouvette and its contents, after the observation of the calorimetric effects.

1 gr. of perchloride of phosphorus in its reaction on water disengages,

I.	. .	660·60
II.	. .	654·56
Mean		657·58 units of heat,

and for the equivalent $PhCl^5 = 209{\cdot}5$ gr.

$$R = 209{\cdot}5 \times 667{\cdot}58 = 137753$$

Now $$R = A + B - x - D$$

Whence

R=137753

A=209476 for the formation of PhO^5 in dilute solution,

B=200960 for the formation of 5ClH dissolved,

D=172310 for the decomposition of five equivalents of water.

By substituting in the last equation for the numbers A, B, and C, their numerical values, we obtain for the calorific equivalent of perchloride of phosphorus

$$x = 100373 \text{ units of heat.}$$

M. Abria, who determined the heat of formation of perchloride of phosphorus, $PhCl^5$, by a method which he has not published, gives a number which, referred to 32 gr. of phosphorus, would be = 102368 units of heat disengaged. Mr. Andrews obtained, as the equivalent of phosphorus in $\frac{1}{4}PhCl^5$, the number 109504.

Protochloride of Phosphorus.—The experiment was conducted in in the same manner as the foregoing.

The calculations are based on the reaction :—

$$PhCl^3 + 3HO + x\,Aq = PhO^3 + 3ClH + x\,Aq$$

analogous to that which phosphoric acid gives rise to.

The calorific reaction presents only one phase, the duration of which does not exceed five minutes.

1 gr. of protochloride of phosphorus in its reaction on water disengages,—

I.	. . .	450·9
II.	. . .	455·7
Mean.		453·3 units of heat.

One equivalent disengages $453{\cdot}3 \times 138{\cdot}5 = 62782$, whence :—

$$R = 62782$$

We have:— $$R = A + B - x - D$$

Whence

R=62782,

A=140394 for the formation of dissolved hydrated PhO^3

B=120576 for the formation of 3ClH dissolved

D=103384 for the decomposition of three equivalents of water,

Whence

$$x = 94804 \text{ units of heat}$$

to express the heat *disengaged* by the formation of protochloride of phosphorus.

By comparing, on one hand, the quantities of heat disengaged by the formation of protochloride and perchloride of phosphorus, and, on the other hand, those disengaged by the formation of phosphorus and phosphoric acids (from ordinary phosphorus), we find that they are, for the combinations of chlorine,

$$:: 1 : 1{\cdot}06$$

and for the corresponding combinations of oxygen,

$$:: 1 : 1{\cdot}49.$$

It is evident that the relation is very different in the oxygenous combinations of phosphorus and in the corresponding chlorides of the same metalloïd. Indeed, the two chlorides do not affect the same physical state, but the difference is greater than would be expected in this case.

I may mention that, according to the experiments of M. Cahours, the equivalent of perchloride of phosphorus $PhCl^5$, represents eight volumes of vapor, whilst the equivalent of protochloride $PhCl^3$ represents only four volumes of vapor. Either we must admit that the molecule of the perchloride is relatively less condensed than that of the protochloride, or else we must suppose that the perchloride of phosphorus is separated into chlorine and protochloride at the temperature at which its vapor density was taken, we find in these circumstances an argument for explaining the slight increase of heat disengaged in passing from protochloride to perchloride of phosphorus.

Moreover, a phenomenon is manifested, which is the inverse of that which was proved by M. Silbermann and myself, for the metallic chlorides, since the heat of chloridation of the equivalent of a metal always exceeds the heat of oxidation of the equivalent of the same metal.

Chloride of Arsenic.—We are at present acquainted with only one chloride of arsenic, $AsCl^3$, which corresponds to arsenious acid.

The operation executed on chloride of arsenic was analogous to the foregoing.

The reaction used as a basis is,—

$$AsCl^3 + 3HO + x\,Aq = AsO^3 + 3ClH + x\,Aq.$$

It was found that 1 grm. of chloride of arsenic disengaged by its reaction on water

I.	. . .	93·27
II.	. . .	93·50
Mean.		93·39 units of heat.

An equivalent of chloride of arsenic disengages—

$$93{\cdot}39 \times 182 = 16997 \text{ units};$$

Whence

$$R = 16997.$$

We have besides

$$R = A + B - x - D.$$

Whence

R=16997,
A=71690 formation of opaque arsenious acid in solution
B=120576 formation of three equivalents of hydrochloric acid dissolved
D=103386 decomposition of three equivalents of water,

Whence

$$x = 71883 \text{ units of heat}$$

disengaged by the formation of 1 equivalent of chloride of arsenic. This number differs little from that of Mr. Andrews, who obtained 74550 units for the formation of $AsCl^3$.

As only one chloride of arsenic is known, we cannot make as complete a comparison between the chloruretted and oxygenous compounds

of arsenic as we have done in the case of phosphorus. We see only that the calorific equivalent of opaque arsenious acid 75363 is higher than the calorific equivalent of chloride of arsenic 71883. This result is in the direction of the phenomena observed as regards the combinations of phosphorus. But the difference is much less marked; we are the less surprised when we remember the energy with which arsenic takes fire in chlorine.

(To be continued.)

ORGANIC CHEMISTRY.

INVESTIGATIONS *respecting the* ETHERS. By M. M. BERTHELOT.

I. Formation of Ethers composed of Ether and Acids.—1. Can ether, which is formed from alcohol by the elimination of the water, reproduce the alcohol from which it proceeded, or even the combinations of which this alcohol formed an integral part? This question has been proposed more than once, and notwithstanding certain facts enunciated from time to time, it is not, I believe, looked upon as settled. Nevertheless, it is not without importance. In fact, in a very widely extended theory, the compound ethers are represented by an anhydrous acid, plus oxide of ethyle, a body which is isomeric or identical with ether. The direct production of the compound ethers, by means of ether and the acids, is of a nature to support this view, although it is susceptible of other explanations.

2. This production is obtained by heating acid and ether to about 680° or 752° F. in extremely strong closed tubes.

3. I have produced benzoïc ether with ether and benzoïc acid. The body thus formed possesses the odor and specific properties of ether; it boils at 410° F., and gives by analysis:

C = 72·2	The formula requires	C = 72·0
H = 6·7	„	H = 6·7

Treated with potassa and water, it reproduces benzoïc acid, and, instead of ether, a volatile, inflammable liquid soluble in water, developing the odor of butyric ether; when put in contact with a drop of a mixture of sulphuric and butyric acids, these characters appertain to alcohol.

The ether employed in the preceding experiment had been agitated five times successively with its own volume of water, so as gradually to dissolve the half, then dried over chloride of calcium and rectified. It gave after nine hours contact with benzoïc acid at about 680° F., 30 per cent. of benzoïc ether (16 grms. produced 5 grms.). The formation of benzoïc ether commenced from 572° F.; but at this temperature, even after a prolonged contact, it was not abundant.

For the purpose of acquiring greater certainty relative to the purity of the ether employed, I rectified the ether, purified by the preceding method, at a fixed temperature until half only was distilled; and I repeated the distillation at a fixed point on this half, only again removing half the product. The ether thus purified gave after three

hours of contact with benzoïc acid at about 680° F., 25 per cent. of benzoïc ether.

4. Ether and butyric acid produce butyric ether at 680° F. (6 h.). The liquid in the tubes submitted to distillation did not appear to me to furnish any thing but ether, water, butyric ether and butyric acid. It disengaged no gas.

5. Ether and palmitic acid produce at 680° (9 h) palmitic ether fusible at 69° F.

In the various cases which I have mentioned, neither acid nor ether enter entirely into combination, whatever may be the respective excess of either one or other of these bodies.

6. Ether and water, heated to the limit of decomposition (842° F.?), do not combine.

II. Direct formation of Ethers by means of Alcohol and the Acids.—1. Alcohol, by its combination with acids, gives rise to the ethers. This union is produced either by the direct way, or by the intervention of a mineral acid. The direct combination is in general very easy with the powerful acids; but with the organic acids, with acetic acid for example, it becomes very slow and very incomplete. With the aid of sulphuric acid, on the contrary, as shewn by M. Thénard, the combination operates immediately and almost entirely.

I have endeavored to generalise the direct preparation of the ethers, by operating in closed vessels with the aid of time and heat. The following are the facts which I have observed:

2. Towards 392° to 482° F., the combination of the alcohols with the fatty acids operates quickly and easily. I have also formed at 482° F. methylpalmitic ether, a crystalline body fusible at 82° F., solidifying at 69° F.

Ethylpalmitic ether, fusible at 70° 7′ F., as shewn by M. Fremy, solidifying at 64° F., reproducing with potassa, palmitic acid fusible at 141° 4′ F.

And amylpalmitic ether, a waxy substance, fusible at 48° F. reproducing with potassa, palmitic acid fusible at 141° F.

The combination of the alcohols with the fatty acids is never complete, either as regards the alcohol or the acid. But the formation of these three ethers is the most abundant in the presence of an excess of acid, which is afterwards separated by lime and ether. These three ethers, heated anew to 500° F. during fourteen hours, with eight to ten times their weight of palmitic acid, are found, after the operation, without any modification either in their properties or their point of fusion.

3. At 212° F., I produced in abundance, after thirty hours contact, benzoïc, acetic, and butyric ethers, especially the last. Stearic ether itself even commences to be produced at the end of 102 hours, but in very small quantity.

4. In the last case, if acetic acid is added to the mixture, the stearic acid etherifies completely in about two hours. This fact agrees with the known action of sulphuric and hydrochloric acids, only differing in the relatively weak action of acetic acid. It appears, especially in

the latter case, that the combination of stearic acid with alcohol is produced by that which takes place between the acetic acid and the alcohol. This phenomenon very clearly demonstrates the propagation of molecular movement.

The ready etherification of the fatty acids in an alcoholic liquor rendered acid even by acetic acid, appears to me to render the purification of these bodies a very delicate matter.

III. On the Decomposition of Ethers.—In the above remarks I have chiefly spoken of the formation of ethers. These bodies may likewise be decomposed by the same agents which determine their formation. Thus:

1st. Water heated to 212° F. for 102 hours with stearic and oleïc ethers begin to decompose them, regenerating stearic and oleïc acids. It has no effect on benzoïc ether in the same conditions.

2nd. Acetic acid diluted with two or three times its volume of water, after contact for 106 hours at 212° F., distinctly acidifies stearic ether without producing acetic ether; it partially decomposes butyric and benzoïc ethers, with production of butyric and benzoïc acids.

3rd. Fuming hydrochloric acid produces, in 106 hours at 212° F., a double decomposition with acetic ether (a fact already mentioned), butyric, benzoïc, and stearic ethers. The acids are set at liberty and hydrochloric ether is formed. The double decomposition is, however, never complete, except in the case of stearic ether.

Thus we can, at will, etherify a weak acid, or decompose its ether by the influence of hydrochloric acid, and even of acetic acid. This contradiction in the action of the same body results from the presence of water in excess in the one case, of alcohol in excess in the other. The relative amount and activity of the reacting acids also aid in these phenomena.

Comptes Rendus, No. 23, December 5th, 1853.

THERAPEUTICS.

On HYDROCOTYLE ASIATICA *for the cure of* LEPROSY.

IF Asia saw the birth of our first parents—if she has been the cradle of human knowledge—and if, likewise, we owe to her those things which are most useful to man,—corn, rice, sugar, spices, tea, coffee, the vine, and almost all our fruits—she has given us, as if in compensation, leprosy, small-pox, and cholera. This last, of more recent importation, appears unfortunately likely to become acclimated amongst us; small-pox has given way before vaccination; and leprosy, which was the terror of Europe in the middle ages, appears now to have returned to the climates which originated it. This last scourge still touches us, however, in the persons of those amongst us whose duties, or the interests of commerce, fix them in these distant countries. This reason would not exist if simple humanity caused us to consider it a duty to collect the attempts made to deliver our species from this hideous malady.

In the "Moniteur Officiel" of Pondicherry, of the 5th August, 1853,

we find two letters making mention of a new remedy for leprosy. The first of these letters is written by M. Lépine, who, as a recompense for his services, is recalled to France with the rank of pharmacien of the first class. We regret to read therein that the formulæ of the preparations used in the *pharmacie* of Pondicherry remain his property. The second letter is from M. Boileau, physician in Mauritius, himself a leper, and the author of the discovery. We will extract the essential passages, after having said what plant is proposed by this practitioner as a remedy for leprosy.

The *Hydrocotyle asiatica* of Linnæus, to which M. Boileau gives the name of *bevilacqua*, has long been known. It is a small umbelliferous plant, with running branches, and provided with leaves which are very similar to those of the violet, which gives it some resemblance to the latter, if it were not for its single bells, to which succeed small fruits composed of two pericarps, separable at maturity.

This plant inhabits the moist places of almost all the hot countries of the Eastern Hemisphere, such as the Malay islands, India, Ceylon, Central Africa, and doubtless also America. Rheede first described and designated it under the name of *codagen;* Rhumphius gave it those of *pancaga* and *pes equinum*, and says that it is used externally as a vulnerary, and internally as a diuretic, and even as food. Ainslie also mentions it under the name of *tamool of vullarey*, and says that it is used as an infusion, conjointly with fenugrec, in infantile colic and fever. He adds, that on the coast of Coromandel the leaves are applied on any parts of the body which have been wounded or bruised, for the purpose of preventing inflammation. The following is a portion of M. Boileau's letter to M. Jules Lépine, pharmacien to the fleet at Pondicherry:

"The Mauritius journals having mentioned that you were trying, at Pondicherry, the administration of the plant of which I have spoken as *manifestly ameliorating* the condition of the lepers, and to which *I have given* the name of *bevilacqua (Hydrocotyle asiatica)*, I was endeavoring to find means of communicating with you, when your letter reached me.

"In the first place, the *bevilacqua (Hydrocotyle asiatica)*, which is of the family of the Umbelliferæ, cannot be confounded with the *cuichunchilli* of America, which is of the family of the Violaceæ.* The leaves alone bear some resemblance to those of the violet, which originally led me into error. But the *bevilacqua* consists of a greyish, round, fleshy root, from the neck of which spring leafy branches, often several feet long, exhibiting at distances swellings from whence proceed roots, leaves, and the organs of reproduction. Violets do not shew any such tendency.

"Is the *bevilacqua* a specific in leprosy? In my first articles I did

* The root of the *cuichunchilli* is mentioned in the "*Histoire naturelle des drogues simples*," *tome* III., p. 89. A Lady having sold *an ounce* to a Governor of Guadeloupe for the sum of a *thousand francs*, I may be permitted to say that this is doubtless the most wonderful effect that this root was ever known to produce.

not approach this question ; I had but one aim : *to console my brothers in misfortune, and to draw the attention of medical men and the commiseration of the public to their sufferings.* A leper myself, I knew how much the prejudice against us had affected me, in fact to such a point that I had voluntarily secluded myself and neglected my most material interests. I had found it a certain alleviation to my sufferings, and I wish these other unfortunate beings to participate in it. I wrote only for them, little thinking that my words would be echoed beyond the coasts of Mauritius. I was mistaken, even Indian journals repeated my words. Thank God if I have been able to give some comfort to any miserable lepers in other regions.

"The amelioration which I have experienced myself, as well as fifty-seven patients, has been officially proved, at Réunion, by Dr. Leroux, of the Civil Hospital. You tell me that two of your patients have received the same benefit from it. I now add that I have had three young patients under treatment for three months, whose improvement has been so great as to lead me to hope for a certain cure. A doctor, in the district of Mocha, has told me of a patient who was radically cured by *bevilacqua* only ; and of another nearly recovered by the plant united to preparations of iodine. All leads me to believe, that by the end of this year, the question of the *bevilacqua* will be settled ; but from this time forward lepers may enjoy the hope of cure either by *bevilacqua* alone, or associated with other substances, *such as the preparations of iodine.*

"But is the treatment which I have described as having been followed in my first twelve cases, invariable? I by no means presume to say so, and the number of my patients having increased, I have myself perceived that I had augmented the doses too rapidly, and that the union of powders, syrups, &c., from the commencement had been too active. The following plan is the one which I now pursue :

"**Preparation.**—From a fortnight to three weeks of tisanes, tepid baths and vapor baths. One ounce of the whole plant dried in the shade, for a bottle of tisane, to be taken in the course of the day. Three pounds of the green plant for a large bath, and five of the same green plant for a vapor bath ; some aperient medicines, preceded by an emetic.

"**1st Treatment.**—Take the syrup alone, increasing from one spoonful per day each day until three are taken ; persevere from three weeks to a month, at three spoonfuls each day ; do not increase further unless there should be but little improvement ; should such improvement be manifest, continue the same dose. Continue, if necessary, up to eight spoonfuls each day ; continue the purgatives and one tepid bath each week. When eight spoonfuls are attained, take them as long as the improvement continues. Suspend the medicine when the amelioration stops, and for a fortnight take a tepid bath each day, and a vapor bath every four days ; purgatives and frictions with the pomade.

"**2nd Treatment**—Take the powders with the syrup, but be careful to increase the doses with moderation. I shall not continue this subject further, not wishing to influence any one. Others will be able to

do better than I can, who am practising although still ill, which must interfere with my experiments. Therefore I would recommend my fellow-laborers to follow their own opinions, and to conform to the symptoms which may manifest themselves, rather than to confine themselves to a treatment laid down beforehand, and which is not likely to suit all cases.

"(Signed) A. BOILEAU."

Nothing could possibly be better thought of. We should, however, have preferred knowing a little of the composition of the *syrup*, the *powders*, and the *pomade* of which the author speaks, so as to judge whether the success of which he speaks should be attributed to the *Hydrocotyle asiatica* rather than to the iodureted or other preparations; but, at last, let him only cure the leprosy, and all mankind will applaud his success.

Journal de Pharmacie et de Chimie, December, 1853.

On the BOSWELLIA THURIFERA, *and on the* EMPLOYMENT *of* INCENSE *in* CARBUNCLE *and* MALIGN PUSTULES.

By M. LOUIS P. DESMARTIS, Senr.

AMONGST the *Burseraceæ*, a family of plants proceeding, as is well known, from the *Terebinthaceæ*, is found a genus which was established by Roxburgh, and dedicated to the naturalist Boswell. This genus consists of a small number of exotic trees, all natives of India. Their habits are similar to those of the *elaphrium*, and a gum resin exudes from their trunks, the flow of which is encouraged by incisions. Many *Boswellia* are cultivated in our hot-houses. The *Boswellia* give a substance known in commerce by the names of olibanum, male incense, or Indian incense. This product, like several other gum resins, mixed with aromatics, has been from time immemorial burned on altars: it was in former times a sanitary means employed to drive away the insects attracted by the odor of the sacrifices; now these aromatic preparations are burnt, generally in grand religious ceremonials. According to the illustrious Virey, the vapors of incense, by their action on the nervous system, produce peculiar emotions, augment the sensibility, and give more fervor to prayer.

Many old pharmacopœias recommend incense as a powerful astringent; they likewise recommend it in hemoptysis, and consider it generally as a hemostatic. It is a stimulant, as are all the gum resins, a stimulant which should only be prescribed with caution in active phlegmasia. It is now but little used internally, and tar-water, &c., are preferred.

Olibanum is,however, still put into some pharmaceutical preparations and into many agglutinative topical applications.*

It has long been known to possess the property of cleansing wounds

* Incense enters into the composition of the balsam of Fioraventi, and of theriacum, of pills of cynoglossum, &c. It is likewise put into the Vigo plaster *cum mercurio*, into the Apostle's ointment, pompholix, *martiatum*, botony, &c.

of a malignant nature, and very recently Signor Romei, an Italian medical man, has discovered the means employed by a peasant of Santa Fiora for the cure of carbunculous affections; this was nothing but the best Indian incense. The substance having been once discovered, the *modus faciendi* of the plaster was easy, for all that is necessary is to mix this substance with saliva so as to form a paste, which is spread on a piece of linen of suitable dimensions. This plaster should be renewed until the work of elimination is complete, after which the ulcerated surface may be treated, according to circumstances, either with emollients or detergents.

Signor Romei, and still more recently, Signor Caifassi, have obtained great advantages from this means, both in true carbuncle and malign pustule.

The number of affections called *incurable* becomes daily smaller. This is a new link removed from the chain of such diseases which surrounds mankind. A short time since we had an opportunity of proving the value of this medicament.

On the 31st of May last, the child of Elias Lesgourgues, living in the Rue du Mu, was brought to me. The child was only fourteen days old. Lesgourgues is a working shoemaker, inhabiting a by no means healthy house and an ill-ventilated room, to which the leather gives an odor which is very unpleasant to those unaccustomed to it, and certainly not salutary to those to whom custom has rendered it tolerable.

Lesgourgues told us that for about four or five days he had perceived a sort of lump on the upper and inner part of the right foot of the child, and we saw, in fact, a species of tumor, in the centre of which was a vesicle of the size of a hemp-seed. It was surrounded by a black slough, the circumference of which was inflamed and presented, to a great extent over the superior part of the foot, a large auriola of a deep red bordering on violet; it was difficult to diagnose anything but carbuncle.

Was it in consequence of the vitiation of the air of the place in which the child lived? Was the infection produced in this poor little creature because its very frail economy offered less vital resistance to the *virus*, because the persons surrounding it were none of them ill? Had leather proceeding from an animal affected with carbuncle, touched the infant?

We could not tell from what cause the infection was produced; but, whatever might be the etiology, we remembered having read, in one of the last numbers of the "*Revue thérapeutique du Midi*" (28th February, 1853), an extract from the work of Signor Caifassi on incense paste as a topical application in carbunculous affections.

We used the process indicated. Two days after the plaster was removed, taking with it the slough which took the whole dermis, and even took one of the subjacent muscles. The topical application was renewed, and a month afterwards the cure was complete, without producing any bad symptom.

M. Enlenberg, says the "*Deutsche Klinik*," had cured a case of malign pustule by means of another resinous product, creosote, which

is, as is well known, the result of the distillation of tar. In this case, creosote* was administered in small doses at the same time that it was applied on the affected part.

Creosote is, it is true, an anti-putrescent, but this is only one point as regards the carbunculous affections, in favor of this medicament; we think that olibanum, by which many cures have already been effected, should obtain the preference.

We should mention, that very recently M. Pomayrol, physician near Perpignan, has proved that simple walnut-leaves or the fresh bark of the tree applied, after piercing the *phlyctene*, on the parts attacked by *carbuncle* or *malign pustule*, is as efficacious a means as *sulphate of quinine* in intermittent fevers.† We are happy to see that this new medicament augments the chances of cure in this terrible complaint. Some time since, certain substances analogous to *incense* were used as fumigations in phthisis. This therapeutical means, extolled especially by Dr. Christison, appears to have been employed successfully in England, Italy, and Russia.

Phthisis can only be cured after the annihilation of the causes which have produced it, and these causes often vary with the individual. Certain it is, that some cases of phthisis which resist all the ordinary medicaments, are cured by change of climate, by residence at the sea-side, &c. But the poor, unable to leave their homes, cannot try this last means. We are persuaded that very advantageous results may be obtained by remaining a shorter or longer time in rooms in which well mixed vapors are suitably disengaged.

Whatever may be said, there are very distinguished medical men, who applying themselves from choice to a special line of practice, have made great progress in the branch of science to which they have devoted themselves. This may be easily understood, and indeed it must be so, for even quacks succeed *sometimes*, for we here find a simple curer of wounds succeed by treating carbuncle and malign pustule with incense, the cure of which is sometimes attained only by very great sacrifices.

"*Révue thérapeutique du midi.*"

AGRICULTURAL CHEMISTRY.

On the NUTRITIVE VALUE *of* RAPE-CAKE. By Prof. EMIL WOLFF.

THE author in his experiments readily obtained ¾ths of a pound of milk with 1 lb. of rape-cake; it has, however, been ascertained, that under other circumstances, especially with cows of higher milk-producing power, this result had risen considerably; so that 1 lb. of rape-cake will produce, according to circumstances, between ½ and 1½ lb of

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* The following is M. Enlenberg's formula:—

	Grs.
Creosote	1·60
Essence of turpentine	1·60
Camphorated spirit	120·00

† "*Revue thérapeutique du Midi*," *t.* IV., *p.* 338.

milk of very good quality, or on an average 1 lb. of milk. The quality of the milk bears reference only to the quantity of butter contained in it. The butter itself acquires a bad taste when the cow has been very plentifully fed with rape-cake; and, as a general rule, 2 lbs a day per head, which quantity was observed to produce the highest effect, is regarded as too much, if it be desired to obtain well-tasted butter. But when the milk is to be sold as such, a daily allowance of 2 lbs. of rape-cake is never too much; and the milk will then taste better in proportion, as the other food is rich in non-nitrogenous nourishment and poor in proteïne compounds.

In the production of milk, 1 lb. of rape-cake cannot be replaced by 2 lbs. of hay; for the purpose of maintaining an average living weight, in cows as well as in sheep, 1 lb. of rape-cake must be regarded as at least equal to 2 lbs. of hay; in many cases, especially when rape-cake is given in very small quantity with foods very poor in nitrogen (straw, potatoes, turnips, &c.), the equivalent of the rape-cake rises considerably, and the same nutritive effect is often obtained with 1 lb. of rape-cake as with 3 lbs. of hay.

The production of flesh is forwarded in as high a degree by feeding with rape-cake as by any other food. The author has observed the greatest effect to be produced with cows which were at their medium living weight, and which, during their highest production of milk, had been accustomed to a tolerably plentiful daily allowance of rape-cake. When these cows were deprived of the entire quantity of rape-cake, they rapidly fell to a considerable extent, but rose again regularly as soon as the daily allowance of 2 lbs. of rape-cake was restored to them. In this manner an increase of weight of 62 lbs. was produced in two cows in fourteen days; and this was obtained with 56 lbs. of rape-cake, so that 4 lb. of this substance effects an increase of fully 1 lb. in the living weight. After this the weight of the animals with the same diet remained pretty constant. From this it appears that with cows 1 lb. of rape-cake daily will gradually produce, and afterwards maintain, an increase of 15 lbs. in the weight of the animal. This rapid and considerable effect of the rape-cake was only possible under the peculiarly favorable circumstances for the production of flesh existing in the present instance. Had the daily allowance of rape-cake been raised to 3 or 4 lbs. per head, the weight of the animal would also gradually have risen to a corresponding extent; but this would hardly have been attained in so short a time, for the nearer the animal is completely fattened, the more slowly does it acquire weight, especially under the influence of the same food.

With sheep, 1 lb. of rape-cake in the daily food, supposing the whole quantity given to the animal to be composed of proper nourishment, produces gradually a weight of 20 lbs., at least in the first period of fattening, and afterwards apparently something less. The poorer the other food is in nitrogenous constituents, the more rape-cake may be given to the animal with good effect, and without giving cause to fear the production of meat of bad quality: nevertheless, it does not appear to be advisable to raise the daily allowance of rape-cake above $\frac{2}{3}$ds of

1 lb. If a sheep of average weight be fed daily with about 4 lbs. of beet-root and 1½ lb. of hay, together with a quantity of rape-cake, at first small, but gradually rising till it reaches 2–3rds of a pound, the weight of the animal will be increased, in from six to eight weeks, according to circumstances, to an average extent of about 13 lbs.; these 13 lbs. will have been produced by 28 or 30 lbs. of rape-cake, which the animal would consume in that time.

The favorable influence of the rape-cake on the quality of the manure produced during the fattening is worthy of a high degree of consideration, as rape-cakes are of all cattle foods the richest in nitrogen; and the presence of nitrogen in manure constitutes its highest value. The author has ascertained, by the direct examination of sheep's dung, that by far the greater part of the nitrogen of the rape-cake passes into the dung, and is also retained in it for a considerable time during its accumulation in the stall, if the dung remains tolerably moist, in consequence of the greater quantity of water taken by the animal as a result of the nature of its food, or if it be moistened from time to time with water. When the rape-cake is given to the sheep in smaller quantity, during the winter feeding, so as merely to keep the animal at an average living weight without fattening, it may be assumed with certainty that only ⅛th of the original quantity of nitrogen is lost in the process of nutrition and by decomposition in the stall; 5–6ths of the value of the rape-cake as a manure are consequently retained in the dung produced. If such an improvement of the manure may thus be ascertained to be the effect of feeding sheep upon rape-cake, the quality of cattle manure is not less improved by similar nourishment; 1 lb. of rape-cake produces on an average 1 lb. of milk; this, however, only contains 1–8th part of the nitrogen contained in the rape-cake; so that 7–8ths of the nitrogen of the latter must pass into the dung; and this nitrogen will be retained more readily than in sheep's dung, as cow's dung is so much wetter and colder, and consequently undergoes a more gradual decomposition.

When rape-cake is employed in larger quantity for fattening, besides this loss in nitrogen, which on the average only reaches 1–6th, or at the outside 1–5th, a larger or smaller quantity will be retained from the dung, according as the increase in the weight of the animal goes on faster or slower. The quantity of nitrogen fixed in the body during an increase, say of 100 lbs. in weight, cannot be determined with certainty; Boussingault's statement, however, that 3·66 lbs. of nitrogen are to be calculated for an increase of 100 lbs. in the living weight during fattening, appears to be too high in comparison with the quantity of nitrogen in the substance of an animal. Nevertheless, the author employs this statement as the basis of the following calculation, as no other observations have hitherto been made on this subject. In certain cases it appears that nearly the whole of the nitrogen contained in the rape-cake may be appropriated to the formation of flesh and milk; thus, such a case was, when in fourteen days (from March 27 to April 9) an increase in the living weight of two cows of 62 lbs. and 38 lbs. of milk was produced with 56 lbs. of rape-cake. These 56 lbs.

of rape-cake contained 2·8 lbs of nitrogen, whilst the flesh and milk produced under their influence contained 2·49 lbs. of that element; as soon, however, as the living weight corresponding to a certain mode of feeding is attained, and remains constant, only 1-8th of the nitrogen of the rape-cake is fixed in the milk, whilst 7-8ths pass off in the dung. If a longer period of fattening be adopted, and the entire quantity of rape-cake consumed during this period be taken into the calculation, we may assume that under otherwise not unfavorable circumstances, 1 lb. of living weight is produced on an average from 2½ lbs of rape-cake, both with cattle and sheep; the quantity of nitrogen contained in the rape-cake and the increase of weight stands in a proportion of 0·129 to 0·036 lbs., so that under such conditions nearly 3-4ths, or at least 2-3rds of the nitrogen originally present in the rape-cake becomes available in the dung, or in other words, the rape-cake loses only 1-3rd of its value as a manure by its employment in fattening cattle.

From all the experiments and observations instituted, the author comes to the conclusion, that in flesh and milk formation, as in assisting to maintain the living weight of the animal, scarcely any food exercises a greater influence than rape-cake; and that in practical agriculture it is excelled by no other cattle food, when proper regard is paid to the quality of the manure produced by the cattle whilst being fed on it.

Bericht über die Landwirthschaft. Versuchsstation in Möckern, 1853; *and Chem. Gaz.*, Jan. 1, 1854.

COMPOSITION *of* PEAT SOILS. By MM. GUERANGER *and* BONHOMET.

THE necessity of knowing by strict analysis the chemical composition of soils intended for the growth of ornamental plants has been long felt. In fact, it is often desirable to ascertain in what proportions and in what form, sand, clay, lime and organic matter are present. It is more especially in the soils intended for the growth of delicate plants that this knowledge becomes valuable; and the simplification of the processes for separating the various substances may be of considerable service. Having had occasion to examine four different sorts of peat-earth, in order to ascertain their composition, we have obtained very satisfactory results indeed, by merely adopting the following entirely mechanical mode of proceeding:—

1. Separation of the Water.—This operation is performed by the ordinary means, that is to say, by exposing a known weight of soil in a water-bath maintained at the boiling-point, and keeping the soil there till it undergoes no further diminution in weight. The difference between the weights before and after drying is the proportion of water.

2. Separation of the Coarse Sand.—Five grms. of peat soil were mixed with water, so as to be of the consistency of a thick soup. This was then carefully squeezed between the fingers, so as to break all the lumps. This rather tedious operation is of great importance; it might be rendered shorter by triturating the soil in a mortar; but by that method the quantity of sand could not be determined, as the

stony parts would be divided as well as the lumps. The mixture having been sufficiently squeezed, more water was added, and the whole stirred briskly. After having settled for a few seconds, the sand fell to the bottom, and the lighter matter remained in suspension. The latter portion was poured upon a fine piece of cloth, held over a vessel intended to receive the thinnest parts carried down by the water. The sand was thus washed several times, and the water used successively filtered through the cloth. When washed till the water remained clear on being agitated, the sand was carefully collected, dried by the fire, and weighed.

3. Separation of the coarser remains of Organic Matter.—The matter deposited on the cloth was shaken under a small stream of water, which carried off the more finely divided portions. This operation was continued until the water passing through the cloth remained clear. The coarser deposits remaining on the cloth were then carefully removed, dried in a water-bath, and weighed.

4. Separation of finely divided Organic Matter known by the name of Humus.—The water of these washings was allowed to rest till next day. When the deposit had collected, most of the clear liquid was poured off, and the remainder, after having been stirred, was passed through a paper filter, in order to collect the solid matters. The filter having been well drained was exposed to the air to dry; it was then dried in a water-bath. In order to guard against any loss, the dried filter and humus were weighed together, and the weight of the paper before the process commenced deducted.

5. Separation of the Fine Sand.—As the humus carries with it the finer parts of the earthy matters, it was incinerated in a platinum capsule; by this means the organic matter was dissipated and a very fine sand remained, which was weighed, and its exact nature examined.

6. Separation of Soluble Matter of Organic and Inorganic Origin.—In order to ascertain the proportion of soluble matter, it was necessary to make a separate analysis. Ten grms. of peat soil were macerated for twenty-four hours in 100 grms. of distilled water. The substance was squeezed and the liquid filtered. Fifty grms. of the limpid infusion were then taken, that quantity representing the amount of soluble matter contained in five grms. of soil. The liquid was then evaporated, and the residue burnt in a platinum capsule, which had been weighed beforehand. The weight after combustion represents that of the soluble inorganic matter. The proportion of soluble organic matter was not specially ascertained. We contented ourselves by subtracting the sum of all the products obtained, from the quantity of soil experimented on, the remainder was the soluble organic matter and loss.

7. Chemical Nature of the Earthy Matters separated by the preceding process.—The chemical nature of the earthy substances was determined by means of hydrochloric acid. In the case of the siliceous sand it has no action; but with the calcareous matter it causes a brisk effervescence. The argillaceous matter, or clay, which could only exist in the fine sand, was recognised by the property which

it possesses of remaining a long time suspended in water. This method, although empirical, appeared sufficient in an analytical process which it was our wish to render as simple as possible.

Results obtained by the above mode of proceeding.

M. Van Houtte's Camellia earth, from Ghent, in Belgium :—

Water	9·00
Siliceous sand	45·50
Finer sand, somewhat calcareous, with traces of argillaceous earth	3·10
Coarse remains of organic matter	33·50
Humus	7·00
Soluble inorganic matter	0·20
Soluble organic matter and loss	1·70
	100·00

Peat soil from Angers (Maine-et-Loire) :—

Water	10·00
Siliceous sand	42·50
Finer sand, somewhat calcareous, with traces of argillaceous earth	4·70
Coarse remains of organic matter	32·00
Humus	9·50
Soluble inorganic matter	0·60
Soluble organic matter and loss	0·70
	100·00

Peat soil from the Epinettes, Commune de Pontlieu (Sarthe) :—

Water	6·00
Siliceous sand	46·00
Finer sand, not calcareous, but with traces of argillaceous earth	6·50
Coarse remains of organic matter	23·50
Humus	10·00
Soluble inorganic matter	0·20
Organic matter of gelatinous appearance, and loss	7·80
	100·00

Peat soil from the Hunaudières, Commune de Mulsanne (Sarthe):—

Water	30·50
Siliceous sand	10·25
Finer sand, calcareous and very argillaceous	6·00
Coarse remains of organic matter	26·00
Humus	25·50
Soluble inorganic matter	0·20
Soluble organic matter	1·55
	100·00

8. **Remarks on the Products of the Analysis.**—It is to be remarked

that water was found to a certain extent in proportion to the organic matter of every kind. In the soil from the Hunaudières it reached the unusual proportion of 30 per cent. This extraordinary quantity is far from being advantageous for the cultivation of plants, because soil that contains so much water hardens and cracks by drought; and when then applied, the water passes through without thoroughly moistening it. To improve peat soils of this description, they require to be mixed with sand, and it is very difficult to make this mixture complete, on account of the hard lumps contained in this sort of soil, and which cannot be sufficiently divided to amalgamate properly. The form and proportion of siliceous sand is not a matter of indifference. This element of the soil greatly contributes to its porosity, an important property which prevents the retention of water, and allows the air to penetrate to the roots, there to carry on its beneficial action. The sand which has been designated siliceous sand, is found in Van Houtte's Camellia soil in nearly equal fragments; it appears as if it had been passed through a sieve. That in the other three sorts of soil is round, unequal, and larger in size than in the preceding. In the soil from the Hunaudières this substance is in too small a proportion.

The occurrence of fine sand is entirely accidental, and its presence in a larger proportion would be injurious by diminishing the porosity of the soil. The remains of organic matter, together with the siliceous sand, greatly contribute to this porosity; by their hygrometric properties they retain part of the water which they may receive; and further, by changing into humus, the organic remains become food fit for the support of plants. This is, in our opinion, one of the most beneficial elements of a peat soil; we find it in large quantities in Van Houtte's soil and in that of Angers; on the other hand it is in small proportion in those of the Epinettes, and of the Hunaudières.

The humus may be said to be the natural food of those plants which grow on peat soils, to which a manure containing too great a quantity of nitrogen would prove fatal. The most advantageous proportions of humus appear to be those shewn in the first three analyses, but when it reaches so high a proportion as in the soil of the Hunaudières it becomes ineligible on account of the property which it has of hardening and cracking, and when once dry of not retaining the water, as has been previously stated.

The part which the soluble inorganic substances play in vegetable nutrition not being well known, we abstain from all comment on that subject.

The soluble organic matter is most frequently nothing but humus in a very advanced state of decomposition, and so reduced as to be easily absorbed by the spongioles, and distributed to the organs by which the food of plants is elaborated.

In examining the soil from the Epinettes, we remarked with astonishment the abundance of soluble organic matter it contained.

Agricultural Gazette.

PHARMACY.

Saccharine Carbonate *of* Iron *and* Manganese.

By S. T. Speer, M.D., of Cheltenham.

The introduction of the metal manganese into the domain of therapeutics, due, I believe, in the first instance, to the Belgian physician M. Hannon, has within the last two years been followed by a careful investigation of its medicinal properties on the part of several French physicians.

From them, it would appear that this metal when given in combination with iron, is capable of rendering signal services in those diseases where iron alone has been hitherto prescribed, and at the present moment, the ferro-manganic preparations hold a prominent place in continental practice. As I am not, however, aware that they have yet undergone a trial in this country, or at least one the results of which have been made public, I venture to state shortly, what a limited experience of two years enables me to say upon this subject; inasmuch as during that period I have embraced every legitimate opportunity of prescribing the combination mentioned at the head of the present communication.

In a paper published in the "Revue Medico-Chirurgicale," for June, 1849, M. Hannon first suggested the following preparations of manganese, viz., the carbonate, the tartrate, the phosphate, the neutral malate, and the iodide. With none of these was a similar salt of iron associated.

M. Petrequin, of Lyons, however, having taken up the subject of manganic preparations, published in the "Bulletin General de Thérapeutique" for March the 15th and 30th, 1852, two papers containing the results of his experience relative to the utility of the metal in question when administered in conjunction with iron.

The preparations employed by him consisted chiefly of the iodide and lactate of iron and manganese in the form of syrup, and of the carbonate of the two metals in the form of pill.

Being desirous of trying the effects of such a combination in some of the numerous cases where iron is usually indicated, I endeavored to associate the two metals in the shape of a carbonate of the protoxide, and to retain them in this condition through the medium of sugar, as is done in the case of the saccharine carbonate of iron, very recently introduced into the London Pharmacopœia—a chalybeate perhaps superior to every other, and possessing an advantage which few practitioners will not recognise in these days of tasteless globules, namely a complete freedom from that nauseous inky flavor which the preparations of iron usually impart to the palate.

The following is the formula I suggested, and according to which the preparation in question was made :—

Saccharine Carbonate of Iron and Manganese.

Take of Finely powdered sulphate of iron .....ℨiij.ℨj
Carbonate of soda .....ℨv.
Sulphate of manganese .... ℨj. ℈j.
White sugar.....ℨiiss.

Dissolve each of the first three mentioned ingredients in a pint and a half of water, add the solutions, and mix them well; collect the precipitate on a cloth filter, and immediately wash it with cold water; squeeze out as much of the water as possible, and, without delay, triturate the pulp with the sugar, previously reduced to a fine powder. Dry it at a temperature of about 120°F.

The compound thus prepared is a powder of a reddish-brown color, and devoid of all taste, save that imparted by the sugar, with which the salts of the two metals are conjoined. The dose is five grains, gradually increased up to ℈j., three times a day; it should be given with the meals, or at least immediately after.

In the papers alluded to above, M. Petrequin asserts, that cases of anæmia, which had resisted the administration of iron alone, yielded rapidly to a combination of this metal with manganese. In confirmation of this statement, I may say, that in two cases which lately came under my notice, the one of chlorotic anæmia, with amenorrhœa; the other of uncomplicated traumatic anæmia, both of long standing, the saccharine carbonate of iron and manganese succeeded entirely, after iron alone had failed. In each of these cases, its effects upon the composition of the blood, and through this, upon the general health, were extremely rapid, thus affording a contrast to the effects of the simple preparations of iron, which, even when eventually successful, are usually slow in their operation.—*Medical Times and Gaz.*

On Syrup *of* Violets.

By M. E. Billot, Pharmaceutist at Besançon.

Syrup of violets is one of the most beautiful pharmaceutical preparations. The importance attached to its color makes it difficult of preparation. All depends on care in choosing the violets, in clearing them from stamens, removing the sepals of the calyx, and washing the petals so as to remove any of the coloring matter of the stamens that may possibly remain; all these very minute precautions make us more inclined to buy our syrup of violets, and thus run the risk of being cheated. The "*Codex*" prescribes that we should take single violets only, I see no inconvenience in taking double violets as well, the name is nothing, and whether it be called scented violet or Parma violet, it is of no consequence, provided the odor and color are the same. I am mistaken though in this, for the color of the double violet is more beautiful and its flowers are more bulky.

A double violet, or any double flower being formed by the transformation of stamens into petals, the end is the same, whether we take single or double violets. But as it may be objected that it will be more difficult to remove the few stamens and white parts which remain, I answer that the process which I am about to describe, does not require all these precautions.

1st.—Take single or double violets; operate exactly as described by M. Soubeiran, in his "*Traité de Pharmacie;*" do not forget a single stamen, infuse your violets in an earthen or porcelain vessel, and you will have a beautiful infusion of a violet blue; that depends upon the care with which you have cleaned the violets.

2nd.—Now leave some of the stamens, infuse the whole in a similar vessel to the above, you will have a dark greenish blue infusion, proceeding from the yellow color of the stamens, united to the blue color of the petals, giving a greenish coloration; put this same infusion (hot) into a tin vessel, and you will have a fine blue color (provided too many stamens have not been left). There has then been some action from the tin.—How does the tin act?—Violets, like all other flowers, contain malic or citric acid; this acid unites with the tin to form an oxide and perhaps an acid; this oxide, or this salt of tin, has the property of restoring the blue color to an ill made infusion.

Now suppose that you have not a tin vessel, but only some pieces of tin; that you have not removed the stamens of the violets (supposing that you have only single violets); infuse them in any kind of vessel but an iron one; add some of the pieces of tin that you have at your disposal, you will not have a fine blue coloration, because the acid supplied by the violets is not sufficient to produce a salt capable of restoring the blue color to the whole of the infusion, it will, therefore, be necessary to supply this want; pour then a few drops of lemon-juice or citric acid into this greenish infusion; agitate and leave it to repose; at the end of twelve hours you will have a magnificent violet blue coloration.

I go even further, leave all the stamens, sepals of the calyx, and add a few more drops of acid, you will have the same beautiful color. There is then evidently an action of oxide, or some salt of tin; I intend examining what this action is. But, it will be said, this salt of tin, these sepals, &c., may modify the constitution, injure the goodness of the syrup. I reply, that for the last two years, my syrup has had the delicious flavor and the very distinct odor of violets; no one has complained of it, indeed quite the contrary.

The following then is the method of preparation which I propose:

Take double violets, be careful to gather them in fine dry weather, shake them, to remove any dust and earth which may adhere to them; do not wash them, for a portion of the aroma will go off in the washing water, leave the stamens and sepals of the calyx; infuse in a glass vessel, adding some pieces of tin, or in a tin vessel; put in a few drops of citric acid, about ten to a quart; cover the infusion; filter after twelve hours; melt the sugar in a portion of the infusion, and when the whole is nearly boiling, add the remainder of the infusion. Cover it speedily, to prevent the volatilisation of the aroma. Preserve your syrup in bottles in which you have put a piece of tin.

This process is, I think, more simple than any hitherto given; it produces a beautiful article and with a great economy of time.

Journal de Chimie Médicale, November, 1853.

PROCEEDINGS OF SOCIETIES.

MEDICAL SOCIETY OF LONDON.

Poisoning from Impure Water.

Dr. E. SMITH directed attention to a case which had come under his observation in which the drinking of water, rendered impure by the presence of animal matter

seemed to have induced both primary and secondary mischief. The family, consisting of a man, his wife and two children, were in indifferent circumstances, and the neighborhood in which they lived was low and flat. They obtained their supply of water from an Artesian well, and about the time that the symptoms shewed themselves, it was observed to give off an offensive smell. In October, soon after this was noticed, the whole family was seized with severe diarrhœa, which remained for about a week, and then subsided. The odor of the water having, in connexion with the outbreak of the complaint, attracted their attention, the cistern was examined, and at the bottom was seen a plate covered by the remains of some herrings. The plate with the fish had been missed for about three weeks, and had probably been in the cistern for that time, and as the diarrhœa ceased upon their removal, Dr. Smith considered that it established the connexion between the malady and its cause. After the cessation of the diarrhœa, the children remained well for about a month, until the mother returned from a house in which scarlet fever had been present, and brought with her some article of clothing from the infected house. The elder child, aged three, was the same evening attacked with symptoms of severe fever, and on the following day by convulsions of a severe kind, succeeded the third day (within forty-eight hours of the first seizure) by the rash of scarlatina. Dr. Smith saw the case for the first time on the sixth day of the rash, and observed that the child's body was then covered with large patches of urticaria. At the same time the scarlet fever rash appeared in the younger child, a girl of a year old, and went on favorably until the third morning following, when laryngitis set in, and proved fatal the same night. On this day, the tenth of the eruption, the elder child was also seized with laryngitis, and died in forty-eight hours. The point of interest in these cases is, whether the animal poison had predisposed the system to the attack of scarlet fever, and to its rapidly fatal termination by laryngitis, by its peculiarly depressing influence upon the system? If so, the effect produced by the animal poison could not be that of mere debility, since the children had enjoyed an interval of good health of a month's duration. The readiness of the system to be influenced by the poison of scarlatina is evidenced by the short interval occurring between the exposure to infection, and the appearance of its peculiar symptomatic effects, and also by the violence of the fever. The occurrence of convulsions seem to justify such an inference. Since also urticaria and laryngitis are not common complications of scarlet fever, and as all three were preceded by an affection of another mucous membrane (that of the intestines), their occurrence appeared to favor the inference. Urticaria is well known to be caused by eating fish.

Dr. WINN gave the particulars of a case in which complete alopecia had occurred in a man, addicted to the use of tobacco. He had, when attending this man three years before, succeeded in renewing the growth of hair. The genital functions were much weakened at the time, but the patient had not previously suffered from depression or from nervous symptoms.

The relation of this case occasioned some discussion as to the degree of injury arising from the free use of tobacco.

Mr. CLARK had met with an instance in which distressing nervous symptoms had come on in the night after the use of tobacco, without drinking. Some degree of anæsthesia in the fingers supplied by the ulnar nerve had been experienced by the same patient.

The PRESIDENT, Dr. CHOWNE, and Dr. CRISP did not think that tobacco produced such injurious effects as had been ascribed to it. No particular nervous symptoms occurred amongst classes of men who were much in the habit of using it, such as the Chelsea and Greenwich pensioners. It was recommended by Sir Wm. Somerville as of great benefit to old men who had lost their teeth, as it stimulated the salivary glands to perform their function, and thus promoted digestion. —*Lancet.*

CHEMICAL DISCUSSION SOCIETY, DEC. 14, 1854.

Mr. J. A. SPENCER in the Chair.

Read a paper on the Iodides of Quinine of Commerce, and gave illustrations of the various reactions, which led to considerable discussion, the general opinion appearing to be, that a true iodide of quinine could hardly exist, but that the compounds so called must be hydriodates.

PULVIS FERRI.

TO THE EDITORS OF "THE CHEMIST."

"GENTLEMEN,—I have to ask the favor of your insertion of the following observations, which I have curtailed as much as possible, by confining myself to the point at issue, that is to say, the charge of substitution of one substance for another, and I leave your readers and the public to determine how far I am entitled to acquittal.

Those who have the best means of judging, will, I am sure, testify, that the practice of my laboratory is sedulously and undeviatingly to attain the utmost accuracy; those who do not, will, I hope, have waited to hear both sides before pronouncing judgment.

ON THE ALLEGED "SUBSTITUTION" FOR THE "PULVIS FERRI," OR "QUEVENNE'S IRON."

"AUDI ALTERAM PARTEM."

The object to be attained in preparing the "Pulvis Ferri," is, to procure it in the purest possible condition, and at the same time in the minutest state of division, with a view to its easy solubility in the acids of the system. This object was evidently contemplated by the process of MM. Quevenne and Micquelard; but in order to bring the reduced metal to the slate color mentioned in scientific works, the heat must be raised to such a degree of incandescence as causes it to become agglutinated in the tube; and when taken out, the metal is so hard, as to require powdering and sifting, before it can be used in medicine, thus obviating the advantages otherwise derivable from the process. Sifted iron filings might answer the purpose equally well, and the troublesome process of the Dublin Pharmacopœa might be dispensed with in favor of the preparation sold in London as Quesneville's Metallic Iron, which has the same appearance, and which is to be purchased in this city, at about 2*s* 6*d* per pound; and I would here ask how it happens, that the preparation which Mr. Morson has sent out at the price of 4*s* per ounce, up to the time of this discussion, has been labelled and called by him "Quesneville's Metallic Iron," although in copying his own letter into the journals, he has altered the term to "Quevenne." The price of Quevenne's Iron in New York is now 16*s* per pound.

Without referring, more particularly, to my own views, regarding the color of minutely divided iron, or to the necessity there appears to be for further investigation into its therapeutic properties, when in different states of aggregation, I merely observe, that in proportion to its minuteness, is its tendency to become oxidised, and that when so partially oxidised, it retains the magnetic property of iron, even though no magnetic oxide of iron be present.

In allusion to the paper "On the Substitution of Magnetic Oxide of Iron for Iron in the metallic state reduced by Hydrogen," I propose to examine the grounds of conclusions adopted by the analysts, whose reports would appear to have sanctioned a charge, which, had its author been more jealous of his own reputation and less so of mine, would not have been made; or had he been more ingenuous, would have been withdrawn before this letter could have been in circulation.

I may observe, first, that a metallic iron, which has been kept for some months, and sent about the kingdom exposed to all influences, would not, if it were in a very finely divided state, shew much disposition to continue metallic, but would exhibit a tendency to become oxide of iron in part or entirely. I will, however, take the analyses as they appear in print, since Mr. Morson states that it is not a question of comparative purity, but of the substitution of one substance for another.

The numerical results recorded by Dr. Gregory, tend only to shew, that a given quantity of oxygen was contained in the substance examined, but do not prove that it existed in the condition of magnetic oxide of iron, much less that one preparation had been substituted for another; nor does he shew whether any of the oxygen was acquired by exposure to the air, before he received it, but after the iron was reduced in my laboratory, or not.

The analyses of Dr. Stenhouse and Mr. Williamson, do not afford even numerical evidence on which to determine that it was magnetic oxide of iron.

Dr. Garrod states that it dissolves in acids, with but a trace of effervescence, due to the presence of sulphuretted hydrogen; and that its solutions are precipitated black with alkalis.

The presence of sulphuret of iron indicates that the preparation under examination did contain metallic iron, whatever oxidation might have taken place subsequently by exposure, and points out that a heat sufficient to produce metallic iron had been used in the process; and, therefore, leads to the conclusion that it had been subjected to the hydrogen process.

The precipitation by alkalis, from the solution of my preparation in sulphuric acid, is green, not black; but the solution of magnetic oxide of iron in sulphuric acid, is reddish brown, not black. There is, therefore, here, no evidence of the presence of magnetic oxide of iron; but, on the contrary, proof that chemical tests do not evidence it. And, here I offer to those distinguished chemists who have conducted the examinations, my regret that I am compelled to point out so wide a discrepancy between their results and their conclusions.

I have submitted to one of the most accomplished microscopical inquirers in the kingdom, some specimens of the "Pulvis Ferri," and I add the result of his criticism.

From ARTHUR FARRE, Esq., M.D., Cantab., F.R.S., Fellow of the Royal College of Physicians, one of the Physicians to King's College Hospital, late President of the Microscopical Society, &c., &c.

"12, Hertford Street, May Fair, Jan. 9th, 1854.

"My DEAR SIR,—I have carefully examined, with a Ross' compound microscope, the several preparations of iron which you sent me, particularly that marked "Quevenne's Iron," and also the "Magnetic Oxide," as well as a sample of nearly pure metallic iron. All of these I find to be black, and the pure metallic iron in a state of very minute subdivision, at least equal to that of the magnetic oxide. I examined also the preparation called "Quesneville's Metallic Iron," which I consider to be decidedly coarse in point of subdivision as compared with the others.

"I felt the more interest in making these examinations from having witnessed the process of making Quevenne's iron in your laboratory some months ago. I may add that in a therapeutic point of view, I should decidedly give the preference to those preparations of metallic iron which exhibit the finer state of subdivision, and, therefore, I consider the black preparations superior to those usually recommended for their slaty grey color.

"I remain very truly yours,

"ARTHUR FARRE, M.D.

"W. E. Heathfield, Esq., Princes Square, Finsbury."

I now desire to state with unhesitating confidence, and I pass my word for it, that I have not, on any occasion, sent out, or allowed to be sent out from my laboratory, any of the Pulvis Ferri or Quevenne's Iron, excepting such as has undergone the process of hydrogen gas passed over peroxide of iron at a red heat. Without calling on Mr. Morson to substantiate his charge, or to withdraw an assertion which he has so unjustly made, I would hope that in future, he will examine with a little more scientific skill, the merits of a scientific case before he pronounces an opinion; and I would point out to him, that had he examined my Quevenne's Iron himself with accuracy, he would have found that the precipitate from its solution in sulphuric acid was *green;* that from magnetic oxide of iron, treated in the same manner, *reddish brown;* and he would thus have avoided the unenviable position of imputing a fraud to another, and me the painful necessity of vindicating myself from such a charge.

I offer my apologies for thus trespassing on the pages of your journal, and am, Sir, your obedient Servant,

WM. EAMES HEATHFIELD.

Laboratory, Princes Square, Finsbury, Jan. 23rd, 1854.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On a NEW METHOD *of detecting* HYDROCYANIC ACID *in cases of* POISONING, *and on the Conversion of that Compound into Hydrosulphocyanic acid in the Body by the Hydrosulphuric acid and Sulphide of Ammonium evolved during Putrefaction.** By WILLIAM HERAPATH, F.C.S., and THORNTON J. HERAPATH.

SOME time in the course of the year 1850, one of us was called upon to examine the body of a young lady, who, it was suspected, had poisoned herself with hydrocyanic acid. The circumstances of the case were these :—The lady in question had been residing with some relatives, who were said, by common report, to have treated her somewhat harshly. Upon going into her room one morning, to call her to breakfast, the maid-servant discovered her lying at full length on the floor, cold and dead. The family physician, when called upon to give his opinion as to the cause of death, hinted his suspicions that the deceased had committed suicide by taking prussic acid. From some unexplained cause, however, the corpse was not opened, and an attempt appears to have been made by the parties with whom the young lady had been domesticated to hush up the affair, and the body was buried; but in consequence of some other friends or relatives of the deceased subsequently interfering in the matter, it was determined (at the expiration of about two months) that the body should be exhumed, and the viscera subjected to analysis. On examining the contents of the stomach and intestines, no trace of prussic acid could be discovered; but upon analysing the blood, though we could not prove the presence of free hydrocyanic acid, we were enabled to detect a small quantity of hydrosulphocyanic acid. We were hence led to suspect that the hydrocyanic acid (if such had been taken) had undergone decomposition in the body, and become converted into hydrosulphocyanic acid, or an alkaline sulphocyanide; and we therefore stated that it was our belief that the deceased had been poisoned either with prussic acid, oil of bitter almonds, laurel water, or some other compound containing hydrocyanic acid. It afterwards came out in evidence, that the young lady had been in the habit of taking oil of bitter almonds as a remedial agent, and that a bottle containing a small quantity of that poison had been found in her escritoir after her death, together with a letter containing some ambiguous expressions, apparently having reference

* Read before the "Bristol Philosophical Society," Nov. 25th, 1853, and now communicated by the Authors.

to an intention of committing suicide. Being thus pretty well satisfied in our own mind that hydrocyanic acid is converted in the body, under certain circumstances, into hydrosulphocyanic acid, and perceiving in this fact a chance of enabling toxicologists to detect this virulent but subtle poison in many cases where the processes now followed would only afford negative results, we determined to undertake a series of experiments, with the view of ascertaining whether this idea was correct. We accordingly did so, with the following results :—

A cat was poisoned with prussic acid, and the dead body was buried in the ground for twelve or fourteen days, until decomposition had commenced; it was then disinterred and dissected, and the heart removed, together with as much of the blood as could be conveniently collected; the latter, together with the washings of the heart, was then carefully evaporated to dryness in a porcelain dish. The brittle mass thus obtained was next reduced to a fine powder and boiled with some strong alcohol, and a little pure and recently ignited ivory black. The alcoholic solution resulting from this operation was afterwards filtered and evaporated to dryness; and the dried residue was acted upon with a few drops of water; this solution was then acidulated with a little pure hydrochloric acid, and tested with a dilute and almost colorless solution of a persalt of iron, when the blood-red coloration, in this case characteristic of hydrosulphocyanic acid, became manifest. Hence, it was clear either that sulphocyanogen is a constituent of healthy blood or putrid blood; or that it is really possible for hydrocyanic acid to be converted into hydrosulphocyanic acid either by the sulphur of the proteine compounds contained in the sanguineous fluid, or by the hydrosulphuric acid and sulphide of ammonium given off during their spontaneous decomposition.

In order to determine whether sulphocyanogen is contained in healthy blood, we examined several specimens of human blood by the process before described, but only succeeded in obtaining negative results; though the quantity operated on in every instance amounted to between two and three ounces.

The same results were arrived at upon analysing diseased blood and the blood of sheep, oxen, cows, and pigs. It would, therefore, appear that sulphocyanogen, even if it occurs in blood, does so in such minute quantity as to be incapable of detection, even when the experimenter operates upon large quantities of the fluid. The next question to be answered was : Is sulphocyanogen formed during the spontaneous decomposition of the blood? In order to decide this point, the blood of different animals, and several specimens of human blood were allowed to putrefy in partially closed vessels, and afterwards examined by the process already described, but no hydrosulphocyanic acid could be detected. It was, therefore, evident that the sulphocyanogen which was detected in the blood of the young lady before mentioned, and in that of the cat which had been poisoned with prussic acid, must have been formed by the action of the sulphur contained in the albuminoid compounds of the blood, or by that of the nascent sulphuretted hydrogen produced by the spontaneous decomposition of those proximate

elements on the constituents of the hydrocyanic acid. That the sulphocyanogen was not produced by the direct action of the sulphur of the proteine compounds on the hydrocyanic acid, was proved by adding some prussic acid to the white of an egg; and in another experiment to pure ov-albumen, and immediately evaporating the mixture to dryness at a low temperature, below that at which the coagulation of the albumen would have taken place. On examining the residue in the usual way, not a trace of sulphocyanogen could be discovered; but when the albuminous solution was allowed to remain for some hours before it was evaporated or the albumen was coagulated by boiling, then sulphocyanogen was invariably formed. Now, as it is well known that albumen whilst coagulating evolves hydrosulphuric acid, and that this compound, together with sulphide of ammonium, is given off during putrefaction, it is evident that the formation of the hydrosulphocyanic acid, under the circumstances we have alluded to, was occasioned by the reaction of the elements of these compounds on those of the hydrocyanic acid.

We can now understand the reason why the hydrocyanic acid in cases of poisoning so rapidly disappears from the body; and, consequently, the difficulties that have been hitherto experienced in detecting this virulent poison, though not yet entirely overcome, are greatly lessened, and thus a stumbling block has been removed from the path of the chemical toxicologist.

It still, however, remains to be proved that sulphocyanogen is *never* contained in human blood, for although our own experiments undoubtedly go far to demonstrate this, yet it must be remembered that they only do so to a certain extent, and that it is still possible that this compound may be generated in the sanguineous fluid in some diseases. Hence it is highly necessary for pathologists to determine if such be the case: this fact can be only ascertained by the combined efforts of several experimentalists; and we therefore earnestly call upon our fellow chemists to unite with us in determining this point satisfactorily by examining different specimens of diseased blood by the processes already described in this memoir. Were it established that sulphocyanogen never occurs in the blood except in those cases where hydrocyanic acid has been exhibited, it would be possible for the toxicologist to detect this poison with as much ease and certainty as he now can arsenic by the test proposed by Riensch. In order, however, that the operator should be enabled to detect minute traces of sulphocyanogen in such a fluid as the blood or the extract of flesh: we need hardly observe that it is necessary that several precautions should be taken in order to guard against the influence which the organic matters present exert over the reaction. These precautions consist, first, in insuring the perfect purity of the animal charcoal; secondly, in removing, so far as possible, the other constituents of the blood, &c., from the sulphocyanide by the action of strong alcohol; thirdly, in effecting the complete separation of the fatty matters from the alcoholic solution by evaporation and re-solution in the smallest possible quantity of water; fourthly, in employing the ferruginous test-liquid in a very

dilute condition; fifthly and lastly, in applying this test in such a manner as to render visible the slightest change of tint that is produced by the admixture of the reagent with the substance to be tested.

On the PRESENCE *of* FLUORINE *in the* FEATHERS *of* BIRDS, *and on the Existence of minute traces of that Element in* HAIR *and* WOOL.*

By THORNTON J. HERAPATH.

THE presence of fluorine as a constant constituent of some of the organs of the body was first demonstrated, I believe, by Berzelius,† who proved that this element is contained in some quantity in the enamel of the teeth, and in the bones of most, if not all, animals, as well as in ivory, and the tusks of the walrus. Berzelius' assertion, though shortly afterwards denied by Dr. G. O. Rees,‡ has since been amply verified by Erdman,§ Marchand, Heintz,‖ and many other chemists.¶ Indeed, Dr. George Wilson,** of Edinburgh, has recently published some interesting experiments on the subject, which clearly prove that fluorine is not only contained in all the organs mentioned by Berzelius, but also occurs in very minute quantity in the blood, milk, and urine. From the results of the last chemist's researches, moreover, it would appear, that this constituent is generally met with in largest proportion in those organs or organic parts which yield an ash rich in silica or the earthy phosphates; and as Gorup Besanez†† has lately proved that the feathers of birds contain a large amount of siliceous matter, the idea struck me that I should also succeed in detecting fluorine in them, and probably also in hair and wool. I therefore undertook a series of experiments with the view of ascertaining if this supposition was correct, and by taking advantage of the process described by Dr. Wilson not only succeeded in assuring myself that this element really occurs in them all to a greater or less extent, but convinced myself that birds' feathers contain it in larger quantity than any of the animal organs, with the exception of the teeth and bones. The process by means of which I made this discovery was similar in character to that described by me last year when I called your attention to the presence of minute traces of fluoride of calcium in the Bristol Hot-well Water.‡‡ It is based on the property which fluorine possesses of combining with silicon to form the peculiar gas called the

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* Read before the "Bristol Philosophical Society," and now communicated by the Author.

† "Alt. Gehlen's Journ." Bd. 3, sec. 1.

‡ "Phil. Mag." J. 15, 459.

§ "I. pr. Chem." 49, 446.

‖ "Ber. d. Ak d. Wiss. z. Berlin." Feb. 1849, s. 51.

¶ "Journal pr. Chem." Bd. 27, s. 83.

** "Edin. New Phil. Journal." Oct. 1850.

†† "Ann. d. Ch. und Pharm." Bd. 66, s. 321—342.

‡‡ "CHEMIST," No. xliii., p. 39: April, 1853.

terfluoride of silicon, and on the decomposibility (if I may be allowed to use the term) of the latter by water, by which it is converted into hydrofluosilicic acid and silica. The mode in which I operated was as follows:—

The ashes of the feathers, &c., were mixed with about an equal weight of concentrated sulphuric acid, and introduced into a small tubular glass retort; heat was then applied to the mixture by means of a spirit lamp, and the hydrochloric and fluosilicic acid gases that were evolved were conducted into a small receiver containing dilute water of ammonia. At the conclusion of the operation the contents of the receiver were evaporated to dryness; and the saline mass thus obtained was acted upon with water. This aqueous solution was then filtered, in order to separate the insoluble silica, and again evaporated in a small leaden capsule. When the whole of the water had been thus driven off a few drops of oil of vitriol were added, and the capsule covered with a thin piece of microscopic glass.

On applying heat hydrofluoric acid was evolved, which corroded the glass in those parts where it was exposed to the action of the vapors.

Operating in this way, I have been enabled to detect fluorine in the ashes of the feathers of the following birds:—

1° The Domestic Fowl.
2° The Common Duck.
3° Geese.
4° Swans.
5° Crows.
6° Sparrows.

And I have no doubt that it is contained in the feathers of every kind of bird, though some species doubtless contain it in larger quantity than others.

In order to enable you to form an opinion with regard to the proportion in which this element occurs in feathers, I may observe, that whilst one thousand grains of the ashes of human blood, of cows' blood, of milk, of flesh, and of hair and wool, contain only such minute traces of fluorine that they can with great difficulty be detected; two hundred grains of the ashes of feathers readily produce a very perceptible etching on the glass as may be perceived by the specimens which accompany this paper.

I will not now attempt to offer you any speculations with regard to the end which Nature had in view whilst distributing the fluorine so unequally in the different organs, but shall content myself with placing the facts I have alluded to on record, and leaving the explanation of them to some more ingenious chemist, or until some future opportunity when the researches of physiologists shall have made us better acquainted with the secrets of animal life.

CHEMICAL TABLES. COMPILED BY THORNTON J. HERAPATH.

No. I.—DENSITIES OR SPECIFIC GRAVITIES.

(Continued from page 274.)

When an * is attached to a name, it must be understood that the *preceding* numbers have been merely copied from some standard work, and are not the result of the Author's experiments.

Substances.	Density at 60° F.	Authority.
Lead-ore yellow	5·706	Hatchett.
„ „	6·760	Haidinger.
„ red	6·000	Thomson.
Lederitite	2·169	Jackson.
Leelite	2·606	Clarke.
Lehuntite	1·953	Thomson.
Lenzinite	1·8 to 2·1	John.
Lepidolite	2·816 to 2·854	Cresy.*
Lepidomelane	3·000	Soltmann.
Leucite	2·490	Thomson.
Leucol	1·081	Hofmann.
Leucophane	2·974	Herrman.
Leucopyrite	6·127	Thomson
Levyne	2·198	Connell.
Liebethenite	3·6 to 3·8	Thomson.
Liebenerite	2·814	Marignac.
Liebigite		
Ligurite	3·490	Viviani.
Lime	2·3 (?)	Brande.*
„	3·08	Dumas.
„	3·16	Karsten.
„	3·18	Filhol.
„ hydrate	2·078	„
„ acetate	1·005 (?)	Gray.*
„ baryto-fluate	3·750	Smithson.
„ nitrate, anhyd.	2·2700	Filhol.
„ nitrate, crys.	1·620	Gray.
„ „	1·780	Filhol.
„ oxalate	1·833	Brooke.
„ bone-phosphate	3·000	Gray.
„ tungstate	5·959	Thomson.
Lime, tungstate	6·076	Haidinger.
Limestone	3·000	Moseley.
„ compact	1·8364 to 2·720	Cresy.*
„ green	3·182	„
„ arenaceous	2·742	„
Linarite	5·3 to 5·4	Phillips.
Lithomarge	2·457	Thomson.
Lobeline	0·950	
Lomonite	2·300	Haüy.
Lonchidite	4·925 to 5·001	Breithaupt.
Lymph	1·037	Marchand & Colberg.
„	1·017	Geiger.
Magnesia	3·070	Richter.
„	3·200	Karsten.
„ acetate of,	1·378	Gray.*
„ borate of,	2·566	„
„ carbonate of,	2·612	Moseley.*
„ ferro-carbonate of,	3·001 to 3·112	Thomson.*
„ nitrate of,	1·460	Gray.*
„ phosphate of,	1·550	„
„ racemate of,	1·980	Thomson.
„ sulphate of,	1·683	Playfair and Joule.
„ „	1·751	Filhol.
„ „ anhyd.	2·628	„
„ „ „	2·606	Karsten.
„ „ c. solu.	1·232	Cresy.*
„ sulphite of,	1·380	Gray.*
„ tartrate of,	1·960	Thomson.
Magnesite or Baudisserite	2·200	Cresy.
„ „	2·808	Breithaupt.

Substances.	Density at 60° F.	Authority.
Magnesite or Baudisserite	2·915	Klaproth.
" "	2·950	Stromeyer.
Magnesium	1·562	Playfair & Joule.
" chloride of	1·558	Filhol.
" "	1·600	Gray.*
Malachite	3·994	Moseley.*
"	4·008	Haidinger.
Mangan-blende	3·95 to 4·014	Thomson.
Manganese, diarsenide	5·550	Kane.
" protoxide	5·380 at 39°·1	Playfair & Joule.
" sesquioxide	4·568 to 4·619	"
" "	4·620	Leonhard.
" peroxide	4·8 to 4·9	Brande.*
" red-oxide	4·325	Playfair & Joule.
" racemate	1·960	Thomson.
" sulphate	2·877	Gray.*
Manganese-glance	3·95 to 4·01	Leonhard.
" "	4·014	Mohs.
Manganese ore, grey	3·819	Thomson.*
" "	4·917	"
Manganiferous zinc-spar	3·98 to 4·20	Monheim.
Manganite	4·312 to 4·328	Turner.
Marble	2·700	Ure.*
"	2·690 to 2·727	"
Margarite	3·0 to 3·1	Dumenil.
Marl	1·6 to 2·87	Thomson.*
"	2·9444	Cresy.*
Meconium	1·148	Davy.
Meerschaum	2·6 to 3·4	Ure.*
Meionite	3·100	Cresy.*
"	2·612 to 2·650	Thomson.*
Melam	1·640	Prout.
Melanchlore	3·380	Fuchs.
Melanite	3·157 to 3·730	Thomson.
"	3·800	Moseley.*
Melanchroite	5·750	Thomson.*
Melilite	2·950	Damours.

Substances.	Density at 60° F.	Authority.
Mellite	1·55 to 1·597	Klaproth.
Menachanite	4·427	Thomson.
"	3·160 (?)	Cresy.*
Mendipite	7·000 to 7·077	Berzelius.
Mercaptan	0·837	Liebig.
"	0·842	Zeise.
" seleniuretted	+1	Siemens.
Mercury, native	13·568	Thomson.
" proto-chloride	7·178 at 39°·1	Playfair & Joule.
" "	7·176	Hasenfratz.
" perchloride	6·200	"
" "	6·500	Graham.*
" potassio-chloride	3·735	Playfair & Joule.
" sodio-chloride	3·011	"
" biniodide	6·250	Filhol.
" protoxide	10·690	W. Herapath.
" peroxide	11·000	Boullay.
" "	11·074	Berzelius.
" "	11·085	W. Herapath.
" "	11·196	Karsten.
" "	11·344	Playfair & Joule.
" red oxide	11·136	"
" proto-nitrate	4·785	"
" per nitrate	5·967	"
" basic nitrate	4·242	"
" sub-sulphate	6·444	Gray.*
" "	8·319	Playfair & Joule.
Mesite	0·805	Reichenbach.
Mesitine-spar	3·360	Stromeyer.
Mesolite	2·218 to 2·225	Thomson.
"	2·333	Freysmuth.
Methylene	0·800	Thomson.
Methylic ether, acetic	0·919 at 70°·5	Dumas & Peligot.
" benzoic	1·100	"
" nitric	1·182 at 71°	"
" sulphuric	1·324	"
" valeric	0·8806	Löwig.

Substances.	Density at 60° F.	Authority.
Methyl, bromide of,	1·663 at 55°·5	Pierre.
„ fluoride	1·186	Dumas and Peligot
„ iodide	2·237 at 71°	„
„ sulphide	0·845	„
„ bisulphide	1·046	Cahours.
Methylic-mercaptan	—1	„
Metals:—Aluminium	2·5 to 2·67	Wöhler.
Antimony	6·610	Breithaupt.
„	6·646	Mohs.
„	6·700	Karsten.
„	6·712	Brisson.
„	6·733	Boeckmann.
„	6·852	Muschenbroeck.
„	6·860	Bergmann.
Arsenic	5·628	Karsten.
„	5·672	W. Herapath.
„	5·7 to 5·96	Guibourt.
„	5·763	Brisson.
„	5·766	Mohs.
„	5·884	Turner.
Barium	4·000	Davy.
Bismuth	9·650	Karsten.
„	9·670	Muschenbroeck.
„	9·800	Brisson.
„	9·832	W. Herapath.
„	9·882	Thenard.
Cadmium	8·604	Stromeyer.
„	8·635	Karsten.
„	8·659	W. Herapath.
„	8·670	Children.
Chromium	5·100	Thomson.
„	5·900	Richter.
Cobalt	8·485	Brunner.
„	8·500	Mitscherlich.
„	8·513	Berzelius.
„	8·538	Haüy.
„	8·558	T. H. Henry.

Substances.	Density at 60° F.	Authority.
Metals—*continued.*		
Columbium or Tantalium	5·600	Moseley.*
„	5·918 to 6·208	Cresy.*
Copper	8·721	Karsten.
„	8·830	Berzelius.
„	8·884 to 8·941	Playfair & Joule.
„	8·895	Hatchett.
„	8·900	W. Herapath.
Gold	19·299	G. Rose.
	19·258 to 19·361	Brisson.
„ standard; coin	17·158	Brande.*
„ „	17·157	Cresy.*
„ „	17·724 (!)	Thomson.
„ standard; trinkets	15·709 to 15·775	Cresy.*
Iridium	18·680	Children.
„	19·500	Mohs.
„	21·78 to 21·83	Hare.
„	22·11 to 23·55	Breithaupt.
Iron:—Cast	7·264	Templeton.
	7·2 to 7·6	Thomson.
„ bar	7·700	Templeton.
„ „	7·6 to 7·8	Kirwan.
„ „	7·788	Brisson.
„ „	7·790	Karsten.
Lead	11·2070	Broeckmann.
„	11·3520	W. Herapath.
„	11·275 to 11·298	Playfair and Joule.
„	11·330	Kupffer.
„	11·3883	Morveau.
„	11·445	Muschenbroech.
Magnesium	2·23 to 2·25	Playfair and Joule.
Manganese	6·861 to 7·100	Bergmann.
	8·030	Backmann.
	8·013	John.
Mercury	13·588	Kupffer.
„	13·619 at 32°	Cresy.*
„	13·580 at 60°	„

Substances.	Density at 60° F.	Authority.
Metals—*continued.*		
Mercury	13·375 at 212°	Cresy.*
„ frozen	14·000	Kupffer and Cavallo.
Molybdenum	7·500	Hielm.
„	8·6156 to 8·636	Bucholz.
Nickel	8·279	Richter.
„	8·380	Tupputi.
„	8·402	Tourte.
„	8·477	Baumgartner.
„	8·637	Brunner.
Osmium	10·000	Playfair & Joule.
Palladium	10·923	Breithaupt.
„	11·800	Moseley.*
Platinum	19·500	Templeton.*
„	19·700	Hare.
„	20·970	Brisson.
„ bar.	21·230 to 21·310	Hare.
„ „	23·500	Claus.
Potassium	0·865	Gay-Lussac and Thenard.
Rhodium	11·000	Wollaston.
	11·200	Claus.
Ruthenium	8·60	„
Silver	10·474	Brisson.
„ bar.	10·566	G. Rose.
„	10·522	Playfair & Joule.
„ standard coin	10·300	Brande.*
	10·312	Thomson.*
Sodium	0·972	Gay-Lussac and Thenard.
„	0·935	Davy.
Strontium	4·0 to 5·0	Gehler.
Tellurium	6·115	Klaproth.
„	6·13 to 6·258	Berzelius.
„	6·343	Müller.
Tin	7·17 to 7·291	Cresy.*

Substances.	Density at 60 °F.	Authority.
Metals—*continued.*		
Tin	7·248	Playfair & Joule.
„	7·285	W. Herapath.
„	7·291	Brisson, &c.
Titanium	5·280	Karsten.
„	5·300	Wollaston.
Tungsten	17·220	Allan and Aiken.
„	17·400	Bucholz.
„	17·600	D'Elhuyart.
Uranium	8·425	Playfair & Joule.
Zinc	6·861	Brisson.
„	6·862	Berzelius.
„	6·843	Playfair & Joule.
„ bar.	7·190	Brisson.
Methylal	0·855	Graham.*
Mesiten	0·808	Weidemann.
Miargyrite	5·234	H. Rose.
Mica	2·824	Thomson.
„	2·934	Moseley.*
„	2·949	Haidinger.
„	3·080	Turner.
„	3·127	Delesse.
Microcosmic salt	1·500	Gray.*
Middletonite	1·600	Johnston.
Milk, asses'	1·030 to 1.035	Peligot.
„ cows'	1·0324	Brisson.
„ „ *(average quality)*	1·0330 to 1·064	Lassaigne.
„ ewes'	1·035 to 1·041	Brisson.
„ goats'	1·0341	„
„ mares'	1·0346 to 1·045	„
„ women's	1·023	„
	1·028 to 1·034	Griffith.
Molybdenum, sulphide	4·138 to 4·569	Graham.*
„ „	4·7385	Brisson.
Molybdic acid	3·460	Thomson.
„	3·490	Berzelius.
Molybdic oxide	5·666	Bucholz.

Substances.	Density at 60° F.	Authority.
Monazite	4·880 to 4·992	Breithaupt.
Monradite	3·267	Erdmann.
Mosandrite	2·93 to 2·98	"
Mountain leather	1·334	Thomson.
Moringic acid	0·908	Walter.
Mortar	1·384 to 1·993	Rondelet.
Mucamide	1·589	Malaguti.
Mucic acid	+1	
Mullicite	1·787	Thomson.*
Murchisonite	2·509	Kane.
Muromontite	4·263 to 4·265	Kerndt.
Must of grapes	1·065 to 1·085	Crasso.
Myrrh	1·12 to 1·18	Ruickholdt.
Mysorite	2·620	Thomson.
NACRITE	2·788 to 2·793	"
Naphtha, coal	0·9 to 0·95	Mansfield.
" rectified coal	0·82 to 0·86	Brande.*
" "	0·817	Reichenbach.
" "	0·857	Ure.
" Persian	0·753	Christison.
" "	0·765	Gregory.
Naphthaline	1·048	Ure.*
Natrolite	2·723	Ekeberg.
"	2·139 to 2·2303	Thomson.
Nemalite	2·353	"
	2·440	Nuttall.
Néoctèse	3·180	Damour.
epheline	2·560	Scheerer.
"	3·270	Roget and Dumas.
Nephrite	2·595	Thomson.*
Neurolite	2·476	"
Newkirkite	3·824	Muir.
Nickel, antimonial	7·541	Breithaupt.
" diarsenide	7·655	Thomson.*
" carbonate	2·570 to 2·693	Sillimann.
" oxide	5·597 at 39°·1	Playfair and Joule.
Nickel, oxide	5·748	Genth.
" peroxide	4·814	Playfair & Joule
" "	4·846	W. Herapath.
" sulphate	2·037	Kopp.
" sulphide	5·278	Playfair & Joule.
" disulphide	6·050	"
" sulph-antimonide	6·097	G. Rose.
Nickel-glance	6·129	Thomson.
Nicotine	1·048	H. & B. Chalard.
"	1·033 at 39°	Barral.
"	1·027 at 60°	"
"	0·9424 at 215°	"
Nigrine	4·445	Klaproth.
"	3·700 to 4·673	Cresy.*
Niobic acid, amorph.	5·2545 to 5·2620	Rose.
" crys.	4·664 to 4·7633	"
Nitranisole	+1	Cahours.
Nitre	1·933	Thomson.*
Nitric acid, red and fuming, or monohydrate	1·500 to 1·522	A. Smith.
" bihyd.	1·485	Ure.
" "	1·486	A. Smith.
" 4-hyd.	1·424	"
" 7-hyd.	1·335	Ure.
Nitrous, or hyponitric acid	1·451 at 0°	Graham.*
Nitrogen, iodide of	+1	
" 4-chloride of	1·653	Dulong (?)
Nussierite	5·0415	Barruel.
OBSIDIAN	2·370	Moseley.*
"	2·363 to 2·373	Thomson.*
Oerstedite	3·629	Forschammer.
Œnanthole	0·8271	Bussy.
Oils—almonds, fixed	0·915	Saussure.
" "	0·918	Ure,* &c.
" "	0·932 (?)	Moseley.*
" " vol.	1·043 (?)	Graham.*

Substances.	Density at 60° F.	Authority.
Oils—*continued.*		
Anise	0·986	Saussure.
Bergamot	0·862	Ohme.
„	0·869	Soubeiran.
„	0·888	Thomson.*
Castor	0·9611	Reichenbach.
„	0·9650	Saussure.
Cinnamon	1·035	Ure,* &c.
„	1·043	Moseley.*
Cloves of commerce	1·034	Lewis.
„ pure	1·055 to 1·061	Bonastre.
Cocoa-nuts	0·910	Brande.*
Cod-livers	0·923 to 0·929	Moseley.*
Colza	0·9136	Thomson.*
Lavender	0·877 to 0·905	Thomson.*
„	0·894	Moseley.*
Lemons	0·847	Saussure.
Linseed	0·928 to 0·932	Brande.*
	0·9347	Ure.*
Nut-oil	0·926	„
Olives	0·9176	Heidenbacher.
„	0·917	Saussure.
Peppermint	0·920	Thomson.
	0·907 to 0·9083	Kane.
Rape	0·9128 to 0·9167	Thomson.*
	0·9136	Heidenreisch.
Rosemary	0·889	Berzelius.
	0·8854 to 0·8875	Kane. Saussure.
Roses	0·867 to 0·872	Chardin.
Turpentine	0·87 to 0·872	Brande.*
„ pure	0·863	Pereira.
„ com.	0·87 to 0·884	„
Whale, or train oil	0·923	Heidenreisch.
Okenite	2·280	Von Kobell.
Oleïc acid	0·898 at 65°	Brande.
Oleïne	0·9003	Heidenreisch.
Oligoclase	2·6680	Berzelius.
Oligoclase	2·66 to 2·68	Kerudt.
Olivine	3·286 to 3·3445	Stromeyer.
„ meteoric	3·3404 to 3·3497	„
Olivenite	4·166	Richardson.
„	4·2809	Bournon.
„	4·2 to 4·6	Phillips.
Opal, precious	2·015 to 2·210	Klaproth.
„ „	2·114	Moseley.*
„ common	1·957 to 2·114	„
„ ligniform	2·600	Cresy.*
„ semi	2·540	„
Opium	1·336	Moseley.*
Orthite	3·288	Thomson.*
„	3·4 to 3·6	Hermann.
Orthose	2·615	Saussure.
Osmio iridium	19·835	Boyé.
„	19·385 to 19·471	G. Rose.
„	20·938	Hare.
„	21·118	Rose.
Oxalhydric acid	1·418 at 68°	Thomson.*
Oxalate of iron	2·489	Thomson.*
Oxalic acid	1·6413 at 39°·1	Playfair & Joule.
„	1·62	Gray.*
Oxygenated water	1·45	Brande.*
Oyster shells	2·092	Moseley.*
Ozocerite	0·885	Johnston.
Palladium, native	11·800	Wollaston.
	12·140	Lowry.
Paraffine	0·870	Graham.*
„	0·890	Lewy.
Parisite	4·35	Spada.
Paulite	3·385	Klaproth.
Pearls, Oriental	2·683	Brewster.
„	2·750	Moseley.*
Pearl-stone	2·340	Cresy.*
Pest	0·8495	Vaux.
„ hard	1·3290	Tredgold.

On a New Test *for* Formic Acid. By Mr. Berkeley Hill.

(Communicated by Mr. Thornton Herapath.)

The test I am about to propose is founded on a reaction that was first pointed out, I believe, by Döbreiner.* That chemist has shown that formiate of ammonia, when heated to about 356° F., is resolved into hydrocyanic acid and water; $NH^4O, C^2HO^3 = C^2NH + 4Aq$. Liebig† has since proved that when hydrocyanic acid is added to a solution of sulphide of ammonium and caustic ammonia, and the mixture is heated with the addition of flowers of sulphur, or when a mixture of prussic acid and ammonia is gently heated with pentasulphide of ammonium, the hydrocyanic acid is soon converted into sulphocyanide of ammonium. The mode in which I apply these facts to the detection of formic acid may be thus described:—The mixture supposed to contain formic acid is mixed with a little sulphuric acid, and carefully distilled in a sand-bath. The acid distillate is then supersaturated with ammonia and evaporated to dryness at a low temperature, and the saline mass thus obtained mixed with a little pure quartzose sand. It is next introduced into a retort and heated to about 400° F., the products of the destructive distillation being conveyed into a solution of the bisulphide of ammonium, which, at the conclusion of the operation, is evaporated to dryness. On acting on the dried residue with a drop or two of water, and separating the precipitated sulphur by means of a small piece of bibulous paper, a clear solution is obtained, which—if formic acid was present in the matters analysed—will yield a more or less distinct blood-red coloration on being tested with a per-salt of iron, in consequence of the conversion of the formic acid into sulphocyanide of ammonium.

TRANSLATIONS AND ABSTRACTS.

INORGANIC CHEMISTRY.

On Fluoride *of* Antimony. By M. Flückiger.

The author has prepared fluoride of antimony by the method of Berzelius, by dissolving oxide of antimony in hydrofluoric acid, and evaporating the solution slowly between 158° and 194° F. He thus obtained rhomboidal octohedra, or rhomboidal tables; by a more rapid evaporation prisms are obtained, and when it is evaporated very rapidly in the presence of an excess of acid, small bracteæ are formed. These crystals contain 68·38 per cent. of antimony, a result which leads to the formula SbF^3. They do not fume in the air, but they attract moisture and decompose. They dissolve readily in water rendering it turbid; but when the solution is evaporated it leaves a white powder, at least if it did not contain an excess of acid. Fluoride of antimony volatilises without decomposition when heated in the air; when distilled in a covered vessel, it leaves a residue.

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* *See* Pelouze, "Ann. Ch. et Phys." xlviii. 399.

† "Chem. Gaz." 1847, p. 143.

The author has prepared some double fluorides, combinations of fluoride of antimony and alkaline fluorides. He obtains the double fluoride of antimony and potassium $2\,KF + Sb\,F^6$, by dissolving in an excess of hydrofluoric acid, a mixture of 153 parts of oxide of antimony, and 200 parts of carbonate of potassa. This double fluoride crystallises in small styptic bracteæ, soluble at 55° F. in 9 parts of water, and disengaging hydrofluoric acid when exposed to moist air. Another double fluoride is formed by dissolving, in hydrofluoric acid, equal parts of carbonate of potassa and oxide of antimony. This combination crystallises in rhomboidal octohedra and contains $KF + Sb\,F^6$.

M. Flückiger has, besides, prepared the double fluoride of antimony and sodium $2\,NaF + Sb\,F^6$, crystallisable in rhomboidal prisms, the double fluoride of antimony and lithium $2\,LiF + Sb\,F^6$, and the double fluoride of antimony and ammonium $2\,NH^4F + Sb\,F^6$.

Poggendorff's Annalen.

Action *of* Ammonia *on the* Sulphomylate *of* Lime.

By M. Marcellin Berthelot.

If sulphomylate of lime be heated to 482° F. with an alcoholic solution of ammonia, decomposition takes place with formation of a salt of amylamine.

This results from the following facts:—The matters, after being subjected to the action of this heat in a closed vessel, were distilled with potassa, and the vapors condensed in a solution of hydrochloric acid. This solution was evaporated to dryness, treated cold with absolute alcohol, again evaporated, redissolved by alcohol, and finally chloride of platinum added while hot. The liquor, when cooled, deposited crystals which, when analysed, gave the following numbers:—

C	19·4
H	5·2
Pt	33·9

The formula $C^{10}H^{13}N$, HCl, $PtCl^2$ requires,—

C	20·5
H	4·8
Pt	33·6

This decomposition is represented by the following formula:—

$$C^{10}H^{11}O, CaO, S^2O^6 + NH^3 = C^{10}H^{13}N, HO, SO^3 + CaO, SO^3.$$

It agrees completely with the assimilation established by M. Wurtz between the ethylic alkalis and the ethers. In the present case, this analogy is the more complete because amylamine, hitherto given only by special ethers (cyanic, hydrobromic, and hydriodic), is produced in the decomposition even of the sulphamylates; its production is consequently quite parallel with that of the ethers, according to the general process indicated by M. Pelouze:—

$$\left\{ \begin{array}{l} = (C^{10}H^{11})O, CaO, S^2O^6 + (Cy)K \\ = (C^{10}H^{11})(Cy) + KO, SO^3 + CaO, SO^3 \end{array} \right\}$$
$$\left\{ \begin{array}{l} = (C^{10}H^{11})O, CaO, S^2O^6 + (NH^3)H \\ = (C^{10}H^{11})NH^3, HO, SO^3 + CaO, SO^3 \end{array} \right\}$$

The sulphovinate of baryta, heated to 482° F. with ammonia, furnishes a deliquescent alkaline hydrochlorate, which is soluble in absolute alcohol; the platinum salt of which, treated with potassa, disengages an alkaline inflammable gas. This is, without doubt, ethylamine.

Annales de Chimie et de Physique, December, 1853.

On the Action *of* Alkalies *on* Rocks. By M. Delesse.

In studying the action of alkalies on rocks, I treated the powder of the specimen under examination in a solution containing five times its weight of potassa; I then sought what substances existed in the new solution.

Since many rocks contain water, I dried and weighed the residue from the potassa solution, I then heated it to redness and weighed again: I thus obtained for the different rocks comparable results, as shewn in the following table:—

Description of the rock.	1	Trachyte, brownish red, with gray globules and black micæ, from Hungary.	Trachyte molaire, with a little vitreous orthose and some micæ, from Hungary.	Retinite, blackish-brown, from Planita.	Retinite, globular, deep red, from Meissen.	Retinite, black with resinous lustre, from Sardinia.	Perlite, grayish-white, nacreous, from the "Cap de Gatis."	Obsidian, dark black, from Lipari.	Eurite, argillaceous brick-red, from Saxony.	Palagonite tuff, with a brown-yellowish paste, from Iceland.	Melaphyre, with dark green paste and greenish-white labrodorite, from Belfahy.	Basalt, black paste and crystal of augite, from Bohemia.	Lava, black, of the flow of Oct. 1819, from Vesuvius.	Porphyry, quartziferous, reddish-gray cellular paste, and much hyaline quartz, from Saxony.
Silica dissolved by the potassa	2	36·00	17·06	19·40	12·23	9·50	19·55	18·39	11·45	7·65	8·50	7·68	4·50	5·35
Alumina dissolved by the potassa . .	3	traces	2·39	3·75	1·16	1·25	1·85	3·78	1·55	2·10	2·20	2·85	2·60	traces
Total loss of rock after calcination of the residue . .	4	37·85	27·27	30·15	17·8	16·55	26·85	24·44	17·20	18·60	18·41	15·85	8·50	5·80

Some general conclusions can be drawn from my researches.

When an alkali attacks a rock, it dissolves from it not only silica, but also alumina, water, potash, and soda. Besides a little lime, magnesia and traces of oxide of iron are taken up by the alkaline liquor.

The quantity of silica dissolved is always greater than that of all the other substances.

Granite may be said to be unaffected by boiling in a solution of alkali; quartziferous porphyry only loses a few hundredths.

Lava, basalt, and melaphyre lose not more than 20 per 100.

Trachyte, retinite, perlite, and obsidian are the most attackable; within my experience their loss did not exceed 40 per 100.

A rock containing water is much less easily affected by the alkalies after it has been calcined. Thus, in the perlite from the Cape of Gatis, the action of the alkali before and after calcining varied in the ratio of $2\frac{1}{2}$ to 1.

On the other hand, a rock is much more easily affected when in a state of decomposition. Thus, argillaceous eurites and kaolins, which are merely decomposed granitic rocks, undergo in the alkali losses much greater than those of granitic rocks.

Other conditions being the same, the action of alkalies on rocks increases with the amount of silica, with the absence of crystalline structure and of hyaline quartz. On this account, vitreous rocks which contain little or no quartz, as retinite, obsidian, trachyte, are very strongly affected by alkalies.

When instead of alkalies we use alkaline carbonates, some rocks, especially the vitreous, are still acted upon, but much less energetically than by the caustic alkalies.

The readiness with which alkalies, and even alkaline carbonates, act upon rocks, shews how cautiously they should be used for the separation of the free and directly soluble silica that may be mechanically mixed in a rock, as particularly in the kaolins and the finer clays.

I may remark, that, for instance, in obsidian the silica dissolved is not free, but in the state of soluble silicate; moreover, in retinite the silica is not in the state of opal, as is supposed, but in that of soluble hydrosilicate. Generally in all vitreous or porphyritic rocks, hydrous or anhydrous, the silica is held in a combination, not definite, forming the paste of these rocks, and which is attacked by the alkali.

The water penetrating rocks by infiltration always contains, even near the surface of the earth, small quantities of alkaline salts; it is easy, then, to conceive that these salts should aid in decomposing rocks and in producing pseudomorphisms. But at a greater depth water becomes largely impregnated with alkaline salts, the temperature and pressure increasing rapidly; it then influences greatly the rocks with which it is in contact. This is the case with the water of mineral springs, of the geysers, of mud volcanoes, and in general of volcanic regions. Consequently, the action of the alkalies and of the alkaline salts plays an important part, not only in the formation of pseudomorphs, but also in the chemical reactions which take place in the interior of our planet.—*Philosophical Magazine*, February, 1854.

METALLURGY.

On the DETECTION *of* GOLD *in* LEAD *and its* COMPOUNDS. By JOHN PERCY, M.D., F.R.S., *Lecturer on Metallurgy at the School of Mines, Jermyn-street; and* RICHARD SMITH, *Assistant in the Metallurgical Laboratory.*

IN the "Philosophical Magazine" for April, 1853, it was stated that gold had been detected in numerous samples of lead and its compounds

met with in commerce, and that the experiments upon which the statement was founded should appear on a future day. Those experiments are now given. The investigation is still in progress, and will embrace the examination of a variety of ores.

All the experiments have been made by Mr. Smith, and the visible specimen of burnished gold obtained in each experiment has been preserved in a hermetically sealed tube.

The diffusion of gold, as proved by the experiments in question, is at least curious, and may probably suggest chemical and geological considerations of special interest at the present time. The fact of gold existing in certain soluble compounds of lead is remarkable; and it may be that sea-water will one day be found to contain the precious metal, though in infinitesimally small proportion.

The quantity of gold obtained in each experiment was far too minute to be capable of estimation by the most delicate balance. In order, however, to give an approximate and comparative notion of the quantities respectively extracted, the following scale of comparison will be adopted in the description,—trace, minute trace, very minute trace, and just perceptible trace.

At present it has not been possible to prepare lead free from a trace of gold.

Method of Examination.—The lead was separated from the compounds of lead examined by a process of reduction described under each experiment; a known weight of the lead was then submitted to the process of cupellation in the usual way, and the button of silver left after cupellation was carefully detached from the cupel, flattened under a hammer to free it from adherent matter, transferred to a small watch-glass, treated first with very dilute nitric acid at a very gentle heat until all action had ceased, and then with strong nitric acid. The black residual matter was carefully washed with distilled water by decantation, transferred to a small piece of writing-paper, dried at a gentle heat, rubbed with a steel burnisher, gummed to the paper, and preserved in a small glass tube hermetically sealed. When necessary, this process was performed under a microscope.

Lead.—*Exp.* 1. A specimen of lead pipe from the Great Exhibition of 1851. Cupelled 2000 grs., the button obtained, treated with nitric acid, left a *trace* of gold.

Exp. 2. A specimen of "Pattinson's crystallised lead." Cupelled 2000 grs. treated the remaining button with nitric acid, and obtained a *minute trace* of gold.

Exp. 3. Specimen of lead from the Nenthead Works near Alston. Cupelled 2000 grs., the button obtained, parted with nitric acid, left a *very minute trace* of gold.

Exp. 4. Specimen of lead from Tuscany, from the Great Exhibition of 1851, hard and brittle, with a close-grained crystalline fracture, probably due to the presence of antimony. Cupelled 2000 grs., the residual button, parted with nitric acid, left a *very minute trace* of gold.

Exp. 5. Specimen of lead from the Austrian collection in the Great

Exhibition of 1851, labelled "23, Bleiberger Probierblei." Cupelled 2000 grs., treated the remaining button with nitric acid, and obtained a *minute trace* of gold.

Exp. 6. Specimen from ditto, labelled "25, Przibramer Weichblei." 2000 grs. cupelled, and the residual button treated with nitric acid, left a trace of black matter; but the color of gold could not be *distinctly* obtained by burnishing, probably owing to a small amount of silver left undissolved.

Exp. 7. Repeated Exp. 6 upon 2000 grs., and obtained a *very minute trace* of gold.

Exp. 8. Specimens from ditto, labelled "26, Przibramer Hartblei." 3340 grs. scorified to a small bulk and then cupelled, left a button weighing ·1 gr.; treated with nitric acid, left a *minute trace* of gold.

Red Lead.—*Exp.* 9. Specimen made from Snail-beach lead, Shropshire. Mixed 2 lbs. troy of red lead with excess of finely-powdered charcoal, heated the mixture in a Cornish crucible, and poured out the reduced lead into an ingot mould. Of this lead, cupelled 2000 grs., parted the residual button with nitric acid, and obtained a *trace* of gold.

Exp. 10. Specimen made from Derbyshire lead. Weighed out 5000 grs. of red lead and 300 grs. of powdered charcoal, heated the mixture in a Cornish crucible, the reduced lead weighed 3840 grs. Of this lead, cupelled 2000 grs., the button obtained weighed about ·1 of a grain; parted with nitric acid, left a *trace* of gold.

Litharge.—*Exp.* 11. Sample purchased at Mr. C. Button's, Holborn Bars (in the form of small, thin scales). Mixed 6000 grs. with 300 grs. of powdered charcoal, reduced the mixture in a Cornish crucible, lead obtained weighed 5460 grs. Of this lead, cupelled 2000 grs., obtained a small button of silver; parted with nitric acid, left a *very minute trace* of gold.

Exp. 12. Sample bought at Mr. G. James's, 72, Wardour Street (in powder). 6000 grs. reduced by heating with 300 grs. of powdered charcoal, gave of lead 4910 grs. Of the lead thus obtained, cupelled 2000 grs., parted the small residual button of silver with nitric acid, and obtained a *very minute trace* of gold.

Exp. 13. Sample purchased at Mr. Caplin's, 42, Great Pulteney-street (in powder and small lumps). 6000 grs. mixed with 300 grs. of powdered charcoal and reduced in a Cornish crucible, gave of lead 5310 grs. Of this lead, cupelled 2000 grs., treated the small remaining button of silver with nitric acid, and obtained a *minute trace* of gold.

Exp. 14. Sample brought from Birmingham (in small, thin scales). Reduced 2880 grs. with 150 grs. of powdered charcoal in a Cornish crucible; lead obtained weighed 2270 grs. Of this lead, cupelled 2000 grs.; the small button of silver obtained, parted with nitric acid, left a *minute trace* of gold.

White Lead.—*Exp.* 15. Specimen purchased at Mr. Button's, Holborn Bars, sent as "pure carbonate of lead," and said to have been

prepared by precipitation from a solution of the nitrate of lead by carbonate of soda. 6000 grs. mixed with 200 grs. of powdered charcoal and heated in a Cornish crucible, gave of reduced lead 4190 grs. Of this lead, cupelled 2000 grs., left a very minute button of silver; parted by nitric acid, a just perceptible trace of black matter remained undissolved, but no *distinct* metallic lustre could be obtained by burnishing.

Exp. 16. Repeated Exp. 15. 2 lbs. troy (11,520 grs.) reduced by heating in a Cornish crucible with 400 grs. of charcoal, gave of lead 8550 grs. Of this lead, 4000 grs. cupelled left a very small button of silver; parted by nitric acid, left a *just perceptible trace* of gold.

Exp. 17. Sample bought at Mr. G. James's, 72, Wardour-street. 6000 grs. mixed intimately with 200 grs. of powdered charcoal and heated in a Cornish crucible, gave of lead 4340 grs. Of this lead, cupelled 2000 grs., parted the small residual button of silver with nitric acid, and obtained a *very minute trace* of gold.

Exp. 18. Sample purchased at Mr. Caplin's, 42, Great Pulteney-street. Mixed 6000 grs. with 2000 grs. of powdered charcoal, heated the mixture in a Cornish crucible, obtained of lead 3840 grs. Of this lead, cupelled 2000 grs.; the small globule of silver obtained, parted by nitric acid, left a *very minute trace* of gold.

Oxychloride of Lead.—*Exp.* 19. Pattinson's oxychloride. 1 lb. troy (5760 grs.) mixed intimately with 4 ozs. (1920 grs.) of dried carbonate of soda, and ½ oz. (240 grs.) of powdered charcoal, heated the mixture in a Cornish crucible, lead obtained weighed 4140 grs. Of this lead, cupelled 4000 grs. (a small portion of the lead was lost during the operation), no button of silver was obtained (?) There was not a sufficient quantity of this sample left to repeat the experiment.

Exp. 20. Sample purchased at Messrs. Blundell, Spence, and Co., 9, Upper Thames-street, as Pattinson's genuine oxychloride of lead. Reduced 3 lbs. troy in the same way as Exp. 19. Of the lead obtained, cupelled 4000 grs.; the small button of silver obtained, parted by nitric acid, left a *very minute trace* of gold.

Exp. 21. Repeated Exp. 20 on 4000 grs. of lead; treated the button of silver left by cupellation with nitric acid, and a *very minute trace* of gold was obtained.

Acetate of Lead.—*Exp.* 22. Sample purchased at Mr. C. Button's, Holborn Bars, slightly colored with oxide of iron. 2 lbs troy (11,520 grs.) reduced by projecting it in small quantities at a time into a hot Cornish crucible, and finally heating until all the lead was separated; lead obtained weighed 5700 grs. 2000 grs. of this lead, cupelled, did not leave any visible trace of silver.

Exp. 23. Repeated Ex. 22 upon 2000 grs., but no button of silver was obtained.

Exp. 24. Repeated Exp. 22. Took of acetate of lead 2 lbs. troy (11,520 grs.) and reduced it by heating in a Cornish crucible; lead obtained weighed 5860 grs. 4000 grs. of this lead cupelled to about 200 grs. on a large cupel; it was then transferred to a small cupel and

the operation completed; a very minute globule of silver remained, which was treated with nitric acid, and a *just perceptible trace* of gold was obtained.

Exp. 25. Sample bought at Mr. H. Barnes's, 38, Long Acre (very white and clean). 5760 grs. reduced by heating in a Cornish crucible, gave of lead 3136 grs. Of this lead, cupelled 2000 grs., obtained a very minute globule of silver, which by parting with nitric acid left a *just perceptible trace* of gold.

Exp. 26. Repeated Exp. 25. 5760 grs. reduced as before, gave of lead 2830 grs. Of this lead 2000 grs. cupelled gave a very minute globule of silver, which, after parting with nitric acid, left a *just perceptible trace* of gold.

Philosophical Magazine, February, 1854.

(To be continued.)

ORGANIC CHEMISTRY.

Researches *on* Gallic Fermentation. By M. E. Robiquet.

Ever since M. Pelouze proved that gallic acid did not actually exist in nut-galls, but was merely a transformation of tannin, chemists have endeavored to discover what were the causes of this curious metamorphosis.

My father was the first who advanced that nut-galls contained a peculiar substance capable of converting tannin into gallic acid, *in the presence of water and out of contact with the air.* M. Larocque showed likewise that this substance was a veritable ferment, which might be isolated to a certain degree, by exhausting the nut-gall of all the tannin it contained, by means of hydrated ether. Still there was a hesitation about mentioning the word gallic fermentation; for it was known that tannin was susceptible of conversion into gallic acid merely with distilled water, and no one could clearly explain the part performed in this simple solution by a greater or less quantity of the powder of nut-galls exhausted by ether. In fact it was said that this addition hastened the transformation, but was not the necessary cause.

In the endeavor to explain this difficulty, I first applied myself to separate the various principles contained in nut-galls, and I pursued the following method:—

One kilogramme of powdered nut-galls was exhausted, in a displacement apparatus, by hydrated ether, and gave as first product 540 grms. of dry tannin, which I shall designate as *etheric tannin A.*

A second treatment, made with a mixture of equal parts of dydrated ether and alcohol of 36° Baumé, gave a product weighing 270 grms., which I shall call *alcoholic tannin B.*

When the alcoholised ether was completely removed, I moistened the residue with 3 kilogrammes of distilled water, and I put this mixture into a stove which was then maintained for twelve hours, at a temperature of from 68° to 77° F.: the magma had tripled in volume. Having continued the displacement with fresh tepid water, I remarked

that alcohol produced an abundant precipitate in the liquor which was produced, and I continued the operation until this reaction ceased. I mixed all the liquors, and precipitated them with a great excess of alcohol ; I thus obtained a voluminous gelatiniform clot, which having been once washed, pressed, and dried, weighed 31·50 gr. This matter was nothing but pectine, containing some traces of *pectic alcohol C.*

The supernatant liquor of the pectic precipitate was distilled, then evaporated, and gave an extract *D*, weighing 25 grms., and containing traces of tannin, salts with a potassa base, and an enormous proportion of extractive principle.

The residue of these treatments was exhausted by boiling water, and the liquors, simply passed through a cloth, were left to themselves in a cool place. After twenty-four hours they deposited a greyish powder, giving, with ioduretted water, a violet coloration, which disappeared on the application of heat. Examined with a microscope, this powder appeared to be entirely formed of granules of starch, partially disintegrated.

Washed and dried, it weighed 4 grms., *E.*

The liquid above this last deposit gave, when evaporated to dryness, a brown extract, *F.*, weighing 17 grms., which was not examined at all.

Powder of nut-galls, thus exhausted, still slightly colored water, giving it a flavor exactly resembling whey. Still the proportion of matters in solution was so small, that I contented myself with drying this last residue and ascertaining its weight, which was 87 grms. : this was hardly anything but lignine *G.*

Consequently, from one kilogramme of powdered nut-galls, I obtained :—

A Etheric tannin	540·00 gr.
B Alcoholic tannin	270.00
C Pectine and pectic acid	31·50
D Brown extract obtained by tepid water . .	25·00
E Amylaceous deposit	4·00
F Brown extract obtained with boiling water .	17·00
G Ligneous residue	95·00
	982·50

The loss is a great deal too great, and can only be explained by the necessity which I found of using much time and enormous quantities of liquid to produce a complete exhaustion.

We will now return to the *pectic precipitate C.* I proved that this precipitate dissolved very readily in hot water, and was transformed into pectic acid by the alkaline carbonates. This pectic acid, separated by means of hydrochloric acid, was converted into mucic acid by nitric acid.

Finally, I very carefully ascertained all the characters of pectine, especially those which distinguish it from gum, for it was impossible to prepare pectine or pectic acid from nut-gall in a sufficiently pure state for analysis.

I was anxious to know whether pectine originally existed in nut-galls, or whether it proceeded from pectose. For this purpose, I exhausted two kilogrammes of powdered nut-galls by a mixture of equal parts of ether and alcohol (which mixture rapidly dissolves tannin); then I continued the lixiviation with tepid water. Now, as the first portions of the aqueous liquors were not precipitated by alcohol, it was evident that pectine did not originally exist there. This experiment likewise proved that nut-galls do not contain any sensible proportion of pure gum. I now wished to ascertain the species of transformation which produced the pectine. I then stopped the displacement, and having added five kilogrammes of water to the former mass, I kept this mixture during the whole day in the sand bath, at a heat of 77° F. The magma became of considerable bulk, and gave, when pressed, a liquor in which alcohol easily detected the presence of pectine.

After the beautiful observations of M. Fremy on pectic fermentation, it was not difficult to decide that nut-galls contain pectose which, under the influence of water, heat, and pectase, becomes converted into pectine.

It still remained for me to discover whether the pectase or pectic ferment exists in nut-galls in a soluble or insoluble state. I assured myself, by many experiments, that nut-galls contain this ferment in both states, but that the soluble variety exists in them in much smaller proportion than the insoluble.

Dry tannin, such as it is obtained when nut galls are exhausted by hydrated ether, retains insoluble pectase; and this is easily proved by leaving in a cool place, a solution made with 1 part of tannin, and 4 parts of distilled water. It deposits albuminoïd flocks, which are nothing but insoluble pectase. It is still more easy to obtain this insoluble pectase by operating on the liquid etheric tannin as it flows from the displacement apparatus.

I also ascertained that the pectase of nut-galls very easily converted into pectic acid the chemically pure pectine prepared by M. Fremy's process, with the juice of very ripe pears and alcohol.

On the other hand, if tannin is completely purified from all foreign bodies by M. Guibourt's method, and an aqueous solution prepared with this solution, it may be preserved for months without alteration; but if a few centigrammes of pectase are added, made from the juice of turnips, or with nut-galls, the transformation then takes place from tannin into gallic acid, and a principle which M. Strœker thinks is sugar, but which I think is more analogous to gum arabic. However, this is a very delicate point, the discussion of which I intend resuming hereafter.

However this may be, tannin is a combination of gallic acid and a neutral substance very similar to the gums and sugars, which under the influence of certain agents (chiefly the mineral acids), is transformed into real sugar, capable of giving, with ferments, alcohol and carbonic acid, and alcohol.

I have likewise made some experiments for the purpose of discovering the nature of the viscid liquid proceeding from the treatment of

tannin by aqueous ether, and which is generally known by the name of *liquid tannin*. M. Pelouze proved that this liquid tannin was nothing but a very concentrated solution of tannin in water, retaining mechanically a certain proportion of sulphuric ether. However, many chemists were inclined to think that this product was a real tannic ether, and grounded their opinion on the following considerations:

1st.—The moment that the powder of nut-galls is moistened with hydrated ether, an amount of heat is developed which is very appreciable with a thermometer.

2nd.—Liquid tannin does not separate into its two constituent principles when agitated with cold water.

These reasons appear at first sight very conclusive; for heat being disengaged almost always indicates a veritable chemical combination; and, moreover, if syrupy tannin were only a simple aqueous solution of tannin, retaining mechanically a certain proportion of ether, why does not such a solution mix with fresh proportions of water? I add, for my part, that when syrupy tannin is subjected to a cold of from —5° F. to —4° F., it becomes suddenly clear, whereas a solution made with 3 parts of dry tannin, and 2 parts of distilled water, and subjected to the same temperature, becomes completely solid.

However, on examining things a little more carefully, we find that it is impossible to admit, in liquid tannin, the existence of tannic ether. For if a cold of —4° F. is without action on liquid tannin, it is not the same when we lower the temperature to —22° F.; for then the liquid separates into two layers; one is ether, the other, tannin dissolved in water.* Moreover, liquid heated in the sand bath, decomposes at about 120° F. into dry tannin and ether; when it is distilled with a concentrated solution of caustic potassa, sulphuric ether alone passes into the receiver. Finally, in whatever way it may be tried it is impossible to produce amide with ammonia and this supposed tannic ether. This would be, it must be acknowledged, a singular compound ether which heat alone can decompose into acid and ether, and which gives no trace of alcohol with potassa. As for the disengagement of heat observed at the moment of the mixture of hydrated ether and pulverised nut-galls, it is a simple phenomenon of hydration which is not observed if the nut-galls have been for some time exposed to moisture. The opinion of M. Pelouze, therefore, remains unshaken, and the above facts only serve to confirm it.

Conclusions.

1st.—Nut-galls contain (besides tannin and the other principles already mentioned by chemists), pectose and pectase. This last ferment exists in them in the soluble and insoluble states; it acts at once on the pectose to change it into pectine and on the tannin, which it decomposes into gallic acid and a neutral body approximating to the sugars and gums. The presence of water and a temperature of about 77° F. are necessary for this reaction, which should be called, as M. Larocque

* This cold was obtained with the mixture of chloride of calcium and snow prepared by the method of M. Person.

observes, by the name of *gallic fermentation*, without forgetting that it is the same as pectic fermentation.

2nd.—Etheric tannin contains sufficient pectase to undergo gallic fermentation when its aqueous solution is left to itself; but if care is taken to purify the tannin, or if it is exposed for a long time to the heat of boiling-water, the metamorphosis does not take place, and the concentrated solution may be preserved for an indefinite period in closed vessels.

3rd.—It is likewise as easy to convert the pectine of fruits into pectic acid by means of pectase prepared from nut-galls, as it is to transform tannin into gallic acid with pectase made from the juices of fresh roots, especially from half grown turnips.

4th.—Liquid tannin cannot be considered as a tannic ether, but merely as a simple juxtaposition of aqueous ether and tannin in indefinite proportions, as was first established by M. Pelouze.

Annales de Chimie et de Physique, December, 1853.

On ANGELIC *and* CINNAMIC ACIDS. By M. L. CHIOZZA.

WHEN Roman essence of camomile is treated with hydrate of potassa, three distinct phases may be observed in the reaction. In the first place, the essence simply combines with the potassa, producing a reddish gelatinous mass, from which water will remove the essence of camomile unchanged.

If we continue to heat this first product moderately a second reaction takes place, accompanied by a rapid elevation of temperature; hydrogen is disengaged mixed with vapors of the carbonised hydrogen of the essence, and an almost dry saline mass is obtained, consisting of angelate of potassa with an excess of the potassa used.

By decomposing the salt of potassa with a dilute acid, the angelic acid floats in the form of a slightly colored oil, which, on cooling, becomes a mass of beautiful crystals.

These two first phases of the reaction had been already observed by M. Gerhardt, who indicated the preparation of angelic acid by means of Roman essence of camomile; but it often happened, in this chemist's experiments, that the acid was obtained in very small quantity in proportion to the essence employed, and that it refused to crystallise.

Wishing to procure angelic acid by this process, I observed the same phenomena: the acid often remained liquid, and, in this case, it possessed a powerful odor of crystallisable acetic acid.

The production of this acid especially fixed my attention. Angelic acid is, in fact, homologous in its composition with acrylic and oleïc acids, and we know that these acids give acetic acid under the influence of oxidising agents.

These facts rendered it natural to suppose that angelic acid really belongs to the same category as the above-named acids. To attain certainty in this respect, I subjected to the action of heat an intimate mixture of angelate and hydrate of potassa. As soon as this mixture

began to melt, I observed a much more considerable disengagement of hydrogen than had accompanied the transformation of the essence into angelic acid.

This third phase in the action of potassa on Roman essence of camomile, once commenced, it is difficult to stop it, even by removing the capsule from the fire.

When the disengagement of hydrogen had ceased, I left the saline mass to cool, and I decomposed it with sulphuric acid.

Not even a trace of angelic acid then separated. This acid was replaced by a mixture of acetic and propionic acids, which were easily separated from the sulphate of potassa, by subjecting the liquid to distillation. The distilled liquor having been exactly saturated by carbonate of soda, and concentrated to a syrupy consistency, there forms on the next day an abundant deposit of brilliant needles of acetate of soda.

This salt, purifird by crystallisation, and decomposed hot with nitrate of silver, gave light and sparkling lamellæ, presenting all the characters of acetate of silver.

0·335 gr. of this salt gave 0·229 of silver.
0·260 gr. of the same salt gave 0·137 of carbonic acid, and 0·045 of water.

Which gives in hundredths,—

	Found.	Calculated.
C	14·3	14·3
H	1·9	1·8
Ag	64·5	64·6
O	19·3	19·3
	100·0	100·0

The mother liquor from which the acetate of soda had been deposited, treated boiling with a solution of nitrate of silver, abandons on cooling a salt crystallised in heavy grains and lumps, composed of fine needles, easily washed by decantation.

0·477 gr. of this salt gave 0·284 of silver.
0·420 gr. of the same salt gave 0·299 of carbonic acid, and 0·099 of water.

Which gives in hundredths,—

	Found.	Calculated.
C	19.4	19·8
H	2·6	2·7
Ag	59·5	59·6
O	18·5	17·9
	100·0	100·0

We see by these numbers, that the salt analysed was propionate of silver. Moreover it presented all the characters of that salt.

This metamorphosis of angelic acid explains why, during its preparation by means of essence of camomile, it often happens that only very small quantities are obtained; an augmentation of heat of only a few degrees is sufficient to determine the decomposition of the angelate into propionate and acetate.

We may by this means even procure with ease notable quantities of propionic acid of great purity.

When the angelic acid is only partially decomposed, acetic and propionic acids remain in solution, and this mixture forms on the surface of the liquor as an oleaginous and uncrystallisable layer, with a penetrating odor somewhat analogous to that of valerianic acid.

Having communicated the preceding results to M. Gerhardt, in whose laboratory I obtained them, that chemist wished me to perform an experiment which he had himself made on the reaction of alcoholic potassa and essence of camomile. This reaction gave him an acid liquid with a fetid odor, similar to that of valerianic acid, whose salts of silver and baryta contained distinctly the same quantities of base contained in the corresponding valerianates. Now, as the angelates present the same composition within a few thousandths (valerianic acid contains two atoms of hydrogen more than angelic acid), M. Gerhardt suspected that the acid liquid which he took for valerianic acid was only angelic acid retained in the liquid state by a little acetic and propionic acids, and that, consequently, its proportions were similar to the angelates:

	Found.	Calculation.	
	by M. Gerhardt.	Composition of the angelates.	Composition of the valerianates.
Barium of the salt of baryta . .	46·5	40·7	40·3
Silver of the salt of silver . . .	51·9	52·1	51·7

On repeating the experiment in question, I found that M. Gerhardt's supposition was well founded.

It is, then, plainly demonstrated that angelic acid, which in its composition is homologous to acrylic and oleïc acids, is likewise so in its properties.

The action of potassa on these three acids is very clearly shewn by the following equations:—

$$\underset{\text{Acrylic acid.}}{C^6H^4O^4} + 2\,KHO = \underset{\text{Formiate of potassa.}}{C^2HKO^4} + CHKO^4 + H^2$$

$$\underset{\text{Angelic acid.}}{C^{10}H^8O^4} + 2\,KHO = C^2HKO^4 + \underset{\text{Propionate.}}{C^6H^5KO^4} + H^2$$

$$\underset{\text{Oleic acid.}}{C^{36}H^{34}O^4} + 2KHO = C^2HKO^4 + \underset{\text{Ethalate.}}{C^{16}H^{31}KO^4} + H^2$$

Another acid is known which, from its composition, may be considered as the fifth of this series, the damaluric acid ($C^7H^{12}O^4$) of M. Stædler. If its homology with the preceding acids really exists, it should be transformed, by hydrate of potassa, into valerianic and acetic acids.

Cinnamic acid presenting to benzoïc acid, the same relations that angelic acid does to propionic acid, I was naturally led to examine whether it were susceptible of the same metamorphosis.

It was already known that cinnamic acid gives benzoïc acid under the influence of oxidising agents, but acetic acid had not been mentioned as among the products of this reaction.

When cinnamate of potassa is heated with an excess of hydrate of potassa, it produces an abundant disengagement of hydrogen, and the

cinnamic acid is completely tranformed into benzoic and acetic acids; at the same time a little salicylic acid is formed; but this acid proceeds from the secondary action of potassa on the benzoic acid. On finishing the operation, as in the preceding case, there is obtained an aqueous solution of benzoic and acetic acids, in which the presence of the latter is manifested by its odor.

The separation of these acids presenting many difficulties, I found it impossible to obtain the acetic acid in a state of sufficient purity for analysis. I tried the salts of iron in vain; the acetate always retained a little benzoate whose presence was manifest in the salt of silver. In fact, a trace of benzoate of silver in acetate of the same metal is sufficient to produce, instead of long and brilliant lamellæ, small tender concretions composed of fine needles matted together. The silver salt which I obtained presented the latter appearance.

0·165 gr. of this salt gave 0·104 of silver,

Which gives, in hundredths:

		The acetate requires		The benzoate
Ag	63·0	64·6		47·1

Thus, the salt analysed was acetate of silver containing a little benzoate. Treated with sulphuric acid it disengaged the penetrating odor of acetic acid; a mixture of sulphuric acid and alcohol disengaged acetic ether.

Finally, coumaric acid appears to behave in the same manner as cinnamic acid; however, I have not succeeded in collecting sufficient acetic acid among the products of the action of potassa on this body, to be able to prove its presence by analysis.

The metamorphosis of cinnamic and coumaric into benzoic or salicylic and acetic acids is explained by the following equations:

$$C^9H^8O^2 + 2\,KHO = C^2H^3KO^3 + C^7H^5KO^2 + H^2;$$
$$C^9H^8O^3 + 2\,KHO = C^2H^3KO^3 + C^7H^5KO^3 + H^2.$$

Annales de Chimie et de Physique, December, 1853.

On the Dried Coffee Leaf *of* Sumatra, *which is Employed in that and some of the adjacent Islands as a substitute for Tea or for the Coffee-bean.* By John Stenhouse, LL.D., F.R.S.

I recently received from my friend Daniel Hanbury, Esq., a quantity of dried coffee-leaves which had been prepared in Sumatra, under the direction of N. M. Ward, Esq., of Padang.* The sample had a deep brown color, and consisted of the leaves of the coffee-tree mixed with fragments of the stalks. The leaves had been very strongly roasted in rather a rough manner, and had consequently acquired a slightly empyreumatic odor. In this respect they pretty closely resemble Paraguay tea, the leaves and twigs of the *Ilex paraguayensis*, which is subjected to a somewhat similar process. The coffee-leaves, when digested with boiling-water, yielded a deep brown infusion, which in taste and odor closely resembled an infusion of a mixture of coffee

* *See* a paper, by Mr. Hanbury, in the 13th vol. of the "Pharmaceutical Journal," p. 207.

and tea. On the addition of milk and sugar, it formed a very tolerable beverage; and as the roasted coffee-leaf can be imported into Europe for rather less than twopence per pound, the poorer classes are likely to find it a very useful substitute for tea and coffee. Should a more moderate temperature be employed in drying the coffee-leaf, I think its flavor would be greatly improved.

The coffee-leaf, as might almost have been expected, contains the two characteristic constituents of the coffee-bean, viz., theïne, or caffeïne, and caffeïc acid. In this respect the coffee-leaf differs essentially from chicory or any of its adulterations, such as roasted turnips, mangel-wurzel, carrots, &c., the usual substitutes for coffee, which do not contain a trace of either of these principles.

The theïne or caffeïne was extracted from the coffee-leaves in the usual way, by precipitating the coloring matter and other impurities, first by acetate and then by subacetate of lead. The acetate of lead threw down a dark brown precipitate containing all the caffeïc acid, and the subacetate produced a scanty bright yellow precipitate. The excess of lead was then removed from the clear solution by sulphuretted hydrogen, and the sulphide of lead having been collected by filtration, the theïne crystallised when the liquid, after being sufficiently concentrated, had been set aside in a cool place for some days.

The crystals of theïne as first obtained were of a brownish color, but after being strongly pressed between folds of blotting-paper and repeatedly crystallised, they were rendered nearly colorless.

1. 1000 grains of dried coffee-leaves, when treated in the way just described, yielded 12·5 grains theïne = 1·25 per cent.

2. 1000 grains of coffee-leaves in a subsequent trial gave 11·54 grains = 1·15 per cent.

The amount of nitrogen in the dried coffee-leaves was also determined by Will's method.

1·344 grm. substance gave 0·2005 platinum = 2·118 per cent. nitrogen.

0·7775 grm. gave 0·1185 platinum = 2·165 per cent.

Now it has been ascertained as the result of numerous experiments,* that "coffee contains from 0·80 to 1 per cent. of theïne, and that tea contains 2 per cent. of the same principle." "And the nitrogen in coffee-beans (*see* page 27 of the same Report) lies between 2½ and 3 per cent."

By a recent determination, I found that 1000 grains of a good black tea gave 21·3 grains of theïne = 2·13 per cent.

1000 grains of a black tea grown in the East India Company's tea plantations at Kemaon, on the Himalayas, gave 19·7 grains = 1·97 per cent.

0·4705 grm. of the same Kemaon tea gave 0·1175 platinum = 3·5 per cent. nitrogen.

* *See* the 32nd page of the "Chemical Report on the Mode of Detecting Vegetable Substances mixed with Coffee, for the purposes of adulteration," drawn up by Professor Graham, Dugald Campbell, Esq., and myself, for the British Government, and communicated to it in December, 1852.

Many years ago I detected the existence of theïne in what is called Paraguay tea, the dried leaves and twigs of the *Ilex paraguayensis*, but I neglected to determine its amount. I have recently found that 1000 grains of Paraguay tea yielded 12·3 grains of theïne = 1·23 per cent.

a. 1000 grains ditto gave 11 grains = 1·1 per cent.

1·748 grm. gave 0·1865 platinum = 1·51 nitrogen per cent.

1·031 grm. gave 0·123 platinum = 1·70 nitrogen per cent.

From these results, it is clear that dried coffee-leaves are somewhat richer in theïne than the coffee-bean, and contain, as nearly as may be, the same amount of that principle as Paraguay tea.

From the violent roasting to which the coffee-leaves had been subjected, I feel convinced a portion of their theïne has been dissipated; and were they only dried at a moderate temperature, I confidently expect that they would yield 1½ per cent. of theïne. The theïne obtained from the coffee-leaves was not subjected to analysis. This I considered unnecessary, as it possessed all the well-known properties of ordinary theïne, crystallising in fine silky crystals, which readily sublimed when heated; and when digested with nitric acid and cautiously evaporated to dryness, they gave when treated with ammonia the characteristic red coloration so closely resembling that from uric acid when acted on by the same reagents.

With regard to caffeïc acid, the other characteristic proximate principle of coffee, the leaf of the coffee-plant contains it also in larger quantity than the berry. Caffeïc acid is precipitated of a deep yellow color by acetate of lead, but is apparently uncrystallisable; at least the numerous attempts which I have made to obtain it in a crystallisable state have hitherto proved unsuccessful. Caffeïc acid does not precipitate solutions of gelatine, and it is therefore not a species of tannin, as has been sometimes asserted. The most remarkable property of caffeïc acid is that first stated at the 34th page of the joint Report on the adulteration of coffee already quoted. "Caffeïc acid appears to be analogous to kinic acid, the acid of cinchona barks, for it yields kinone when oxidated by means of sulphuric acid and binoxide of manganese. To observe this properly, the coffee is boiled with water and a little slaked lime, the infusion filtered and evaporated down to the consistence of a syrup. The syrupy liquid is then mixed in a retort with four times its weight of binoxide of manganese and one part of oil of vitriol diluted with an equal bulk of water. Sufficient heat is produced by the action of the sulphuric acid upon the other materials, to bring over the greater portion of the kinone; and the lamp need not be applied till towards the close of the operation. The distillate consists of yellow crystals of kinone, which usually coat the neck and sides of the retort, and a bright yellow liquid, which is a saturated aqueous solution of kinone, with a considerable quantity of formic acid. Kinone is easily discernible by its volatility and peculiarly acrid odor, which greatly resembles that of chlorine. The solution of kinone gives with ammonia a sepia black color. It is decolored by sulphurous acid. The beautiful green hydro-

kinone is obtained by exactly neutralising the solution of the yellow kinone with sulphurous acid, great care being taken not to introduce the latter in excess.

"The peculiar acid in Paraguay tea agrees with caffeïc acid, to which it is no doubt related, in yielding kinone to similar oxidising agencies; so does the acid of the leaves of the common holly, *Ilex aquifolium*, and the whole of the cinchona tribe."

When coffee-leaves are boiled with a considerable quantity of water, and a slight excess of milk of lime, the dark brown strongly alkaline liquor is cautiously evaporated to dryness, and then treated in the way already described, with three times its weight of black oxide of manganese and one part of sulphuric acid, diluted with its own bulk of water, a much larger quantity of crystals of kinone are obtained than can be procured from an equal weight of the coffee-bean. This clearly shews that the leaves are richer in caffeïc acid than the beans.

Kinone may, I find, be obtained in small quantity by a similar process from a great number of our commonest plants. Thus I also obtained indications of kinone from the leaves and branches of the privet, *Ligustrum vulgare;* from the common ivy, *Hedera helix*, from the *Quercus Ilex*, the evergreen oak of our gardens and shrubberies, a native of Turkey; from the *Quercus robur*, the common British oak; from the *Ulmus campestris*, the common small-leaved elm; from the ash, *Fraxinus excelsior;* and from the bush-tree of the Cape of Good Hope, the *Cyclopia latifolia* of De Candolle, a plant of the natural Order Leguminosæ.

From numerous plants which I tried I could not obtain a trace of kinone. This was the case, among others, with laburnum, *Cytisus laburnum;* with tobacco; with *Prunus spinosa*, the sloe; and others too numerous to mention.

The kinone was only obtained in crystals from the coffee-bean, from the coffee-leaf, and from the holly, *Ilex aquifolium*. In all the other cases, its existence was detected by the deep yellow liquid which distilled over at a comparatively low temperature, and which yielded the dark humus-like coloration with ammonia, so characteristic of solutions of kinone. Kinone is so exceedingly soluble in water, that unless a considerable amount of it is evolved, and proper precautions are employed, a deep colored solution of it only is obtained, either from the coffee-leaf, the berry, or the leaves of the holly. The comparatively small amount of the kinone yielding substance, or perhaps, we should say, substances, present in such plants as the privet, the oak, &c., is most probably the only reason why crystals of kinone have not likewise been obtained from these plants.

In order to assist in forming an estimate of the comparative value of coffee-leaves as a beverage as compared with the bean, I determined the amount of soluble matter which each of them yielded to boiling water. 6·048 grms. of dried coffee-leaves and 6·038 grms. of roasted and ground coffee beans were repeatedly treated with precisely similar quantities of boiling water, till the liquid which came off from them was nearly colorless. The 6·048 grms. coffee-leaves were found to

have lost 2·348 grms. = 38·8 per cent., while the 6·038 grms. roasted coffee-beans had lost 1·759 grms. = 29·1 per cent. From this determination it is clear that coffee-leaves yield to boiling water near 10 per cent. more soluble matter than the bean. In this respect, therefore, the coffee-leaf has an advantage over the berry.

So far as regards the two characteristic principles of coffee, viz.: caffeic acid and theïne or caffeine, these are common both to the leaf and the bean, the leaf being decidedly rich in both. In other respects, however, they differ considerably. The coffee-leaf contains some tannin, and scarcely any sugar or fat. The coffee bean contains about 12 per cent. of fat and "8 per cent. of cane-sugar." (*See* Joint Report.)

So far as I can judge, the infusion of the coffee-leaf has a much greater resemblance to that of tea than to a decoction of the coffee bean; so that should the coffee-leaf ever come into general use in European countries, it will be rather as a substitute for tea than for coffee. If the coffee-leaves were only dried at a somewhat lower and better regulated temperature, I have little doubt that they would yield a much more agreeable beverage than can be made with the present roughly roasted and partially empyreumatised product.

Pharm. Journal.

On the Presence *of* Formic Acid *in the* Human Secretions.

By Dugald Campbell, Analytical Chemist to the Brompton Hospital

In a former communication upon this subject,* it will be noticed that no acids whatever were used by me either upon the vomits or urine, to distil from the former the formic acid and from the latter the formiate of ammonia. My object in avoiding acids was to preclude its being said, that, by their action with heat upon the substances I examined, I had assisted in the formation of the formic acid; although, for my own part, I do not see any objection to the use, especially with care, of the inorganic acids not easily decomposable by organic substances with heat, such as phosphoric acid, and likewise of several of the stronger organic acids, such as tartaric acid.

Not using an acid to liberate the formic acid in the urine caused the experiments† to be very much more bulky and tedious than they otherwise would have been. A pint of urine in a retort, acidified by either phosphoric or tartaric acid, and distilled carefully by a water-bath, will yield sufficient formic acid to be detected by all ordinary tests.

The operator should observe that the urine in the retort is acid at the conclusion of the experiment, or the distillate may be tested from time to time by sensitive litmus paper; if found alkaline, the heat must be withdrawn, and when the contents have cooled considerably, more acid added to the retort. A good deal of trouble may be saved

* Chem. Gaz. vol. xi. p. 310.

† Chem. Gaz., vol. xi. p. 312.

by having, in place of the stopper of the retort, a perforated cork with a thistle funnel passing to nearly the bottom of the liquid, and by this means adding the acid gradually if the liquid comes over alkaline, as stated before.

I have been engaged for some time back in the examination of blood of phthisical patients before, during, and after different treatment, for an investigation not my own, and with a different object in view than the detection of formic acid; still I thought it would be an interesting fact to pronounce whether formic acid is or is not to be found in blood.

The quantity of blood which I had to operate upon was necessarily very small; little more than half an ounce from each patient. The serum fortunately was not much required for my other investigations, and was what I alone wanted, so I was able to collect a few ounces; and before it became putrid I acidified it, and distilled it in a retort by means of a water bath. Whether I acidified with phosphoric or tartaric acid, the results I obtained were the same. The distillate, treated as I have stated in my last paper,* when acting upon the acid liquid from the vomit, will yield an acid with all the properties corresponding to formic acid.

The only other opportunity which I had of examiuing blood was from a woman said to be healthy, otherwise than in a tendency of flow of blood to the head. In this I found formic acid likewise, but as far as I could judge by qualitative tests, not in such quantity as in the blood of the phthisical patients first examined.

Chem. Gaz. February 1st, 1854.

ANALYTICAL CHEMISTRY.

Researches *on* Atmospheric Ammonia. By M. J. Isidore Pierre.

In a former work, presented to the Academy of Sciences on the 7th June, 1853, I arrived at the following result:—

In the neighbourhood of Caen—under the almost constant influence of winds which removed any emanations from the town, from the place where the observations were taken, there were found in the air, as the mean for the winter season, more than 4½ milligrammes of ammonia per cubic metre of air, that is to say nearly 3½ millionths of the weight of the air.

Those experiments continued for 118 days, during the months of December 1851, January, February, March, and April 1852, and were tried on 2700 litres of air.

The experimentalists who preceded me on this subject had always operated on a much smaller volume of air (from 400 to 1100 litres), and this circumstance may be one principal source of the discrepances observed between the results obtained.

The place and mode of experimentation, atmospheric circumstances,

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* Chem. Gaz. vol. xi. p. 311.

and many other causes, may likewise have had an influence on these results, of which it would be difficult to form an idea in the present state of our knowledge.

In all cases agronomical deductions, of considerable importance, have been drawn from the presence or absence of ammonia in the air, deductions which require to be strictly controlled by multiplied experiments.

These considerations determined me to renew the investigation in rather different conditions, and operating on a larger mass of air, 4015 litres instead of 2720 litres.

These 4015 litres of air were collected during 169 days, divided among nine months, at varying hours of the day, from the 15th May, 1852, until the 1st April, 1853, inclusive.

The following table shews for each month :—

1st. The quantity of air taken between 5 and 11 A.M.

2nd. That taken between 11 A.M. and 5 P.M.

3rd. The quantity of air taken between 5 and 11 P.M.

4th. That taken between 11 P.M. and 5 A.M.

Name of Month.	Between 5 and 11 A.M.	Between 11 A.M. and 5 P.M.	Between 5 and 11 P.M.	Between 11 P.M. and 5 A.M.	Gen. Total.
May 1852 . . .	100	80	85	0	
June „ . . .	35	35	25	0	
July „ . . .	275	230	350	5	
August „ . . .	235	160	235	5	
October „ . . .	10	5	0	0	4015
January 1853 . . .	180	315	235	10	
February „ . . .	175	210	225	65	
March „ . . .	180	225	275	35	
April „ . . .	10	5	0	0	
Totals . .	1200	1265	1430	120	

The curious and interesting observations of M. Barral on the rain waters of Paris, in which he found notable quantities of ammonia or ammoniacal salts, were of such a nature as to lead to the idea that it would not be useless to keep an account of the rainy days in such investigations.

The following table will give, for each of these nine months,—

1st. The number of days on which air was taken ;

2nd. The number of days during which rain or snow fell ;

3rd. The number of days in which, although there was no rain, there were moist fogs.

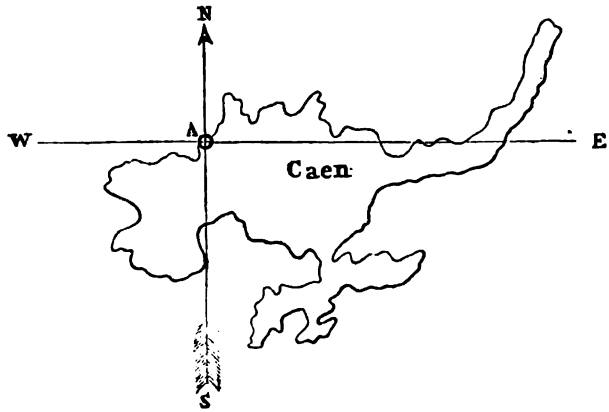
Name of Month.	Number of days on which the air was taken.	Number of days on which either rain or snow fell.	Days without rain, but foggy.
May 1852 . .	16	8	1
June „ . .	4	1	0
July „ . .	30	9	4
August „ . .	28	14	1
October „ . .	2	2	0
January 1853 . .	29	16	3
February „ . .	28	13	5
March „ . .	31	10	3
April „ . .	1	0	0

As the direction of the wind might have a considerable influence on the results, especially in the neighborhood of a large town, I made, during the time of my experiments, 752 observations on the direction ot the wind.

These observations are shewn in a third table, and divided in the following manner:—

Direction of the wind.	Number of times that the direction of the wind was observed in									Total
	May 1852	June	July	August	October	Jan. 1853	February	March	April	
N.	0	0	39	17	0	27	29	35	0	147
N. ¼ N.W. . .	0	2	14	6	0	3	4	8	0	37
N.N.W. . . .	0	0	12	3	0	12	20	14	0	61
N.W.	5	2	9	17	0	11	30	10	0	84
W.N.W. . . .	3	2	4	0	0	11	5	8	0	33
W. ¼ N.W. .	5	3	0	0	0	0	1	2	0	11
W.	0	0	0	0	0	3	3	5	0	11
W. ¼ S.W. . .	0	3	1	2	0	1	0	0	0	7
W.S.W. . . .	1	3	1	3	0	9	4	13	2	26
S.W. ¼ W. . .	0	0	11	0	1	0	0	0	0	12
S.S.W.	6	7	2	17	2	22	2	11	1	70
S.W.	2	1	8	22	0	42	28	29	0	122
S. ¼ S.W. . .	0	3	1	25	0	1	0	2	0	32
S.	0	0	4	1	0	1	1	2	0	9
S. ¼ S.E. . . .	0	0	2	0	0	0	0	1	0	3
S.S.E.	0	0	11	0	0	0	0	1	0	12
S.E.	0	0	4	1	0	0	0	0	0	5
E.	2	0	0	0	0	0	0	0	0	2
E.N.E.	3	0	0	0	0	0	0	0	0	3
N.E.	2	0	2	1	0	1	4	0	0	10
N.N.E. . . .	2	0	2	2	0	1	3	2	0	12
N. ¼ N.E. . .	4	0	15	7	0	2	1	4	0	33
Total . .	35	26	145	124	3	147	135	137	3	752

To be able the more readily to judge of the influence of the direction of the wind and that of the neighborhood of the town, I have drawn a small bird's eye view of the town of Caen, in a scale of about 1-80,000dth. The place where the observations were taken is indicated by the letter A.



It is easy to comprehend by means of this figure, that the place where the observations were made is free from the influence of the wind from the city when their direction is between the north north west and west; and as the direction of the wind was always observed when each fresh volume of air was taken, and as of 752 observations of the wind, 379, or nearly half, were comprised within the limits which we have just mentioned, it results that nearly half the air subjected to experiment was collected before passing over the city.

I used the same apparatus as in my former researches, and the tubes with the bulbs were, as in the first case, washed with the greatest care, after a treatment with nitric acid.

The greatest speed of aspiration has not exceeded three litres in the hour; it was more frequently under this limit.

The mouth of the leaden tube by means of which the air was taken was about three metres from the ground, in my former experiments; in these new ones it was carried to a height of 8·10 metres.

The spot where the present observations were made is about 35 metres from that of the preceding, and in even a more airy situation, and farther from any habitations.

The opening of the tube was placed at $1{\cdot}50^{m}$ before the window of a room in which was placed the aspirator, as in the former experiments. The opening of this tube was turned down, to avoid the introduction of rain.

The method which I employed for the estimation of the ammonia was as follows:—after having removed from the tubes the acid which had absorbed the ammonia, I washed them several times with the

greatest care, with a known quantity of distilled water. Into the acid liquor proceeding from the union of the washing water and the original acid, I poured 30 drops of a weak solution of chloride of platinum, and subjected the mixture to evaporation in the sand bath.

When the liquor was reduced to 2 or 3 grms., I added 10 more grms. of distilled water, to facilitate the disengagement of the hydrochloric acid; the whole quantity of water used was 84 cubic centimetres.

After this last evaporation, I poured into the capsule containing the residue, almost dry and previously cooled, a mixture of alcohol and ether, containing 1 volume of ether to 3 of alcohol; then I left the mixture until the next day under a glass shade, to prevent dust. I poured the contents of the capsule on to a very small paper filter, then I washed the capsule with the mixture of ether and alcohol, until no more chloride of platinum passed.

Roasted with great care in a platimum capsule, the little precipitate obtained left a residue of platinum weighing 0·01625 grms., representing 0·00294 grms. of ammonia.

The same quantity of the hydrochloric acid, treated in the same manner, by the same quantity of the same solution of chloride of platinum, in the same distilled water, and the same alcoholic mixture, filtered through a similar filter, only left a small residue of platinum, weighing 0·00225 grms. equivalent to 0·00032 of ammonia.

Deducting from the quantity of ammonia found, that which might be imputed to the reagents employed, we arrive at the number 0·00262 grms. of ammonia, for 4015 litres of air, or 62-100dths of a milligramme per cubic metre, or about 1-½ millionth of the weight of air.

This quantity of ammonia only represents a seventh part of the proportion which I found in my former researches.

Must we suppose that this difference is because the air really received less ammonia in the second period of time than in the first? Must we attribute it to the circumstance that it was taken, on this occasion, at 5·1 metres higher than in the first observations, and in a less sheltered spot.

We can only reply to these questions by conjectures.

We are naturally led to think that during the summer months, when organic matters ferment more readily, that more ammonia should be diffused into the air; and as the nine months during which the new observations were made, comprised those of June, July and August, we might have anticipated finding a more considerable quantity of ammonia than in the winter months.

But we repeat that the conditions were not the same:—

1st. The air was taken 5·1 metres higher, and in a more airy situation.

2nd. A much larger proportion of water fell during the time of the observations.

3rd. One portion of the air was taken in a season when the foliaceous organs of the vegetables which cover the earth probably exerted a sensible influence.

The question of the variable quantities of ammonia diffused through the atmosphere would require numerous researches. Such as,—

1st. Simultaneous determinations at different heights, at the same place.

2nd. Simultaneous determinations in places near each other, in the open air, and under trees in full vegetation, at the same height.

3rd. Determinations at one spot in different seasons, &c.

These are investigations which appear to me to deserve the attention of experimentalists, and which I can only now mention, not knowing whether I may be permitted to execute any part of them myself.

Annales de Chimie et de Physique, December 1853.

New PROCESS *for* PROVING *the* PRESENCE *of* IODINE, *and* DETERMINING *its* PROPORTION. By M. DE LUCA.

AMONGST the different processes in use for the estimation of iodine or for proving its presence, there are some of very great sensibility; I have proved their value on the occasion of the analysis which I was employed to make of two samples of bromine prepared by M. Tissiera.

These two samples of bromine were perfectly free from iodine; but some persons having persisted in the contrary assertion, M. Balard had the kindness to shew me a process, which I tried for the first time at the laboratory of the *Collége de France*, which is very practical, of an almost unlimited sensibility, and which enabled me to finish the dispute.

This is a very simple process; it may even succeed in very inexperienced hands; and the presence of chlorine or bromine makes no difference to it. The following is the mode of operating:

The liquid supposed to contain iodine in the state of iodide is introduced into a tube closed at one end, and a few drops of sulphuret of carbon or chloroform poured in, then a very dilute aqueous solution of bromine is added. The bromine only decomposes the iodides without touching either chlorides or bromides; the mixture is agitated: the displaced iodine dissolves in the sulphuret of carbon, which it colors of a deeper or paler violet color, or of a rose color, if in very small quantity.

We can thus easily discover the iodine contained in 1–100th of a milligramme of iodide of potassium, and with some precautions, this sensibility may be carried to 1–1000th of a milligramme.

It is necessary to avoid an excess of bromine, which would form a combination with the iodine which would not give a violet coloration with sulphuret of carbon; and, besides, the bromine in excess will give a yellow color to this same sulphuret of carbon.

If the ioduretted solution is alkaline, it is necessary to neutralise it with weak nitric acid before subjecting it to the above described treatment.

This process may also be applied to the estimation of iodine. For this purpose a normal solution of bromine is prepared with 1 grm. of bromine and 4 litres of distilled water; 4 cubic centimetres of this

solution contain 1 milligramme of bromine; 40 cubic centimetres of this solution, that is, 10 milligrammes of bromine are taken, and the necessary quantity of water is added to form a litre, namely, 960 cubic centimetres of distilled water; each cubic centimetre of this new solution will contain 1–100th of a milligramme of bromine.

Two thin, graduated pipettes, are necessary to make this operation; one for the bromuretted water, the other for sulphuret of carbon; for it is necessary always to employ the same quantity of sulphuret of carbon, to be able to appreciate the shade of color in the same volume of liquid.

After a first operation the sulphuret of carbon colored by the iodine is removed, and replaced by a fresh volume of the liquid; and this operation is repeated until the liquid no longer becomes colored.

This is a kind of estimation which may be compared to that of silver by chloride of sodium, in which the operation is stopped when the chloride no longer gives a precipitate; in this case the action is ceased when the sulphuret of carbon is no longer colored.

The quantity of bromine employed, that being deducted which did not color the sulphuret of carbon, indicates, by a simple calculation founded on the chemical equivalents, the quantity of iodine set free, and contained in the substance analysed.

The normal solution of bromine should be added by drops, having first ascertained how many drops form 1 cubic centimetre.

According to the above, we can estimate at one time the chlorine, bromine, and iodine contained in a given mixture in the following manner:

By means of an estimated solution of silver the quantity of silver necessary for the precipitation of the three metalloids is ascertained; then by means of bromine the iodine is estimated; finally, with the known chlorine the iodine and bromine are estimated together, and thus are obtained the elements necessary for the calculation.

It is almost unnecessary to mention here how to prepare the known solution of chlorine. First, a solution of chlorine in distilled water is prepared, which is then diluted with a sufficient quantity of water to make a predetermined volume. The strength of this solution is ascertained by means of a solution of iodide of potassium of known strength, proceeding for this purpose with sulphuret of carbon, as above described.

The quantity of chlorine used to drive off all the iodine from the iodide of potassium, indicates the strength of the solution.

The solution of chlorine should be freshly prepared and carefully preserved in a blue bottle with a ground-glass stopper. When the chloruretted liquid has been prepared for several days, it is better to determine its strength before using it.

Comptes Rendus, December 5th, 1853.

On the ESTIMATION *of* NITRIC ACID, *accompanied by* ORGANIC MATTERS; *application to Tobacco.* By M. TH. SCHLOESING.

THIS memoir, which contains the results of numerous researches, will shortly be the subject of a report: we here only give the introduction.

All chemists, says M. Schloesing, are acquainted with the elegant process employed by M. Pelouze for the estimation of nitric acid, especially nitrate of potassa. Partly founded on the reduction of hypermanganic acid by protochloride of iron, this process presupposes that the nitrate is free from substances capable of instantaneously reducing hypermanganic acid. I say instantaneously, because the reduction of this acid by an extremely small quantity of the salt of iron being instantaneous, a substance which would require a certain time to produce a similar phenomenon, would not cause any sensible perturbation in the estimation.

But there are many substances, especially among organic bodies, which reduce hypermanganic acid at the very instant that they come in contact with it. When a nitrate is mixed with such a substance, the process of M. Pelouze is inapplicable; he has even taken the precaution of stating this fact in his memoir. In such cases, can we, by chemical means, eliminate the substance which prevents the estimation? This is sometimes possible; but, in general, the means of separation possessed by chemists are insufficient, especially when we wish to determine the nitric acid in a complex mixture of organic products, such, for example, as a vegetable extract.

Wishing to know the quantity of nitric acid contained in various kinds of tobacco, a case in which the use of hypermanganic acid is impossible, I tried to discover some other means of determination. The process with which I was satisfied appeared to me sufficiently exact and general to deserve being known, and I do not hesitate to make it the subject of a memoir, knowing that the estimation of nitric acid in the presence of organic matters will aid in the study of several important questions, among which I may mention the formation and the decomposition of nitric acid under natural influences; the study of the quantities of this acid contained in soils, manures, vegetables, and waters of nature; the knowledge of the part played by nitric acid in vegetation, and whether green leaves have the property of decomposing it to cause its nitrogen to enter into the production of nitrogenous matters.

This Memoir is in three divisions. In the first I describe a process for the estimation of nitric acid, supposing that this acid is free from organic matters; in the second, I shew that this process extends to those cases in which nitric acid is accompanied by organic matters; in the third, I give, as an example, the application which I have made of it to the case of tobacco.

Comptes Rendus, December 5th, 1853.

On a NEW METHOD *of estimating* NITRIC ACID, *whether alone or accompanied by any nitrogenous organic substances, with the exception of* AMMONIA. By M. MARTIN.

IT is a fact well known to all chemists, and first proved by M. Kuhlmann, that if we pour nitric acid or a nitrate into a vessel from which hydrogen is disengaged, the gaseous disengagement becomes slower and sometimes ceases entirely. There is then formed, besides sulphate of zinc, an ammoniacal salt due to the transformation, by the nascent hydrogen, of nitric acid into ammonia.

M. Gerhardt, in his introduction to the "Study of Chemistry" by the system of units, and M. Barral, in his "Memoir on Rain Waters," are the only chemists who, to my knowledge, have advanced that the transformation took place equivalent for equivalent. But they cite no experiment, no result of analyses in support of their assertion.

I have endeavored to ascertain whether the transformation of the nitric equivalent into ammoniacal equivalent was entire. The numerous analytical experiments which I made, leave, I think, no doubt upon the subject. I then thought that a method of estimating nitric acid might be founded on this fact which should be easy, requiring little time, and serve as a check on the ordinary method of combustion and the exact process of M. Pelouze.

Moreover, by the method of organic analysis, it is only possible to employ very small quantities of substance, which again is sometimes accompanied by other substances which assist in throwing a doubt over the results obtained. On the other hand, the process of M. Pelouze is sometimes inapplicable; thus, in the search for and estimation of the nitric acid in rain, river, spring, and sea waters, a residue is always obtained which may contain nitrogenous organic substances which are not ammoniacal, which are very difficult of elimination, and which always add a lesser or greater quantity of nitrogen to that of the nitrate, or rather reduce the alkaline permanganate at the moment of contact.

The method which I have the honor to submit to the judgment of the Academy has none of these drawbacks, and, moreover, very considerable quantities of matter may be employed.

Method of Analysis.—Into a glass containing a piece of zinc, which has that instant been washed, is put the salt or solution in which we seek nitric acid and wish to estimate it, and it is boiled with caustic potassa prepared with perfectly pure bitartrate of potassa, so as to remove any ammonia which it might contain. Perfectly pure sulphuric or hydrochloric acid is then added by degrees. It is better to take four or five times as much zinc as there is supposed to be nitric acid, for the quantity indicated by calculation is not sufficient, because a little free hydrogen is always disengaged. It was then necessary to prove that the nascent hydrogen has no action on the nitrogen of other nitrogenous substances, and causes them to undergo no decomposition.

For this purpose, I added, into the glass in which the transformation was being performed, gelatine, uric acid, sulphate of quinine, and the

nitrogenous substance found in the water. The gelatine alone interfered with the results; it rendered the entire transformation required more difficult, but nevertheless it did not prevent it. The other substances were absolutely without action.

All that remained now to be done was to estimate the ammonia obtained. In consequence of the small volume of liquid which there is to operate on, a quantity never exceeding 25 or 30 cubic centimetres, I have given the preference to M. Schlœsing's method, as the most convenient and rapid. I say the most rapid, although it sometimes requires four days; for, as that chemist observes, "we should draw a distinction between the duration of an experiment and the time which the chemist is obliged to devote to it."

I must mention, before proceeding further, a more simple disposition which I have given to the apparatus intended for absorption. I take a vessel similar to a Daniell's pile, I fix the edges firmly on to a thick glass. This has a hole in its centre, and thus permits, by means of a well waxed cork, the introduction into the apparatus of a piece of test-paper. My apparatus is, consequently, merely that of M. Schlœsing reversed.

To be perfectly sure that the glass placed over the vessel is hermetically closed, it is a good plan to paste it round with suet, or, better still, with melted caoutchouc, and to place upon it a sufficiently heavy substance. By choosing a flat bottomed vessel, of from ten to twelve centimetres in diameter, the liquid from which the ammonia is to be expelled forms a very thin layer, which is very favorable to the complete disengagement of the body which is to be estimated.

It may often happen that we wish to estimate the nitric acid in a liquid containing salts of lime, magnesia, &c., salts which, in the presence of potassa, give voluminous precipitates; the displacement of the ammonia will then take a longer time, especially when the temperature is low.

Strength of the normal Liquids employed.—The test liquor of sulphuric acid which I have used, contained, in each cubic centimetre, 0·005 gr. of anhydrous sulphuric acid. The alkaline liquor intended to complete the saturation, was a very dilute solution of ammonia, of which 39 cubic centimetres, 390 divisions of the burette, were required to neutralise 10 cubic centimetres of the sulphuric acid liquor. The 390 divisions of alkaline liquor contained, consequently, 0·0325 gr. of ammonia (NH^3O), or 0·0000833 gr. in each division.

The Memoir is terminated by ten experiments, proving its theory.

Comptes Rendus, No. 25, December 19th, 1853.

QUALITATIVE *and* QUANTITATIVE ANALYSIS *of* IODINE *and its separation from* BROMINE *and* CHLORINE *by means of* BENZINE *and* NITRATE *of* SILVER. By M. ED. MORIDE.

BENZINE has the property of dissolving iodine wherever it finds it in a free state.

The color of this solution is of a bright red. It becomes darker in proportion to the quantity of iodine which it contains.

When exposed to the air the iodine volatilises and the solution becomes decolored.

When a few drops of hypo-nitric acid are poured into a liquid containing an alkaline iodide, and 2 or 3 grms. of benzine added to the mixture; the whole being strongly agitated the benzine quickly rises to the surface of the liquid, its magnificent color being due to the iodine which it takes with it.

This reaction enables us to ascertain, with the greatest ease, the presence of 1 milligramme of iodine in 4 litres of water.

Neither ether, the essences of lavender, lemon, nor turpentine will give, in similar circumstances, such decisive results.

Chloroform, used either according to M. Rabourdin's method or that of M. Grange, will, it is true, clearly show the presence of iodine in many cases; but its sensibility and the color which it acquires are far from being as conclusive as the characters produced by benzine.

In some very carefully conducted exderiments I have been able, by this method, to determine the presence of iodine wherever starch paste indicated a trace, and the employment of benzine has always appeared to me to give even more satisfactory results.

I must add to this, that if benzine will separate from water infinitesimal quantities of iodine, it is likewise very easy to estimate them quantitatively by nitrate of silver, or metallic mercury.

I operate in the following manner:

After washing the iodised benzine several time in distilled water I remove it with a pipette, and I introduce it into a sealed tube, in which I agitate it with a few drops of a solution of nitrate of silver, or of a known weight of mercury until the liquor is entirely decolored.

I wash the yellow precipitate of iodide of silver with alcohol at 33°, I throw it on a filter, and treat it in the same manner as chloride of silver which it is desired to weigh.

In the second case, I agitate the mercury, weighed before hand, in the iodureted solution, and I ascertain the augmentation of the weight. The accuracy of these results may be tested by dissolving the proto-iodide of mercury formed by an excess of iodide of potassium.

Bromine, the bromides with the addition of dilute nitric, hyponitric, or hydrochloric acids do not color benzine; it is the same with chlorine and the chlorides. Bromine and chlorine remain dissolved in the water in which the benzine is washed, and they may be separated from it as a white precipitate by means of nitrate of silver. As benzine possesses the property of removing iodine with dissolving either bromine, it enables us to separate iodine perfectly from those two last-mentioned bodies, and to prove, in a very precise manner, the presence of chlorides or bromides in the iodide of potassium of commerce.

Annales de Chimie et de Physique, December, 1853.

AGRICULTURAL CHEMISTRY.

On the PROPERTY *possessed by* WOOD CHARCOAL *of favoring* GERMINATION. By M. VIOLETTE.

IT was in endeavoring to preserve, in different mixtures, the germs or eyes of potatoes, that I found a remarkable example of the property which wood charcoal possesses of favoring the germination of potatoes.

On the 10th of April, 1849, I detached, by means of a punch, germs in the form of a cylinder, 1 centimetre in diameter, and 2 centimetres long. I immediately introduced,—into a white glass bottle *A*, holding about a pint, and half full of finely pulverised wood charcoal,—100 germs still damp, which I shook briskly so as to cover them completely with the powdered charcoal by a sufficiently prolonged agitation. I filled the bottle completely with the same powdered charcoal, shaking it continually, so as to disseminate the germs throughout the mass; the bottle was then well corked. A similar bottle, *B*, was entirely filled with 100 potato germs in dry peat ashes; a bottle, *C*, contained 100 germs in wood ashes; another, *D*, 100 germs in dry powdered plaster; another, *E*, 100 germs in slaked lime; and the last, *F*, 100 germs in dry sand. These six precisely similar bottles, well corked, were put into a dark closet, through which passed the chimney of a stove, which was kept lighted all the winter, and which kept the temperature at about 59°F.

On the 10th of February, 1850, that is after ten months had elapsed and the closet had never been opened, these bottles were successively examined. The last five bottles exhaled a nauseating odor, and the germs were all completely putrefied. The bottle *A*, filled with wood charcoal, formed the only exception: the charcoal, which had slightly adhered together, left empty in the neck below the cork, an interval of about 3 centimetres in height, in which were a crowd of little white thin stalks, bent where they had touched the cork; moreover, the sides of the bottle were entirely covered with a mat of fine, white roots. It was in vain that I tried to empty the bottle; the charcoal and germs formed a consistent, compact mass, too large to pass through the neck of the bottle, which was at least 3 centimetres in diameter; the bottle having been carefully broken, the mass was uncovered, which still preserved its shape; it required shaking for a long time, to separate the charcoal dust, and with care and patience I succeeded in disentangling and isolating the 100 germs, which were all, without exception, developed in the following manner:—from the germ started a very fine, white stalk of 20 to 25 centimetres in length, along which were lateral fibrils about as coarse as horse hair, to which hung the rudiments of potatoes, under the form of white, round globules, from 2 to 3 millimetres in diameter; six and eight little potatoes might be counted together on one stalk. The upper extremity of the stalk was terminated by a globular enlargement, the rudiment of the part above ground and directed towards the cork; the other extremity terminated in fibrils analogous to roots and directed towards the

bottom of the bottle. These stalks were immediately planted in good earth, vegetated extremely well and produced the usual tubers.

This experiment was made in 1849, and I described it in a sealed packet, deposited in the Academy of Sciences, on the 15th of April, 1850, which I now request the Academy to open. I have decided upon making it known, because I could not continue the analogous experiments which I then intended making, and because the fact appeared to me of a nature to interest physiologists. I regret that it is of less interest to agriculturists; for some thousands of eyes or germs perfectly preserved by the same means through the winter of 1850-51, and planted in March, 1851, in the number of 5 in each hole, shewed an abundant vegetation, but only produced half the ordinary weight of tubers. It may, nevertheless, be possible to restore the fecundity of the germs by a previous granulation with nutritive matters, and thus to turn into food all the pulpy portion of the potatoes which are intended for planting, and which represents 8-10ths of their weight. Nothing would be easier than to remove the eyes of potatoes intended for consumption either by men or animals, and to preserve the germs in a cask filled with pulverised wood charcoal.

Comptes Rendus, December 12th, 1853.

On the Formation *of Ammonia in the* Decomposition *of* Organic Matter.

Ammonia is a compound of hydrogen and nitrogen, which does not exist, *eo nomine*, in organic subjects, either in a living or in a fresh state; but its constituents exist in them, in other combinations; and it is only as organic matter begins to decay or become decomposed, that ammonia is formed or developed. A question then arises: when ammonia is formed or developed in decomposing organic matter, is the presence of both its constituent elements necessary in the decaying substance, to its formation or development? or, if not, which of its two constituents is the one whose presence is necessary, in the decomposing matter, to its formation or development?

I think the tendency of current opinion, so accustomed to regard nitrogen as the useful element in ammonia, would be to maintain that it could not be formed or developed without the presence of nitrogen in the decaying matter; but if the view I have taken in my communication of the 12th November, "On the use and purposes of ammonia and vegetable economy," be founded in truth, it becomes merely the converse of the proposition therein advocated, to affirm that, whenever ammonia is developed in the decomposition of organic matter, it is in virtue of hydrogen being contained in it; and that the presence of nitrogen in the decaying substance is not necessary for that end or purpose. By this, I do not mean to imply that both the constituent elements of ammonia may not exist at times in organic matter, in such proportional quantities as to reconstitute that substance on decomposition: or that ammonia may not be formed by the union of its con-

stituents previously existing in the decaying matter. I merely wish to be understood as affirming that the presence of hydrogen alone is necessary in organic matter, for the development of ammonia; and that, if it be so present, ammonia will necessarily be developed in the process of decomposition, though there be no nitrogen in the decaying substance.* A simple experiment will prove this. Let a heap of vegetable matter, in which there is no appreciable quantity of nitrogen present, be subjected to fermentation or decomposition under the requisite conditions of moisture and temperature. It will be found that in the progress of decomposition a sensible amount of ammonia will be developed, making itself known by its pungent odor. Whence is this ammonia derived? Certainly not from any nitrogen contained in the vegetable substance, since there was none, or next to none, from which it could be formed. But there was hydrogen in it, as there is in all vegetable matter, from which alone, therefore, it could have been derived. If it be asked, whence the nitrogen was obtained? I reply—from the atmosphere, into which the nitrogen was originally discharged, or given out by the plant, when the ammonia absorbed by its roots was decomposed, for the purpose of appropriating the hydrogen in the formation of its structural substance. Of course, if there be any nitrogen present in the decomposing vegetable matter, the hydrogen may as readily unite with it, as far as it will go, in forming ammonia, as with the nitrogen of atmospheric air. But that will make no essential difference in the question; for, unless the nitrogen contained in the vegetable substance be in sufficient abundance to convert the whole of the hydrogen into ammonia, the uncombined or unappropriated portion of hydrogen will seek and find its complement of nitrogen in atmospheric air. It does not pass into the atmosphere in a free or uncombined state; for there are no traces of uncombined hydrogen in the atmosphere; while there are at all times abundant indications of the presence of ammonia in it. Now, as there must be immense quantities of hydrogen being continually evolved from decaying vegetable matter, does not this fact prove the remarkable facility or readiness with which hydrogen unites with the nitrogen of the atmosphere in forming ammonia?

The fact of ammonia being formed from hydrogen, and not from nitrogen, in the decomposition of organic matter, may be shewn by a reference to other facts. Take, for instance, flesh or animal fibre—a highly nitrogenous substance—from the decomposition of which, of all organic matter, there should be abundance of ammonia formed, if nitrogen in organic matter be the source or origin of ammonia. But does the result correspond with the hypothesis? or, are the gases given off by putrid or decomposing animal matter at all indicative of the formation of ammonia? Take, again, the excreta of animals, which being merely the refuse of their food, after supplying the wants of the animal system, must partake of the nature and properties of

* When sulphur, for which hydrogen seems to have a ready affinity, is present in organic matter, sulphuretted hydrogen gas will also be formed.

the substances they eat—are the excreta of strictly carnivorous animals, or even of those whose food is of a mixed or partly vegetable character, as rich in ammonia-producing matter as the excreta of animals whose food is derived entirely from the Vegetable Kingdom? An apparent difficulty, I am aware, is suggested by the properties of guano, which generally contains a very noteable proportion of ammonia. This substance is considered as the excrementitious deposit of sea-birds, deriving their subsistence from the stores of the ocean. But what is our exact knowledge respecting their food? What were the chemical constituents of the fish tribes they are supposed to have subsisted on?—did they abound or not in oleaginous or fatty matter? It is to be borne in mind, that even in the Animal Kingdom there is a vast amount of hydro-carbonaceous matter, in the form of fat, oil, spermaceti, &c., which, when reduced to its constituent elements, of which hydrogen is one, may, in certain circumstances or conditions, be productive of ammonia; in others of water. Thus, if fat or oil be burned in a lamp or candle, the products of combustion are carbonic acid and water, the carbon and hydrogen uniting with the oxygen of the atmosphere. It is important to observe, however, that hydrogen unites with oxygen only at the temperature of ignition. But the result is different when fat or oleaginous matter is decomposed in the body of an animal. In this case, the carbon is taken into the sanguiferous system, and is carried to the lungs, where it unites with the oxygen of the atmosphere, and forms carbonic acid, producing animal heat, which is its designed object. But the hydrogen having no part to perform in the functions of the animal system, is ejected with the excreta, and, meeting with the nitrogen of the atmosphere, is converted into ammonia. If the food then of those sea-birds abounded in oleaginous or hydro-carbonaceous matter, the difficulty of accounting for a large proportion of ammonia in their excreta ceases; and whatever successive assimilations such hydro-carbonaceous matter may have undergone in the bodies of a class of animated beings (fishes) whose existence is maintained by preying on each other, still it must primarily have been derived from vegetable matter, as the food of some living creatures, which in their turn become the food of others.

The bearing of all these considerations tends, I conceive, to the conclusion that ammonia is of vegetable origin; that is, derived from an element inherent in vegetable matter, that element being hydrogen; and this brings me back to the proposition I set out with, that the primary use and purpose of ammonia in vegetable economy, is to supply hydrogen for the formation of vegetable substance; at the same time furnishing whatever nitrogen is found under any form or combination in vegetable productions.—J. H. H.

Agricultural Gazette, January 28th, 1853.

On URINE *as* MANURE. By M. HOFFMANN.

FROM time immemorial it has been known that the excrements of men and animals formed a very efficacious means of augmenting the

fertility of the soil. These excrements restore to the soil those principles which the roots of herbs and grain have removed. But the manure whose composition the most nearly approaches that of wheat is human urine; in this liquid are found all the necessary elements for the formation of wheat; and hitherto all who have used it have obtained very good results and no diminution in the quality of the grain. It is evidently to the ammoniacal salts, to the urea and uric acid, as well as to the nitrogen and phosphates which urine holds in solution, that its efficacy is to be attributed.

The composition of wheat, according to Saussure, is :—

Seed.		Straw.	
Potassa . . .	16·00	Potassa . . .	12·5
Phosphate of potassa .	32·00	Phosphate of potassa .	5·0
Sulphate of potassa .	0·06	Hydrochlorate of potassa.	3·0
Earthy phosphate .	44·05	Sulphate of potassa .	2·0
Silica . . .	00·05	Earthy phosphate . .	6·2
Oxide of iron . .	00·25	Earthy carbonates . .	1·0
Loss	7·59	Silica	61·5
	100·00	Oxide of iron . .	1·0
		Loss	7·8
			100·0

But urine presents some inconveniences; it contains an enormous quantity of water which is useless (952 parts of water in 1000). Several means have been proposed for the purpose of collecting the fixed and volatile principles of the urine alone. These means are :—

1st.—Evaporation by means of heat.

2nd.—Spontaneous evaporation in tanks.

3rd.—Watering stable manure with urine, and allowing it to ferment or evaporate in the open air.

4th.—The precipitation of the salts from the urine by means of certain very cheap agents, such as lime, for instance. We will examine each of these means separately.

Evaporation.—Evaporation by means of heat is too expensive. This means must be abandoned.

Evaporation in covered tanks, or in the open air, is bad for the public health, and infects the neighboring habitations.

Watering Common Manure with Urine.—Watering stable manure with urine is the only means in the least practicable; there is still a loss of some nitrogenous principles, but when the tanks into which the manure is put are made of masonry, so that the aqueous liquor cannot infiltrate the soil, they perfectly fulfil their purpose.

By the fermentation of stable manure, soluble and insoluble salts are formed. The urine, in contact with the manure, renders the latter richer in phosphates, and in ammoniacal and magnesian salts. The silicates, so necessary to corn, are found in abundance in it as well as the salts of potassa, soda, and lime.

We believe that such a manure as this unites all the conditions necessary for the success of all cultivated plants. In fact, the salts

proceeding from urine are found in abundance in it; such as ammoniaco-magnesian phosphate, the alkaline and earthy salts, the chlorides, silicates, &c.

This manure, if the farmer takes pains, will give excellent products. For this purpose it is necessary to construct, at some distance from the house, good stone tanks, making a small canal at a slight incline between the lower part of the tanks, so that the liquid should not always occupy the bottom, but should pass anew through the next tank, the second being lower than the first will fulfil the desired conditions.

After the liquid from the first tank has moistened the manure in the second, the liquid is directed to a separate reservoir, from whence it is taken by means of a pump to be thrown again on the first and second heaps of manure. If there is no pump, the liquid which remains in the reservoir may be rendered solid with plaster, clay, or simple earth.

Thus prepared, this manure unites all the elements necessary either for corn or potatoes, and may even replace guano,* whose price daily rises; that found commonly in commerce is almost always adulterated. On glancing over the analysis of guanos we perceive that the manure which we have just described contains the same substances as guano, although they exist in it in smaller proportions; but when we compare the price of this manure, which the farmer can manufacture himself, with the high price of guano, we see that it would be infinitely more advantageous to him to take the pains to produce a good manure for himself which would cost him so little.

The farmer will soon save the expense if he follows the plan, and constructs the tanks, of which we have given a short sketch. Let us remember that the earth requires to take as manure what she has parted with as harvest; it is only restitution, and it is necessary that this should be in accordance with the nature of the plant which we intend to cultivate.

Manures must, consequently, contain the same elements which constitute the vegetable intended to grow in it.

Precipitation of the Salts from the Urine by means of Lime.—By putting milk of lime in contact with human urine a precipitate is formed: 1000 parts of this precipitate contain: lime, 520; magnesia, 15; phosphoric acid, 465.

Now, by comparing this analysis of the precipitate with the analysis of urine evaporated and incinerated, we find that 1000 parts of the ash of the precipitate obtained from urine, contain: lime and magnesia, 535, and phosphoric acid, 465. The supernatant liquor, therefore, still contains the alkalies, the chlorine, sulphuric acid and organic matters.

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* Commercial guano contains of potassa, soda, and lime, 119·7; magnesia, 7·9; chlorine, 17; sulphuric acid, 36·4; phosphoric acid, 149·2. Guano unites the excremental matters of the urines of animals; this manure gives to cultivated plants the necessary quantities of phosphoric acid, lime and magnesia, as well as the alkalies, chlorine and sulphuric acid. This exclusive property of guano belongs neither to animals nor urine, but to a mixture of these two bodies.

Whereas 1,000 parts of human urine evaporated and incinerated, contain :—

Alkalis	479
Lime and magnesia	34
Chlorine	108
Sulphuric acid	214
Phosphoric acid	165
	1,000

1,000 parts of cow's urine contain :—

Alkalis	573
Lime and magnesia	52
Chlorine	210
Sulphuric acid	105
Phosphoric acid	60
	1,000.

The urine of the horse is less rich in nitrogen than human urine. It only contains 5 per cent. of solid matters, and 0·7 of urea; whereas human urine contains four times as much.

Hog's urine contains a great deal of magnesia and ammonia, as has been frequently observed in urinary calculi.

Therefore it would be still more advantageous for farmers to employ manure obtained according to No. 4. All the elements of urine exist in it. It is not the same with the precipitate obtained by the reaction of lime and urine; for this precipitate contains only lime, magnesia, and phosphoric acid. Nevertheless, in large towns, in which a great quantity of urine is collected in casks, this process may be rendered very useful, and a passable manure may be thus obtained. This process, by its simplicity and the ease with which the precipitate may be dried and transported in a small compass, and under a very convenient form, would be a valuable acquisition to agriculture.

Ashes.—By washing wood ashes the same proportion of silicates of potassa are obtained as with straw; independently of this salt they contain phosphates. Deal ashes contain from 9 to 15 per cent.; those of oak, traces only; and those of the beech, one-fifth of their weight. Ashes constitute an excellent manure, and deserve to be placed in the first rank, especially if we employ the ashes of the same plant which we intend cultivating.

Répertoire de Pharmacie, December, 1853.

Superphosphate of Lime. By C. Lawrence, Cirencester.

There are some farmers so located that they cannot conveniently procure the manufactured superphosphate of lime—others, who could obtain it readily, are fearful of getting a spurious or adulterated article, and many others who would make it themselves if they knew exactly how to set about it. I have seen several directions at various times, and in various publications, but they have lacked precision, and omitted

some point or other in the manipulation, of more or less importance. Having given the matter some attention, I think I may offer the following directions to those who have not had any experience in this preparation. For drilling 5 acres of turnips or swedes, procure 20 bushels of ½-inch bones, crushed in a dry state; this quantity will usually weigh about 800 lbs.; get 200 lbs. of the ordinary sulphuric acid of the manufacturer, which would generally be of the specific gravity 1·840.* Add to this acid an equal weight of water, and stir it in an earthen or wooden vessel, pour this very gently over the bones, as equally distributed as may be, delivering it close over the bones, as any splashing will destroy clothes, and may damage the operator. When the mixture has laid about two days, take it out and form it into a conical heap, and cover it with turf ashes, or such as are commonly prepared on the farm for drilling, in which state it may be left for three or four weeks, to break down more effectually any portions of bone which have not been sufficiently done before. Uncover the heap, and add to the ashes first used as many as will make up the quantity the party usually uses in drilling, whether 20, 30 or more bushels per acre, and then mix those and the bone paste very thoroughly together. Form the mixture again into a heap, and cover it with earth, to remain till within a few days of its being required for drilling, when the heap should be again turned over, and every portion of the bone paste then found should be broken down with the spade or fork, in order that the whole should be completely mixed with the ashes. The acid and bones may be mixed in various ways, but as this is an article required every season, I consider the most efficient and economical, because permanent, receptacle is a brick tank formed thus—dig out the earth in some convenient spot near the homestead, and bottom it and line it with brick (one brick laid flat is sufficient) set in coal ash lime mortar, and pointed in the inside with good fresh Portland cement. For mixing 5 quarters of bones with the acid at a time, I have found a tank 6 feet long, 3 feet wide, and 30 inches deep, a convenient size. None need exceed this size, for the same mixture may be repeated every two days, until any quantity required is prepared.

Agricultural Gazette, February 4th, 1854.

On a Means *of* Preventing *the* Attacks *of the* Oidium *on* Vines.

By Dr. La Sourdette.

The preservative means which I propose is simple, easy, cheap, and has succeeded perfectly in a certain number of experiments which I have made, during two years, in the neighbourhood of Bordeaux.

To prevent and stop the development of the oidium, it is only necessary, about three weeks after pruning the vines, to daub the

* If it be desired to use this manure in a more concentrated state, so that the whole of the bone may be available for the root crop, one part of acid to two parts of bone, by weight, should be employed.

branches with pure liquid tar, which is laid on with a paint brush. This operation costs very little, and has completely succeeded on all the branches of an infected vine to which it was applied.

I am now sufficiently convinced of this to announce these results to the Academy, and to beg that they may be examined, I having the means of renewing my experiments before a committee; not wishing, by any means, to recommend a process whose use had not received the sanction of that body.

Comptes Rendus, December 19th, 1853.

PHYSIOLOGICAL CHEMISTRY.

On the ALTERATION *of the* BLOOD *in* YELLOW FEVER.

By M. CHASSANIOL.

MY duty as surgeon-major requiring me to attend the families of the officers of my regiment, I thought it right, as the yellow fever prevailed very much among our younger soldiers, to follow attentively the different modes of treatment which were used to oppose this scourge. I never saw so many different treatments from the time of the epidemic in 1828, in my first visit to the Antilles, as surgeon to the corvette *Orythie*, until my return in 1853. Having at length selected a mode of medication from among this veritable labyrinth, I then tried to discover how this treatment produced the advantages derived from it.

The following is in brief the point on which I founded my hypothesis:—the signs observed in its course are of such a nature as to be distinctly divided into two periods; one, which may be termed of reaction against the deleterious agents in a latent state in the atmosphere; the other, of solution of the sanguineous fluid by a septic agent imbibed in the economy. Now the medication during this second period is essentially tonic and febrifuge; if we add, that it is impossible not to be antiseptic, we arrive at the explanation of its efficacy taken with our hypothesis as a point of view.

Continuing our train of reasoning we had to ascertain whether the passing from the one stage into the other was not the result of the passage or continuance of a septic agent proceeding from the urinary secretion, for it may always be observed that in the second period of yellow fever, this important secretion is considerably diminished. We then thought of urea, and investigated whether this principle would be found in any notable proportion in the blood; we likewise tried whether it would be absent from the urine.

Having explained this view of the case to my colleague, M. Walter, we asked M. Vardon, pharmacien to the fleet, to join us, and likewise M. Huard, surgeon of the third class to the fleet, who deserves great praise for the zeal and intelligence which he exhibited during our necroscopical researches.

Investigations as to the quantity of urea contained in the urine and blood of subjects attacked with yellow fever. (Made by M. Vardon.)

Urine taken from a body some hours after death.—200 grms. of

urine were evaporated on a sand bath to a syrupy consistence. This mass, redissolved by alcohol and filtered, was likewise evaporated to the consistence of a syrup. The syrupy liquor, treated with nitric acid, gave nitrate of urea, which was collected on a tared filter. After having been washed, it was pressed between sheets of blotting paper, dried and weighed. According to the weight of nitrate, the quantity of urea amounted to 1·90.

This urine contained at the utmost 0·45 per cent. of albumen. Not even a trace of uric acid was denoted.

Blood of the same subject collected at the autopsy.—200 grms. of serum were evaporated to dryness on a sand bath. This mass was then bruised in a mortar, then treated with alcohol, which precipitated almost all the albumen. The alcoholic liquor, separated by filtration from the albuminous coagulum, was heated to the boiling point, filtered to separate a fresh quantity of albumen which a too great quantity of alcohol had retained in solution and finally evaporated to the consistence of a syrup. This syrupy liquor was slightly diluted with alcohol, then subjected to boiling: a fresh quantity of albumen separated from it. This last alcoholic solution, at last freed from albumen, was filtered and evaporated on a sand bath to the consistence of a syrup. This syrup, when cold, was treated with nitric acid, and nitrate of urea was formed. We dissolved it in water and crystallised it. The quantity which we obtained was very appreciable.

With such a result we determined to make further experiments, so as to determine as exactly as possible the quantity of urea contained in the blood, and the sensible diminution of the substance in the urine. Numerous autopsies made by MM. Chassaniol and Huard gave us the averages.

Urine collected from a corpse some hours after death.—15 grms. of urine were analysed and gave us 0·08 of urea, 0·50 of albumen, no trace of uric acid.

Blood taken from the heart of the same subject.—50 grms. of blood gave 0·21 of urea.

Other trials were made, and we obtained nearly the same results,

Finally we made analyses of urine collected during the first period of the complaint and some hours after death.

Eighty grms. of urine during the first period gave,—

Water	76·08
Urea	2·64
Albumen	0·40
Uric acid	0·08
Earthy phosphates, alkaline sulphates, phosphates and chlorides . .	0·80
	80·00

Twenty grms. of urine collected shortly after death gave,—

Urea	traces
Albumen	0·50
Uric acid	not a trace.

We at the same time sought for urea in the blood of the same subject, and in 60 grms. of serum taken from the heart at the autopsy, we found 0·29 of urea.

These results shew how sensibly the urea is diminished in the urine, and how great the quantity becomes in the blood. We even think that the blood which we examined must have contained a larger proportion of urea, and that the quantity which escaped our notice had been taken by the albumen, which, by reason of its coagulation, might have prevented its complete separation.

Comptes Rendus, December 12th, 1853.

On the Influence *of* Menstruation *on the* Milk.

By MM. Becquerel and Vernois.

The return of menstruation during lactation is considered by most obstetricians to be very injurious to the health of the child, and an indication that nursing should be discontinued, or a substitute procured. MM. Becquerel and Vernois have examined the milk of nurses during the presence and absence of the catamenia, and the results of their investigations militate against the commonly received opinions, shewing that the composition of the milk is very little changed during menstruation, and that, so long as the child continues to thrive, there is no necessity for changing the nurse. The following are their analyses :—

	I. When catamenia absent.	II. During menstruation.
Specific gravity . . .	1032·24	1031·58
Water	819·51	881·44
Solids	110·49	118·56
Sugar	43·88	40·49
Casein and extractive matters .	38·69	47·49
Butter	26·54	29·15
Salts in residue . . .	1·38	1·45

The above changes do not materially affect the nutritive qualities of the milk. The authors recommend that the child be put as seldom as possible to the breast during the catamenial period, and that a little sugar and water be given to compensate for the diminished saccharine matters of the milk.

These remarks apply only to menstruation as it occurs at an early period of lactation, and without causing much pain and constitutional disturbance. The secretion of milk becomes generally arrested when menstruation occurs too profusely and of long duration ; and almost with certainty when the catamenia return towards the end of lactation. This latter circumstance, indeed, is an indication for the weaning of the child. Painful menstruation occurring during lactation, may occasionally cause the milk to disagree with the child, inducing colic, vomiting, flatulence, or diarrhœa ; but these effects are very transitory, and the milk soon recovers its normal healthy condition.

[It must be recollected that chemical changes in the milk are not the only possible alterations. Although it is ascertained that the milk is really richer in nutritious material during menstruation occurring in the natural period of lactation, yet it is well known that occasionally, though rarely, the milk at the same time acquires some obnoxious influence which is exerted upon the child. The cause of this we cannot detect by chemical analysis; but the result in the child proves its existence. On the occurrence of menstruation in a nurse, a paroxysm of rage or of grief will often affect the child injuriously (the latter sometimes mortally), without any corresponding discoverable chemical change in the secretion.]

Monthly Journal of Medical Science, February, 1854.

On the Effects *of* Tea *and* Coffee *on the* Animal Economy. By Dr. W. F. Bocker.

Dr. W. F. Böcker, with great perseverance, has been investigating the effects of this beverage on the animal fluids and solids. For this purpose he experimented on himself; living in his usual way, upon food of a uniform character, and drinking alternately, for periods of seven days, water alone, and tea alone. The total amount of the ingesta and egesta for each seven days was carefully noted, and the general results compared and tabulated thus:—

A. Ingesta.	I. Drinking water only.	II. Drinking tea only.
Total weight (in grms.)	3610·50	3617·00
Water	2938·84	2957·87
Solids	671·52	659·13
B. Egesta.		
Loss of weight of body	539	203
Fœces	178·30	96
Perspiration	1349·90	1335·7
Urine	2621·143	2550·000

From this it will be seen, that while drinking tea alone the amount of the excretions was lessened; the daily loss of weight was not so great as when water was drunk, and that the fœces, urine, and perspiration, were all diminished.

Arch. für Wissensch. Heilk. 1, 2, 1853.

The Nutritive Power *of* Aliments.

The nutritive power of different aliments is directly in proportion to the quantity they contain of the elements of the blood, in a state capable of assimilation. The latter clause is inserted by Dr. Molleschott especially to exclude the valuation of food solely in the ratio of the quantity of nitrogen it contains. This proposal would class perfectly insoluble fibrin, coagulated vegetable albumen, &c., among nutritive substances, besides supposing that the body is increased by

thein, caffein, and other similar principles, of which there is no evidence; except, indeed, what is afforded by the resemblance of their chemical formula to that of creatinine, and some of the uninvestigated animal alkaloids. Nutritiveness, then, depends in no slight degree on digestibility, so far our investigations on this head will receive help from a consideration of the mechanical conditions of the several component parts of the alimentary body. While looking to the more obviously required constituents of food, it must not be forgotten that blood, and that which is to be formed of blood, contains also inorganic mineral elements. Soda, and potassa, and chlorides, are required, and the food must possess these, or it cannot be said to be nourishing. Now, the researches of Liebig (quoted in Lehmann's "Phys. Chemie," part iii. p. 91) shew a very different proportion of sodium and potassium in the extracts of the flesh of different animals. A pursuit of similar researches into the comparative amount of phosphate of lime, mentioned in the same place as being carried on by Schlossberger and Von Bibra, may lead to valuable results. The deficiency of this salt in the flesh of young animals was observed by Liebig and Berzelius, and may perhaps help to account for the inferiority of its nourishing power, which has long been empirically suspected.

The proper proportions in which the elementary principles of food should be combined cannot be better estimated than by observing what they are, in that typical aliment which the Creator has prepared for us before reason makes us cooks, and when the "whole duty of man" consists in growing big and strong. The proportions in milk are, according to Dr. Lehmann, ten parts of plastic material, ten parts of fat, twenty parts of sugar, and 0·6 of salts. But then we must remember that this golden age has an end, that other functions and other duties arise; the outer world makes its demands, mental and bodily deeds have to be done, and therefore (have we not a right to say, *therefore ?*) a different expenditure and a different income must be calculated on. This is obvious enough, but as to what the differences are we are completely in the dark. We cannot even guess why different young animals should be supplied, as they are, with different fare—why the calf should be allowed so little sugar and so much fat and casein (unless, indeed, we count butter and cheese dairies as part of the final causes of cows)—why the "poor little foal of an oppressed race" should be treated to as much sugar as his rider, and much more fat. Why should kids and lambs enjoy the sweets which puppies are denied ?

Brit. and For. Med.-Chir. Review and Monthly Journ. Med. Science.

INDUSTRIAL CHEMISTRY.

On Acid Sulphate *of* Soda *and* Tartaric Acid *in the extemporaneous preparation of* Effervescing Waters. By M. Dorvault.

When the price of a product exceeds a certain limit, the ordinary consequence is an endeavor to find a substitute for it.

The price of tartaric acid having become more than double during the last few months, and threatening to rise yet higher, it is time that we should try whether some other product might not replace it for some purposes. Such is the aim of this note.

Two causes have concurred to raise the price of tartaric acid. First, the enormous quantity now consumed in dying, and in the factories for the preparation of effervescing drinks, by means of aerating apparatus. Secondly, the failure of the vine crops has produced a great want of tartar.

For the extemporaneous preparation of effervescing drinks by means of the abovementioned apparatus, in which its use augments annually, we think that a substitute for tartaric acid will easily be found. The proposition which we now lay before the public, will contribute to this.

If sulphuric acid were not inconvenient in its form and dangerous to use, its low price would perfectly decide the question. These two peculiarities oblige us to reject it. But if, instead of taking it with its ordinary physical qualities, we solidify it in some way by combining it with some oppropriate salt, we approach, or might even attain, the desired end.

The use of the alkaline bisulphates has already been proposed for this purpose; but it was at a less opportune moment than the present; perhaps, too, it was not sufficiently persevered in; and, besides, the bisulphates proposed might not have been free from drawbacks, therefore the proposition fell to the ground.

The bisulphate of potassa was proposed. It had the defect of being deliquescent, besides its preparation was difficult.

The bisulphate of alumina, which was next tried, did not succeed, because, in the presence of bicarbonate of soda, it was liable to disengage its alumina, and thus to prevent the free disengagement of the carbonic acid.

The bisulphate of lime, which was likewise thought of, forms badly, and gives an abundant precipitate, which renders the apparatus dirty; otherwise this salt may be obtained very cheap.

As far as we know no one has tried sulphate of soda with the addition of sulphuric acid. We have made many investigations on this subject, and we have obtained very satisfactory results. We have found that sulphuric acid combines with sulphate of soda with remarkable facility, and in considerable proportion (up to 100 per cent. or even more). The operation is rapid, and the product flows into solid semi-opaque plates, sufficiently friable and not deliquescent, except in a very moist atmosphere.

The following is the *modus operandi*:—

Crystallised sulphate of soda	1003
Sulphuric acid at 66°	550

The whole is placed in an enamelled cast-iron pot, and it is heated until a small portion of the matter, thrown on a cold surface, becomes a mass on cooling; it is then poured on earthenware plates, allowed to cool, and preserved for use.

By reason of the slight deliquescence of this product, it must be preserved in covered pots or stoppered bottles. In this state it may be kept without any alteration.

It is therefore better for the purpose of which we have spoken, the preparation of effervescing waters, to deliver it only in closed vessels and in coarse powder, in the same manner as is now generally done with tartaric acid, and not in paper parcels. Small measures of lead, earthenware, or glass, of different sizes, in accordance with the size of the apparatus, will serve to measure the quantities required.

As we have said, sulphuric acid unites with sulphate of soda in all proportions, the products resemble each other in their physical properties; but the proportions which we have fixed upon are such, that a given weight of this product will decompose an equal weight of bicarbonate of soda.

Besides the advantages presented by such a compound, or rather such a mixture, for we are not speaking of any definite salt, there is another of equal importance, its solubility in water, which is very nearly equal to that of tartaric acid, so that the manner of using this product differs in no respect from that of this acid. There is the same harmlessness in its use, the same time required for the production of the gas, the same stability in the nature of its products.

The low price at which it can be sold is not without importance, and deserves the greater observation, as the cheaper the product the more its use augments. Now with the acid sulphate of soda, the price of effervescing waters might be diminished one half. It is possible, in fact, to sell it to pharmaceutists at one franc the kilogramme.

From these considerations we conclude:—

That the acid sulphate of soda may be substituted for tartaric acid in the preparation of effervescing waters by means of apparatus.

That this product has all the advantages of tartaric acid; and that its price being so much less, it could completely replace it, and augment the consumption of aerated waters, the employment of which is of importance both to therapeutics and public health.

Répertoire de Pharmacie, December, 1853.

THERAPEUTICS.

On Iodide *of* Potassium *as an* Antidote *to the Injurious Effects of* Mercury. By Dr. Corson.

The good effects of iodide of potassium in those cases which suffer from the injurious consequences of a mercurial course have been long recognised, but M. Melsens, of Paris, was the first to shew, by an extensive series of chemical and clinical experiments, the mode of action of the remedy. His observations were published not long since in the "Annales de Chimie et de Physique," and have been translated by Dr. Budd, of Bristol, in the "British and Foreign Medico-Chirurgical Review," for January, 1853.

M. Melsens lays it down as an admitted principle, that mercury as

well as lead combines with the animal tissues, and remains fixed in the system for years. This is proved by the well-known fact that persons, who have once freely taken mercury, years afterwards find gold coins discolored by the mercury in their perspiration; also that mercury has been sometimes detected in the body after death. 2ndly, that in the body, as well as out of it, iodide of potassium acts as a powerful solvent of the compounds of mercury and lead, disengages them readily from the animal tissues, and drains them off through the kidneys. M. Melsens first proved that the iodide of potassium passes off principally in the urine, by taking large quantities himself, and then analysing the different secretions of the body; he found that the urine was loaded with it, while the fæces scarcely contained a trace. It passes off with great rapidity; thus a person took seventy-seven grains and in a few minutes the urine was charged with it. He then found that the compounds of the iodide of potassium with mercury and lead pass off by the kidneys in the same way. An extraordinary cure of mercurial paralysis in a looking-glass maker is given, in which the iodide of potassium in very large doses was taken for several months, and repeatedly the iodide of mercury was detected in the urine.

The great efficacy of iodide of potassium was an antidote to the slow poison of mercury and lead, M. Melsens proved by experiments on dogs, which were fed with the carbonate and sulphate of lead till paralysed, emaciated, and nearly dead, and then in a short time restored to health and flesh by the administration of iodide of potassium. He also cured three cases of severe lead paralysis among house-painters and workers in lead; and greatly relieved a fourth by the same remedy. In five cases of mercurial paralysis among gilders and workers in quicksilver, the iodide of potassium accomplished great relief or perfect cure in a few weeks.

As to the doses in mercurial or lead poisoning, M. Melsens recommends that we should begin with fifteen grains of the iodide in solution three times a day, and increase it as the patient will bear it. Such large doses, according to Dr. Budd, require to be given on an empty stomach, and largely diluted. In milder cases, where mercurial paralysis is not induced, and the system is not highly charged with the noxious mineral, Dr. Corson's cases seem to prove that smaller doses, if continued sufficiently long, are highly efficacious. The quantity that may be safely borne is often immense. Thus, M. Melsens took from half a drachm to a drachm and a half daily for two months, two thousand grains in all, without any inconvenience beyond temporary coryza, and the eruption of a few pimples; and with a decided increase of appetite. One of his worst cases of mercurial paralysis took 2314 grains in three months, and one of Dr. Corson's cases had five grains three times a day for eleven months with the greatest benefit.

In some cases the poison seemed to be liberated so rapidly by the remedy that it was badly borne. Sometimes profuse salivation was the consequence; and Dr. Corson believes that the iodide of potassium never salivated, except by liberating mercury. In one case, a gilder, aged sixty-five, suffering from mercurial tremors and paralysis,

took eight grains three times a day; but such distressing ptyalism was produced, that he refused to continue the remedy. Dr. Corson is in the habit of neutralising, as he imagines, the too severe effects of mercury in syphilitic and scrofulous throat affections, by combining it with the iodide of potassium; and in strumous syphilitic cases he gives blue pill at night, and the iodide by day; he also suggests the exhibition of iodide of potassium as a protection to painters and workers in lead in seasons of special exposure.

It was in the hope of directing more attention to M. Melsen's observations that Dr. Corson published, in confirmation of them, five cases, selected from many others. Four of these had legitimate evidence of scrofulous, or tuberculous taint, and the fifth suffered from Bright's disease; in all, therefore, from the constitutional state, mercury was contra-indicated; yet he says mercury was the only remedy capable of meeting the exigency of the cases. Extensive dropsy, with hepatic congestion, threatening obstructive inflammation of the throat, immense pleuritic effusion, suffocative laryngitis, and severe puerperal peritonitis, were the formidable maladies he had to combat; and he believes that the singular counteracting agency of the iodide of potassium enabled him to give mercury to salivation with perfect safety, notwithstanding the constitutional contra-indications. Mercury was given till ptyalism was freely produced; iodide of potassium was then administered, and no wandering pains, cachectic appearance, or other signs of injury, from the constitutional effects of mercury, ever appeared, although some of the cases were watched for years.

New York Journal of Medicine and Monthly Journ. Med. Science.

PHARMACY.

On the CINCHONAS *and those Questions which, in the Present State of Science and Commerce, are more Particularly Connected with them.*

By MM. DELONDRE and BOUCHARDAT.

BEFORE publishing our last article on twenty barks, of which we have still to give the description and analysis,* M. Delondre wishes to take a retrospective view of the causes which determined his voyage to the South Seas, and which are connected with the history of the Cinchonas.

After the unfortunate results of my expedition in 1828 to the forests of Bolivia, I was always convinced that I should have succeeded better if I had been able to conduct it myself; and for twenty years I have never thought of that journey without a smile.

When the government of Bolivia stopped the general exportation from the forests of the Republic, I perceived that we should be under the yoke of the Monopoly, and I resolved to free my country from its effects, either by becoming shareholders in the company or by seeking, in the forests of the New World, other cinchonas which should enable us to meet all rivals, and to retain that superiority in this manufacture which seemed likely to be taken from us.

* This Memoir will appear next month.

My partner, M. Levaillant, who had been my pupil, and who had surpassed me in all respects, was the only person to whom I confided the secret of my plan.

I took with me every necessary means of preparing, in America, extracts from the barks of those cinchonas whose comparative poverty in alkaloïd would not admit of their exportation in the natural state, and I embarked at Bordeaux on the 3rd of October, 1846.

We landed at Rio Janeiro, and reached Valparaiso on the 5th of February, 1847.

On my arrival at Valparaiso, I met with M. Pinto, head of the Bolivian Company, and I failed in every endeavor which I made to procure regular consignments from him. I tried in vain to induce him to wait for the fresh powers which I expected to receive from France; M. Pinto preferred the offers that had been made to him by a house in the United States, and for several years all the Bolivian cinchonas were sent to New York. Immediately after this disappointment, I received the sad news of the death of M. Levaillant, my partner; this unexpected misfortune embarrassed me extremely, as his support and advice were indispensable for the success of my enterprise.

It was, however, necessary to overcome my grief and anxiety, and to employ myself with my manufacture, which I organised after immense trouble, often being obliged to act myself as working blacksmith and mechanic. At length I prepared to fulfil the first object of the excursion and proceed into the forests of Peru.

At this time, the end of April, 1847, M. Vinueza, of Cuzco, came to me, and gave me some samples of a very good cinchona of which I was going in search, and entered into an engagement to deliver to me, at Valparaiso, 100 serons per month, commencing with the close of the ensuing May. This consignment, and those which had been promised me from Lima, produced by the forests of Huanuco and Loxa, seemed sufficient for my regular operations, both for my manufactory at Valparaiso, and for my exportation to France in the natural state

But after waiting with patience until the end of June without receiving any tidings of M. Vinueza, I decided on losing no more time, but to go and judge for myself of the state of the forests.

I embarked on the 1st of July, and on the 6th reached the little port of Islaï. The moment I landed, I hired mules and a guide to cross the desert, and reached Arequipa the next evening.

To shew the first difficulties of this excursion, it will be sufficient for me to transcribe a passage from the expedition of M. Castelnau, who had crossed this same desert a few months before.

"It is difficult to give the reader an idea of the extreme barrenness of the coast of Peru; it can only be compared to the great African deserts; no trace of vegetation is perceptible, and the view is impeded only by moving sand hills, to which has been given the name of *medanos*. They are due to the action of the incessant winds which reign in this region; it is to this origin that they owe the crescent form which they almost always take. It is only with experienced guides

that any one can venture across these deserts; for when the tempests move these sandy masses, they form clouds of sand thirty and forty metres high, which would swallow up any traveller who was unaccustomed to the country. The *medanos* of which we speak usually attain a height of six to eight metres; and it is asserted that when driven by a high wind, they go across the plain with great rapidity. Moreover, nothing is more uncertain than the formation of these sand hills; a region which is covered with them over night, may by morning present a flat, unbroken surface. The want of water is severely felt by travellers. During the war of independence, entire regiments were lost, and found a frightful death under these mountains of sand.

"The post route, which we followed, was indicated by a border of stones on each side; after travelling for fifteen leagues, rendered very fatiguing by the heat of the sun, we reached a *tambo*, or sort of little inn, built of planks in the midst of the desert. An old French soldier was at the head of this establishment, which had been constructed for the purpose of offering a shelter to those travellers who, a few months previously, had been obliged to travel thirty leagues in a day. But the principal profit made by him was produced by the sale of water, which he brought from a distance of four or five leagues, and sold at thirty centimes the glass. When thinking of the delight with which we rested ourselves in this spot, I cannot regret the twenty francs which we expended on the water which was necessary for us and our animals."

I received the most cordial hospitality from M. Braillard, partner and director of the house of Viollier and Co. Thanks to his good advice I arranged my journey into the forests of Cuzco, so as to see whether I might reckon on the cinchonas promised by M. Vinueza, or if I had better enter myself into this speculation.

After two indispensable days of rest, I quitted Arequipa and the volcano at the foot of which this town is built, to enter into the Cordilleras, and arrived on the eleventh day at Cuzco, after unheard of fatigue and privations of all kinds. I only speak from the memory of the dangers inseparable from such a journey, without distinct roads; suffering in the middle of the day from the intense heat of the sun, hardly able to breathe at a height of 5,000 metres above the level of the sea, and at night exposed to a freezing cold, lying on the ground in a miserable Indian hut, on my own little travelling mattrass.

However, I arrived without accident at the ancient capital of the Incas, and I hastened instantly to the house of M. Santo Domingo, partner of M. Vinueza, where I heard that the latter had gone to the forest thirty days before with a considerable number of workmen; but that no news had been received from him, and there was reason to fear that they had been massacred by a tribe of Indians who inhabited the neighborhood of the forests. On the other hand, M. Santo Domingo was at a silver mine belonging to him, which caused him much anxiety, and his return might be expected in a few days.

This first news was confirmed by a French merchant, named Romain-

ville, whose memory will always be dear to me, who told me that if M. Vinueza were lost I must not depend upon M. Santo Domingo for cinchona, for his mines and other unsuccessful enterprises had ruined him. The good Romainville offered me hospitality and his assistance in realising my project of investigating the forests. My decision was soon made; I begged M. Santo Domingo's clerk to procure me mules and a guide, to find the latter at the mine, and thus to learn exactly what I had to expect, before making any arrangement with Romainville. The clerk preferred going himself, acknowledging that the affairs of his principal were in a very embarrassed state, and that my unexpected visit would add to his annoyances; two days afterwards he informed me of the suicide of M. Santo Domingo, who, for a month previously, had written each evening his miserable reflections upon the day, the causes of his fatal determination, and had fixed the very day on which he put it into execution.

In this part of his journal I could not avoid thinking of Sir Walter Scott's "Antiquary," only here there was no romance, nothing but a frightful reality.

An adventurer, a countryman of M. Santo Domingo, knowing his speculative mind and his great resources, led him into the purchase of a silver mine, which had been abandoned for a long time, but which, he said, was of incredible richness, which had cost him a great sum, but which he could not work for want of sufficient means. Santo Domingo hastened to the mine, where he found a skilful foreman who made some experiments, and produced, as a result, a magnificent ingot of silver. M. Santo Domingo hastened to his friends, procured a quantity of quicksilver, made all the necessary advances, spared no expense to ensure the success of the enterprise, and then established himself at the mine to superintend the works.

But after a few days he became convinced that he had been deceived, and that the foreman, charged with the fusion of the ore, was only an accomplice of the other swindler.

It was then that M. Santo Domingo expressed in his journal the determination of killing the two scoundrels who had made him their victim, and then killing himself, if at the end of thirty days the results of the work did not cover the current expenses. The pistols were beside him night and day, and he never lay down at night until he had written his reflections and drunk a bottle of brandy, doubtless to intoxicate himself, and thus temporarily to forget his griefs.

The new Dowsterswivel mistrusted the taciturnity of M. Santo Domingo and the mysterious measures which he saw him take, and at the end of twenty days he escaped his vigilance and disappeared without leaving any clue, carrying off all the money which he had got from M. Santo Domingo.

But the accomplice, who was obliged to direct the laborers in working the ore, was less on his guard, or perhaps better watched by Santo Domingo, who one morning caused his mules to be saddled, dressed himself as if for a journey, and calling the foreman desired an account of his operations; the man brought the books hesitatingly, for he

knew what poor results he had to shew. M. Santo Domingo remained standing behind him, and cocking his pistol put it close to his head, when, the man turning suddenly, the direction of the ball was changed, and it only grazed his shoulder. Being a powerful man he seized M. Santo Domingo and pushed him into an adjoining room, of which he fastened the door, while calling for help; the workmen only arrived in time to hear a second explosion: it was M. Santo Domingo who had put an end to his earthly troubles.

The fatigues of the journey and these terrible tidings overwhelmed me, as may be imagined, for it had been impossible for me to take the rest I so much needed. M. Romainville took me home with him, and the next day erysipelas attacked me in the head, and I became delirious. The energetic measures of Dr. Nateri, and the kind nursing of M. Romainville and his wife, restored me to health in six days, and on the eighth I remounted my mule, to undertake at length, in the mountains of Santa Anna, the excursion, the preparations for which had been stopped when I was confined to my bed.

In the midst of one of my conferences with my intended fellow-travellers I saw Dr. Weddell enter my chamber; he had formed part of M. Castelnau's expedition, and had separated from him near the frontiers of Paraguay, to seek alone for chichonas in the forests of Bolivia. My surprise was very great, and his was not less, when we explained to each other the purpose of our journeys.

We then determined to travel together, with MM. Garmendia and Galdos, who were to work the forests on my account, and whose expeditions were to be made under the direction of M. Romainville, who also intended accompanying us. The next day we started for the forest, and then we employed an Indian as a guide, who cleared the path before us by striking down the branches of trees and bindweed which impeded our passage.

After a most fatiguing journey, overcoming a thousand obstacles and exposed to a fine rain, which wetted us to the skin, we heard the blows of the Indian's axe, who had reached the top of the mountain before us, for we were almost unable to move from fatigue.

But the blows of the axe, which proved our success, renewed our strength as if by enchantment, and we were soon by the side of this magnificent tree, which I now saw for the first time, and which had been for so long the subject of my dreams. I stood in ecstasy before its beautiful silvery bark, its large leaves of brilliant green, and its flowers, whose sweet perfume reminds one in some measure of the lilac.

The tree did not fall at once, it remained suspended among the twining plants and the branches of the neighboring trees, and they were obliged to be broken down for some distance to allow our much desired conquest to be extended on the ground; and thus we could admire it at our ease, cut the bark off the trunk and branches, taste the leaves, the flowers, and the fruits, to ascertain their various degrees of bitterness. While descending the mountain, I could not help reflecting on the indifference with which the Indians cut down the tree

at a certain height from the ground, so as not to have to stoop; they thus leave the trunk and lower branches; in fact I have calculated that they must waste half the bark which each tree ought to produce.

After three more days in the forest, I fixed my conditions and chose the cinchona which was to be worked and prepared for my return.

I embarked at Islaï and returned to Valparaiso; the following month I received the news of the death of M. Romainville: another grief to add to so many others! I likewise had advices from Paris, which led me to anticipate great difficulties on account of my consignments of cinchonas and extracts. I hastened to prepare the extracts of those barks which were the least productive of alkoloids; and having collected those cinchonas which I thought of better quality, I embarked again in the month of March, 1848, with a very considerable value of goods, and arrived at Havre on the 23rd June, where such bad news and great anxiety awaited me.

Now, in my sixty-fourth year, I have again mounted the breach to defend the manufacture to which I have devoted all my resources and faculties, endeavoring to render my long experience useful. I shall be quite happy if I succeed.—*Répertoire de Pharmacie*, Dec. 1853.

PATENT.

TO THE EDITORS OF "THE CHEMIST."

"32, Moorgate-street, City, Feb. 8th, 1854.

"GENTLEMEN,—Mr. Herring desiring to see his patent correctly reported, I take the liberty of forwarding you the enclosed copy from his Specifications. Its insertion will oblige the Patentee, and, Gentlemen,

"Your obedient Servant,

"H. DIRCKS."

HERRING'S PATENT SULPHATE OF QUININE.

The following is the specification of Edward Herring, of Southwark, Manufacturing Chemist, for *Improvements in the Manufacture of Sulphate of Quinine*. Sealed 28th July, 1853:—

MY said improvements relate to certain processes for the complete extraction of quinine from Peruvian bark and other suitable barks in the form of sulphate of quinine; and without the aid of alcohol.

I will now proceed to describe my processes, under three heads, as follows:—

1. In my *First* process, the bark in a powdered state is boiled in a solution of caustic soda or other alkali, which removes the useless extractive and gummy coloring matters. After well boiling the bark, it is to be pressed, and returned to a tub, to be therein agitated with cold water, which operation is repeated until the coloring matter is exhausted. I next proceed to other boilings, as follows:—In the first boiling, the decolorised bark, after the foregoing process, is boiled in sulphuric acid and water, in due proportions, and kept agitated by suitable mechanical means; and this hot solution of sulphate of quinine is then run off into an evaporating pan, as customary. The remaining bark is now boiled a second time, in weak sulphuric acid and water, and run off as before. The bark may be boiled a third time, or oftener, in sulphuric acid and water; but as each time the liquid will be less charged with the sulphates, these after-boilings may be reserved for each succeeding portion of fresh bark, decolorised by the *First*, or alkaline process.

I would here observe, that in my *First* process I prefer using caustic soda; and

that the first and second acid boilings are to be mixed and evaporated at a temperature of 120° F., or thereabouts, in a large water bath, until sufficiently concentrated, and filtered when cool to separate the flocculent coloring matter which then deposits, and which may be treated with dilute sulphuric acid until exhausted. The filtered cold solution of the first and second concentrated boilings, before named, and also the washings of the flocculent coloring are to be neutralised by excess of caustic alkali, and the precipitated alkaloïd drained, washed, and pressed. This last process is then treated with dilute sulphuric acid, a gentle heat applied, and the resulting impure sulphates of quinine, quinidine, and cinchonine, crystallised into a thick mass, which when cold is to be pressed, washed, and again pressed. The pressed cakes thus obtained are next to be dissolved in a large quantity of water, and crystallised as usual. These crystals will be sufficiently pure for chemical purposes, and afford the unbleached or hospital sulphate of quinine.

2. In the *Second*, or bleaching process, the foregoing sulphate of quinine is subjected to one or two further crystallisations, to produce the pure white article of commerce; and for this purpose the solutions are boiled with a due admixture of pure animal charcoal to avoid contamination with the free salts of ordinary animal charcoal. The crystals obtained are to be drained and dried at a low temperature, when they will form the snow-white article of commerce, or the bleached sulphate of quinine.

3. In my *Third* process, the blood-red products of the bark boiled in the caustic soda solution, or other caustic alkaline solutions, as described in the *First* process, are to be treated with hydrochloric acid in excess, to retain the alkaloïds in solution, which may have been dissolved by the first soda or other alkaline boilings and washings. These being mixed are evaporated, and then filtered to separate a quantity of coloring matter; the filtered solution is then to be precipitated by excess of lime, forming muriate of lime, and muriate of soda, or other alkali in solution; while the alkaloïds and free lime are precipitated, and may be washed, filtered, pressed, dried and powdered. The calcareous precipitate is then ready to be treated by benzole, or any other solvent of the alkaloïds, that is not a solvent of lime. These various tinctures or preparations are treated in the manner following: if by benzole, or turpentine, or lard, the tinctures or preparations are mixed with cold dilute sulphuric acid and water, and well agitated; the benzole, turpentine, or lard, floats, while the water charged with the sulphuric acid sinks; the acidulated water having now extracted the alkaloïds from the benzole, turpentine, or other tincture or preparation, this acid solution is syphoned off into a convenient vessel, and is precipitated by caustic soda, and the crude alkaloïds are obtained without the application of heat. And herein the advantage of my improved processes consists, rendering such crystals less amorphous than they otherwise would be by the customary alcohol process, which does require heat. If the calcareous precipitate has been treated by alcohol, ether, naphtha, or any spirit solvent, the tinctures are to be submitted to distillation, and the crude alkaloïds will be thereby obtained, but this is subject to the objection before named, of the requisite heat rendering the products more amorphous. The crude alkaloïds obtained by either process are to be treated with dilute sulphuric acid, by gentle heat, and the impure sulphates crystallised into a mass. These crystals are then pressed, washed, and again pressed, and are subjected to two or more crystallisations in order effectually to remove all trace of impurity; and the product will then correspond in quality, though considerably less in quantity, to that obtained in the first product of my alkali process, by which the sulphate of quinine obtained forms 80 to 90 per cent. of the required product, according to the quality of the bark, the remaining 10 to 20 per cent. being recovered by my Third process, before described.

The Patentee states, that what he claims as his invention, is—

The peculiar processes of manufacturing the sulphate of quinine, without the employment of alcohol, as described.

Filed, 28th January, 1854.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the FORMATION *and* CHEMICAL COMPOSITION *of* RED CINCHONIC.*

By M. A. F. GUIRAUD of Chalons-sur-Soane.

(Communicated by Mr. Duchesne.)

THE author of the paper I am about to submit to your notice, has for some time past, directed his attention to the various chemical preparations of cinchona bark. While engaged in the laboratory he happened to let fall a small quantity of caustic potassa into a bottle of tincture of cinchona. A dense precipitate was immediately thrown down. Imagining this to be owing to a simple displacement of the kinic acid, which by combining with the potassa, had liberated the quinine and cinchonine, he collected the precipitate, dried it, and submitted it to the action of alcohol and ether. Finding, to his great astonishment, that neither of these agents had any effect on the black, curdy, gum-resinous looking mass, he tried it with distilled water, supposing then that it might have been produced by the combination of the red cinchonic with the potassa. In this it was entirely dissolved, giving an intensely red solution, which, on the addition of a few drops of dilute sulphuric acid, changed to a yellow. The *proto*salts of iron produced no change, but as soon as by the action of the atmosphere they had become *per*salts a green color was formed, and a precipitate of tannate of iron gradually settled at the bottom of the glass. Gelatine gave a glutinous precipitate. The presence of tannin being thus sufficiently indicated, there remained to be ascertained what had become of the red coloring matter. With this view he proceeded as follows: taking a portion of the extract of cinchona prepared with proof spirit he calcined it in a platinum crucible, and dissolving the whitish ash which remained in weak acid, was enabled to determine the presence of potassa, magnesia and lime. It still remained a question whether these were combined with the red cinchonic or were merely alterative agents. This he endeavored to discover by washing the alcoholic extract of cinchona with distilled water till it passed off colorless and collecting the red cinchonic in a filter. This he dried and calcined. In accordance with his anticipations the potassa and magnesia had dis-

* Read before the "Chemical Discussion Society," January 12th, 1854.

appeared, leaving only the lime and red cinchonic; he considers it then to be merely tannin, modified by the caustic alkalies, soluble when the modification is slight, insoluble when it has become more complete. Obtained by levigating the extract, it is deprived of its bitter taste and a portion of its astringency. The water used in the process dissolves the soluble tannate (called soluble red cinchonic) as well as the alkaloïds. From the foregoing experiment he derives the conclusion, that if the insoluble matter alone be properly considered red cinchonic, it is not combined with the quinine and cinchonine, as certain chemists have advanced, but that these alkaloïds are in the bark, combined with the soluble tannate, and more particularly with the kinic acid. He considers this to be rendered less doubtful by the fact, that the insoluble red coloring matter may be easily separated without affecting the alkaloïds. In its combination with the caustic alkalies it afforded the same reactions as ordinary tannin. Dissolved in weak spirit and boiled in an open vessel it became altered, and presented the appearance of a resinous body, from which peculiar transformation some pharmaceutists have erroneously imagined the pre-existence of the resin in the cinchona bark. Submitted to a heat of 302° F. it gave off violet colored vapors and a peculiar aromatic odor. By distillation it afforded pyrogallic acid, an empyreumatic oil, a volatile aromatic substance of the color of carmine, soluble in alcohol, ether, and the caustic alkalies, but insoluble in water. It was insoluble in water and ether, soluble in alcohol and the caustic alkalies. Its chemical composition is as follows: $C^{14}H^{8}O^{6}$, with traces of iron and lime. He attempted to produce it artificially by the action of caustic potassa, lime, and tannin; and found that in proportion as the maceration was continued for a greater or less space of time, so was the alteration of the tannin more or less complete. Its chemical properties approached very closely to those of the true red cinchonic, while its physical characters were very dissimilar. It was equally soluble in alcohol and ether, insoluble in water, and gave the same reaction with the persalt of iron.

The coloring matter of wine, oak, rhatany, tormentilla, and generally of the bark of such trees as are indigenous to warm climates, afforded very nearly the same results; from which he draws the conclusion, that red cinchonic is not a substance *sui generis*, but existing wherever tannin may be found. Besides being richer in tannin, the bark of trees from the centre of Africa and America possessed the peculiar aromatic odor before mentioned, which is wanting in that procured at home. The tincture which had yielded the red cinchonic, when distilled, gave an extract. As the distillation had been only partial, he found, on decanting the extractive matter, a resinous substance adhering to the vessel. It had a bitter herbaceous taste, and an odor similar to that of bark. It was insoluble in water, but dissolved readily in dilute sulphuric acid, a fatty substance floating on the surface of the solution. This was separated by filtration, the liquor was decolorised with purified animal charcoal, and by evaporation crystals of sulphate of quinine and cinchonine were procured. On repeating his expe-

riments, 4 oz. of yellow bark yielded him 52½ grains of sulphate of quinine, and 7½ grains of sulphate of cinchonine.

SANITARY NOTES.

THE following extracts from the Quarterly Return of the Registrar-General as to the meteorology of the last quarter of 1853, are deserving of much consideration in connection with the state of the public health.

"The temperature till 20th of October was 1·8° below its average; in the period from 21st of October to 8th of November it was 5·3° above; and from 9th of November to the end of the year it was 4·8° below the average.

"The reading of the barometer was low in October; it was very high in November. The excess of reading in November over that in October was nearly 4–10ths of an inch in all places; it decreased by December in England, but still farther increased in Scotland.

"The fall of rain was 1–3rd above its average in October, and fell short of the average in November and December except in Cornwall and Devonshire. The general deficiency for the quarter is about one inch.

"The direction of the wind has generally been a compound of North and East, except in the interval from 21st of October to the 8th of November, when it was mostly South-west.

"The air has been drier than usual, particularly in December, in which month the difference of air and dew-point temperature, notwithstanding the low value of the former was greater than usual, consequently the degree of humidity was low.

"The mean temperature of the air at Greenwich for the three months ending November, constituting the three Autumn months, was 49·4°, being 0·1° above the average of 80 years.

The state of the public health is thus expressed :—

"The period was unhealthy . . . . The annual mortality has been at the rate of 2·252 per cent. in the ten years, 1843—52; it was 2·186 in the last quarters of those years; and last quarter it was 2·272 per cent.

"In 117 districts, comprising the chief towns, the rate of mortality per annum was 2·778 to 100 inhabitants; the annual mortality in 10 Autumn quarters (1843—1852) was 2·634. In 507 districts, consisting chiefly of small towns and country parishes, the mortality was 1·911; the average was 1·965.

"The variations of the temperature are already shewing what we are to expect from a prevalence of warm weather. During the week, March 5—11, the mean temperature varied from 6° below the average on Monday, to 14·3° above the average on Thursday in London; and during the same week the mortality was 1343, being 152 above the average of 10 years."

In Glasgow, at Kanturk in Ireland, and at Leeds, the cholera has made its dreaded re-appearance; and there is no reason to sup-

pose but that its summer attack will be certain and violent. And are the large towns better prepared to resist the approaches of this most insidious and unscrupulous enemy than they were in 1849? Is there much change in the state of the extensive marsh land in the country, from which a diminution of a prolific source of disease can be hoped? We fear not. The malaria of the Essex marshes still spreads its baleful influence over the whole Eastern portion of the metropolis. In the City of London (a small portion of the vast town) vigorous proceedings are being adopted to effect a constant and plentiful supply of water; but in other parts the supply is as deficient as it was five years ago; and some of the worst burying-grounds are still in use. Some few of the large towns have executed extensive works for the supply of water, but even in them the domestic services are but limited. In drainage very little has been done. The Lodging House Act, where adopted, has been productive of much good; but the filth and horrors of the streets and by-lanes of towns from Newcastle to Portsmouth are still beyond the conception of those who have not had ocular evidence.

In Scotland matters are not much better.

Notwithstanding the constant outcry against centralisation in Government, nothing effectual will be done towards saving life and staying off misery until a powerful body, under the direction of the Secretary of State for Home Affairs, is established with nearly absolute authority to direct the execution of sanitary works. There are various bills before Parliament now relating to these matters, and in them the attempt is made to authorise the management, more or less, by local and representative bodies. We sincerely hope that no such principle will be adopted by Parliament. The great Metropolitan Drainage Company proposed to execute works which would have purified the River Thames to a great extent, and by which many hundred acres of water-surface which at present deal out deadly evaporations day and night amongst the 2¼ millions of people, would have been converted into a simply harmless surface on which the numerous travellers might pursue their occupations or pleasure without the nauseous accompaniment which at present they have. But because the company asked for a moderate guarantee on the security of the rates, the local authorities bestir themselves, and oblige their members to throw out the bill. As a money calculation, this course was most foolish, for purifying the water of the Thames would annihilate a most active source of disease; but the temper which prompted the rejection of the bill is the most formidable thing which sanitary reformers have to deal with. Even educated people will go on for themselves and others as their forefathers did, paying the expenses of doctor and undertaker, rather than consent to be compulsorily charged with an annual sum for the execution of works which would shew their result in dismissal of both doctor and undertaker; and which sum after all would be less than the former. No! disease and death are voluntary; the other would be compulsory, though accompanied by life and health. Is the principle of this preference very different from that of suicide?

And are Christians prepared to assume the responsibility before God which they readily affix to suicide?

We have mentioned the fate of the above bill as an illustration; but many examples of the recklessness of mankind and the evils of private local managers might be mentioned. One of a different description to the last, but of a very large class, may be given. The scene is in London, in the parish of St. Pancras:—at a national school for above 600 children, the water cisterns are professedly supplied by the Hampstead Waterworks Company; for several years constant and energetic complaints had been made of the non-supply of water. At first it was said the service-pipes were not large enough; larger ones were adopted at once, still the complaints were not remedied, and it has been found impossible to keep the privies in a wholesome condition. Last Autumn, when a Sanitary Board was established in the parish vestry, a strong representation was made on the subject; the answer returned after an interval of time was, "The Water Company say your pipes are not large enough." But for some days after that there was a plentiful supply of water in the cisterns. It soon ceased, and the complaints are as bad as ever. Yet this same Company, on a demur being made to pay a high service-rate for water that had never been delivered, threatened at once to cut off the supply of water altogether. Here is a case where it has been shewn to be perfectly easy to give the full supply of water which is paid for; and yet, though the health of above 600 children is concerned in the matter, the Waterworks Company "care nought for those things."

The ravages of the cholera in Newcastle last Autumn are calculated to have caused a loss to the town generally of above £30,000. This was in a population of about 90,000. It would surely be far better to pay even this sum in the execution of works which would be the means of putting an end to the wholesale miseries that the cholera brings in the loss of husbands wives, children and friends.

CHEMICAL TABLES. COMPILED BY THORNTON J. HERAPATH.

No. I.—DENSITIES OR SPECIFIC GRAVITIES.

(*Continued from page* 331.)

When an * is attached to a name, it must be understood that the *preceding* numbers have been merely copied from some standard work, and are not the result of the Author's experiments.

Substances.	Density at 60° F.	Authority.
Pebble, English	2·609	Tredgold.
Pektolite	2·690	Von Kobell.
Pelokonite	2·567	Richter.
Pelopic acid	5·98 to 6·735	Rose.
Pennine	2·653 to 2·659	Marignac & Descloiz.
Pentathionic acid, con. solu.	1·67	Fordos and Gelis.
Perchloric	1·600	Brande.*
,, acid	1·717 to 1·800	Nativelli.
Periclase	3·750	Scacchi.
Pericline	2·640	Abich.
Perowskite	4·071	G. Rose.
Peristerite	2·568	Thomson.
Perthite	2·586	,,
,,	2·576 to 2·579	Hunt.
Petalite	2·420	Arfwedson.
,,	2·426	Gmelin.
,,	2·450	Dr. Clarke.
Petrolene	0·891 at 70°	Boussingault.
Pharmacolite	2·536	Selb.
	2·640	Klaproth.
	2·730	Haidinger.
Pharmacosiderite	3·000	Bournon.
Phenamylole	—1	Cahours.
Phenakite	2·969	Nordenskiöld.
Phocenic acid	0·937 at 77°	Brande.*
Pholerite	2·564	Smith.
Phloridzine	1·4298	Graham.*
Phosphocerite	4·780	Watts.
Phosphoric acid		
,, vitreous	2·387	Brisson.
,, hydrated	2·000	Brande.*
Phosphoric acid of L. Ph.	1·064 at 62°	Brande.*
Phosphorus	1·770	Berzelius.
,,	1·869	Böckmann.
,,	2·033	Fourcroy.
,,	2·089 at 63°	Böttger.
,, solid.	1·83 at 50°	Schrötter.
,, liq.	1·88 at 113°	,,
,, amorp.	1·964	,,
,, terchloride	1·45	Davy.
,, persulphide	2·02	Dupré.
,, oxychloride	1·700 at 54°	Würtz.
Picamar	1·100 at 68°	Reichenbach.
Picrolichenite	1·176	Alms.
Picrolite	2·591	Thomson.*
Picrosmine	2·596 to 2·660	,,
,,	2·976	(?)
Pichuric acid	0·883 at 68°	Georgey.
Pickeringite	1·78 to 1·80	Hayes.
Pikrophyll	2·730	Svanberg.
Pinite	2·782	Haidinger.
,,	2·757	Gmelin.
Pinguite	2·315	Breithaupt.
Pipestone	2·607	Thomson.
Pistacite	3·390 to 3·490	Herrmann.
Pitch-blende	6·468	Thomson.*
Pitch-stone	2·338 to 2·3604	,,
Pitch	1·08	,,
,,	1·150	Tredgold.
Pittizite	2·400	Karsten.
Placodine	7·988 to 8·062	Plattner.
Plagionite	5·400	Thomson.*

Substances.	Density at 60° F.	Authority.
Planets:—Saturn	0·2461	Author of the Vestiges of the N. H. of the Creation.*
„ Jupiter	1·047	
„ Mars	3·2870	
„ Earth	4·50 (?)	
„ Venus	5·7333	
„ Mercury	9·9000	
Plenakite	3·086	Bischoff.
Plaster, cast	1·286	Belidor.
Platinum, native	16·000 to 18·940	Humboldt.
„ bisulphide	7·224	Böttger.
„ protosulphide	6·20 (?)	Brande.*
„ sulphide	8·840	Böttger.
Pleisto-magnetic iron	5·094	Berzelius.
Plinthite	2·342	Thomson.
Plomb-gomme	4·880	Dufresnoy.
Plumbo-calcite	2·824	Johnston.
Polyadelphite	3·767	Thomson.
Polybasite	6·214	G. Rose.
Polyhallite	2·770	Stromeyer.
Polylite	3·231	Thomson.
Polymignite	4·806	Berzelius.
Polysphærite	6·092	Breithaupt.
„	6·029 to 6·983	Karsten.
Poonahlite	2·262	Gmelin.
Porcelain, China	2·384	Moseley.*
„ Sèvres	2·148	„
Porcelainite	2·300	Cresy.*
Porphyry	2·676 to 2·972	Moseley, &c.*
„	2·4 to 2·6	Portlocke.
Potassa, anhyd.	2·656	Karsten.
„ hyd.	2·044	Filhol.
Potassa-fusa	1·706 (?)	Ure, &c.*
Potassa, carbonate, anhyd.	2·2643	Karsten.
„ „ „	2·267	Filhol.
„ „ fused	2·600 (?)	Brande.*
„ „ sat.	1·500	„
„ „ solu.	1·540	Dalton.
Potassa, bicarbonate	2·053 at 39°·1	Playfair and Joule.
„ „ „	2·085	Gray.*
„ chlorate	1·989	Brande.*
„ „	2·3264 at 39°·1	Playfair and Joule.
„ chromate	2·600	Gray.*
„ „	2711 at 39°·1	Playfair and Joule.
„ „	to 2·723	
„ bichromate	2·620	Gray.*
„ „	2692 at 39°·1	Playfair and Joule.
„ sesquichromate	2·624	„
„ terchromate	2·655	„
„ ferrocyanate	1·830	Gray.*
„ nitrate	1·933 to 2·073	„
„ „	2·0985 to 2·1078	Playfair and Joule.
„ oxalate	2·1265	„
„ binoxalate	2·044	„
„ 4-oxalate	1·8488	„
„ biphosphate	2·850	Gray.*
„ racemate	2·080	Thomson.*
„ biracemate	2·558	„
„ sulphate	2·623	Karsten.
„ „	2·636	Watson.
„ „	2·656	Playfair and Joule.
„ „ anhyd.	2·625	Filhol.
„ „ sat.	1·972 at 47°·0	Anthon.
„ „ solu.		
„ „ „	1·0798 at 66°	Karsten.
„ bisulphate	2·277	Jacquelain.
„ „	2·478	Playfair and Joule.
„ sulphite	1·586	Gray.*
„ tartrate	1·556	„
„ „	2·140	Thomson.
„ soda-tartrate	1·740	Mitcherlich.
„ bitartrate	1·950	Gray.*
Potassa, liquor, L. Ph.	1·063	Brande.*
Potassium, chloride crystal	1·910 at 391°·1	Playfair and Joule.
„ „ anhyd.	1·984	Filhol.

Substances.	Density at 60° F.	Authority.
Potassium, chloride, anhyd.	1·945	Kopp.
„ „ „	1·977	Playfair and Joule.
„ „ „	1·915	Karsten.
„ „ sat. solu.	1·1635 at 64°	„
„ bromide	2·415	Playfair and Joule.
„ „ „	2·675	Karsten.
„ iodide	2·9084	„
„ „ „	3·056	Boullay.
„ „ „	3·091	Filhol.
„ sulphide	2·130	Brande.
Potato.starch	1·500	Kirwan.
Pot-stone	2·768 to 3000	Cresy.*
„	2·800 to 3·000	Erdmann.
Praseolite	2·754	Thomson.*
Prasilite	2·311	Thomson.*
Prehnite	2·900 to 2·953	Cresy.*
„	2·61 to 2·945	Morley.
Propione	—1	Berzelius.
Pseudo-malachite	4·20	Rammelsberg.
Psilo-melanite	4·080 to 4·360	Haidinger.
„	4·145	Ure.*
Pumice-stone	0·370 to 0·914	Brande.*
Pus	1·030	Kirwan.
Puzzolana	2·57 to 2·85	Erdmann.
Puschkitnite	3·430	Buchols.
Pycnite	3·503 to 3·530	Thomson.*
Pyrallolite	2·555 to 2·594	Nordenskiold.
Pyrargyllite	2·1505	G. Rose.
Pyrochlore	4·206 to 4·216	Thomson.
Pyrolusite	4·819 to 4·70	Berthier.
„	4·7 to 4·9	Thomson.
Pyromorphite	6·5781 to 6·9150	Klaproth.
„	5·560	Karsten.
„	7·009 to 7·050	Thomson.
Pyrope	3·780	Cresy.*
„	3·718 to 3·946	„

Substances.	Density at 60° F.	Authority.
Pyrophysalite	3·450	Cresy.*
Pyrorthite	2·190	Berzelius.
Pyrosklerite	2·740	Kobell.
Pyrosmalite	3·081	H. Rose.
„	2·95 to 3·10	Rammelsberg.
Pyroxene	3·233 to 3·349	Thomson.
„ ferruginous	3·713	Gruner.
Pyroxylic or Wood Spirit	0·824	Ure.*
	0·798 at 68°	Dumas.
Pyruvic acid	1·250	Thomson.
Quartz	2·613 to 2·654	Beudant.
„	2·674	Hauy.
„	2·690	Mohs.
„ Kilpatrick	2·525 (?)	Thomson.
Quinoleine	+1	Gerhardt.
Raphilite	2·850	Thomson.*
„	2·845	Hunt.
Resins, Animé	1·0284	Brisson.
„ benzoin	1·0284 to 1·0630	„
„ copal	1·045	Moseley.*
„ „	1·059 to 1·071	Ure.*
„ „	1·069	Thomson.
„ dammara	1·097 to 1·123	Brisson.
„ dragon's blood	1·196	„
„ „	1·201	Moseley.*
„ elemi	1·018	Brisson.
„ guaiacum	1·2289	Brande.*
„ Highgate resin	1·046	Thomson.
„ jalap	1·010	Unverdorben.
„ lac	1·139	Brisson.
„ labdanum	1·186	„
„ mastic	1·074	„
„ Maynas	1·120	Lewy.
„ rosin	1·080	Thomson.
„ sandarach	1·060	„
„ tacamahac	1·0463	Brisson.
Rhodalite	2·000	Thomson.

Substances.	Density at 60° F.	Authority.
Rhodochrome	2·668	Tiedler.
Ricinoleic acid	0·940	Säalmüller.
Ricottine	1·055	Schubler.
„ dry	1·355	„
Rionite	5·560	Thomson.
Rochelle-salt	1·749	Mitscherlich.
Rosemarine	0·867	Kane.
Ruby, Brazilian	3·531	Cresy.*
„ ballas	3·646	„
„ Oriental	4·284	„
„ spinelle	3·525	Thomson.
„ „	3·57 to 3·76	Cresy.*
Rum	0·9349	Ure.*
Rutile	4·180	Klaproth.
„	4·209	Damour.
Rutilite	3·1 to 3·5	Cresy.
Ryacolite	2·581 to 2·589	„
Sahlite	3·231 to 3·256	D'Andrada.
Sal-ammoniac	1·528	Thomson.*
Sal-gemme	2·143	Moseley.*
„	2·257	Mohs.
Salicine	1·1731	Piria.
Salicite	2·666	Thomson.
Salicone	1·0212	„
Salicylous acid	1·1731	Graham.*
Saliva	1·0038 to 1·0043.	Tiedemann & Gmelin.
„	1·0061 to 1·0088.	Mitscherlich.
Sand, quartzose	2·750	Tredgold.
„ river	1·886	Belidor.
„ pit	1·454 to 1·638	Tredgold.
Santonine	1·247 at 70°	Thomson.*
Sap, birch	1·004	John.
cow-tree	1·0124	Thomson.*
Sapphire	3·951	„
„ Oriental	4·200	Moseley.*
„ „	3·991 to 3·994	Cresy.*
„ Brazilian	3·131 to 4·083	„
Sapphirine	3·4282	Stromeyer.
Sardonyx	2·5951 to 2·6284.	Cresy.*
Scapolite	3·68 to 3·70	„
„	2·709 to 2·749	Thomson.
Scarbroite	1·480	Vernon.
Scheelite	5·95 to 6·07	Karsten.
Scheererite	0·680	Macaire Princep.
Schorl	3·452	Moseley.*
	3·0926 to 3·4520.	Cresy.*
Schorlamite	3.783	Shepard.
Scolegite	2·214	Fuchs and Gmelin.
„	2·270	Brooke.
Scorilite	1·708	Thomson.
Scorodite	3·110	Damour.
„	3·162 to 3·300	Haidinger.
„	3·400	Bournou.
Scorza	3·289	Thomson.
Selenic acid, con. solu.	2·524	Brande.*
„ „	2·600	Graham.*
Selenite	2·3117	Cresy.*
„	2·322	Royer and Dumas.
„	2·3905	Thomson.
Selenium	4·31	Boullay.
„	4·30 to 4·32	Berzelius.
„ granular	4·796 to 4·805	Schaffgotsch.
„ vitreous	4·276 to 4·286	„
Serpentine	2·412	Thomson.
„ precious	2·50 to 2·60	L. Gmelin.
Shale	2·600	Moseley.
Shell, pearl	2·190	Brewster.
„ oyster	2·020	„
Sideroschisolite	3·000	Thomson.
Silica	2·660	Kirwan.
Silicite	2·666	Thomson.
Silicium	+1	
Sillimanite	3·1636	Thomson.
„	3·41	Bower.

Substances.	Density at 60° F.	Authority.
Sillimanite	3·20 to 3·357	(?)
Silvanite	5·723	Reichenstein.
Silver, antimonial	9·4406	Haüy.
„ chromate	5·770	Playfair & Joule.
„ chloride, fused	5·450	Gray.*
	5·552	Thomson.
„ ditto, native	4·75 to 5·55	Klaproth.
„ fahl-ore	5·007	Weissenbach.
„ glance	7·196	Thomson.
„ iodide	5·500	Filhol.
„ oxide	7·140	W. Herapath.
„ „	7·147	Playfair & Joule.
„ „	7·249	Boullay.
„ nitrate	3·521	Gray.*
„ „	4·336	Playfair & Joule.
„ phosphate	7·300	Gray.*
„ selenide	8·000	G. Rose.
„ sulphate	5·410	Filhol.
„ „	5·322	Playfair & Joule.
„ „	5·340	Karsten.
„ sulphide	6·850	„
„ „	7·200	Brande.*
„ „	5·5 to 5·6	Phillips.
„ telluric	8·412 to 8·465	Thomson.
Silver-ore, dark red	5·8 to 5·9	„
„ light red	5·552	H. Rose.
Sinople	2·691	Cresy.*
Sismondine	3·564	De Lom.
Slate	2·110	Moseley.*
„	2·512 to 2·888	Cresy, &c.*
Smalts	2·440	Moseley.*
Smaragdite	3·000	Cresy.*
Smithsonite	3·584 to 3·598	Smithson.
Slag, iron	2·905 to 4·183	Dr. Percy.*
Snow, including interstices	0·041	
Soap, good commercial	—1	
„ foreign Castile	1·0715	Ure.*
Soap English Castile	0·9665	Ure.*
Soap-stone	2·396 to 2·411	Thomson.*
Soda, anhyd.	2·805	Karsten.
„ hydrated	2·000	Brande.*
„ „	2·130	Filhol.
„ concent. solu.	1·500	Dalton.
„ acetate	2·100	Gray.*
„ arseniate	1·736 at 39°.1	Playfair and Joule.
„ biborate	1·730	„
„ „	1·740	Kirwan.
„ „	1·757	Wattson.
„ carbonate	1·454	Playfair and Joule.
„ „	1·423	Haidinger.
„ „	1·620	Brande.*
„ concen. solu.	1·260 at 60°	„
„ „	1·107 at 46°·5	Anthon.
„ sesquicarb.	2·112	Haidinger.
„ bicarbonate	2·192	Playfair and Joule.
„ nitrate	2·182 to 2·2606	Playfair and Joule.
	2·188	Marx.
	2·200	Kopp.
	2·226	Karsten.
	2·260	Filhol.
„ phosphate	1·525	Playfair and Joule.
„ pyro-phosphate	1·836	„
„ sulphate, anhyd.	2·645	Thomson.
„ „	2·730	Cordier.
„ „	2·632	Karsten.
„ „	2·629	Filhol.
„ „ crys.	1·349	Thomson.
„ „	1·460	Gray.*
„ „	1·520	Filhol.
„ „	1·469	Playfair and Joule.
„ bisulphate	1·800	Thomson.
„ „	2·742	Playfair and Joule.
„ sulphite	2·950	Gray.*
„ tartrate	1·980	Thomson.

Substances.	Density at 60° F.	Authority.
Sodalite	2·288	Hofmann.
„	2·295 to 2·378	Thomson.*
„	2·29 to 2·49	L. Gmelin.
Sodium, bromide	2·340	Playfair and Joule.
„ chloride	2·011 at 39°·1	„
„ „	2·030	Unger.
„ „	2·078	Karsten.
„ „	2·150	Kopp.
„ „	2·240	Filhol.
„ „ sat solu.	1·2046 at 66°	Karsten.
„ „ „	1·205 at 46°5	Anthon
„ iodide	3·450	Filhol.
„ sulphide	2·471	„
Sordawalite	2·580	Thomson.*
Spar, fluor	3·791	Moseley.
„ calc.	2·837	„
Spermaceti	0·943	„
Sphærulite	2·416 to 2·452	Breithaupt.
Sphene	3·238 to 3·510	
Spherostilbite	2·310	Beudant.
Spinelle	2·523	Thomson.
„ artif.	3·548	Ebelman.
„ natural.	3·523 to 3·585	„
Spirits of wine, (*Sp. communis*)		
L. Ph.	0·815 at 62°	Gray.*
E. Ph.	0·796 at 60°	„
D. Ph.	0·810	„
Sp. com. tenuior L. and E. Phars.	0·920	„
D. Ph.	0·9194	„
Spodumene	3·115	Leonhard.
„	3·170	Haidinger.*
„	3·188	Thomson.
„	3·218	Moseley.*
Stalactites	2·546	„
„	2·324 to 2·478	Cresy.*
Stannolite	6·55 to 6·945	Berzelius.

Substances.	Density at 60° F.	Authority.
Starch, potato	1·530	Raspail.
Staurolite	3·5 to 3·8	L. Gmelin.
„	3·693	Thomson.
Steam	0·00076	Daniell.
Stearic acid	1·0100	Chevreul.
Stearine	—1 (?)	
Steel	7·78 to 7·84	Thomson.
„ soft	7·833	Templeton.
„ hardened	7·816	„
„ cast	7·920	Schaffgotsch.
Steinkeilite	2·6003	Stromeyer.
Sternmannite	6·833	Zippe.
Stellite, of Beck	2·836	Beck.
Stellite	2·612	Thomson.
Stercorite	1·6151	Th. Herapath.
Sternbergite	4·2151	Haidinger.
„	4·250	Zippe.
Stibæthyl	1·3244 at 61°	Lowig & Schweitz.
„ bromide of	1·954 at 63°	„
„ chloride of	1·540	„
Stilbite	2·0 to 2·2	Phillip.*
„	2·13 to 2·143	Thomson.*
„	2·161	Descotils.
„	2·500	Moseley.*
Stilpnomelan	3·27 to 3·40	Glocker.
Stone:—Bath, or roe-stone	1·975	Tredgold.
„ „	2·494	Kirwan.
„ blue lias	2·467	Tredgold.
„ brick	2·000	Templeton.
„ Bristol or mountain limestone	2·554	„
	2·640	Moseley.
	2·510	Tredgold.
„ Burford	2·049	Tredgold.
„ Caen	2·108	R. Tredgold.
„ chalk	2·781	Templeton.
„ Clitheroe limestone	2·686	Watson.
„ Collalo	2·040	Tredgold.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

On the Form *and* Composition *of the* Ammoniacal Carbonates.

By M. H. Sainte Claire Deville.

In a work published in "Poggendorff's Annalen," (Vol. XLVI.) the results of which are now inserted in every chemical work, M. H. Rose produced and analysed many combinations of ammonia with carbonic acid. I have often had occasion to examine and study these matters, in consequence of the double salts which the ammoniacal carbonates form with the metallic carbonates. The results of my observations will form the subject of this memoir.

Before commencing a subject on which I differ from this celebrated German chemist, I must mention the importance of the proofs to which any substance, which is considered new, ought to be subjected to establish its homogeneity, and to ascertain its existence as a distinct species. Long ago, in his Treatise on Organic Analysis, and recently in some articles published in the "Journal des Savants." M. Chevreul has fixed upon the method which should guide us in this species of determinations. These well-known principles will guide me in the examination of the system of experiments adopted by M. H. Rose.

Well defined crystalline forms, when susceptible of exact measurement, are the best characters, and those which may be relied on with the greatest security in deciding the questions of homogeneousness and identity. There are likewise other physical and chemical properties which it is unnecessary to enumerate here, and which are useful in discussions of this nature. But when these characters are wanting, the absence of simplicity in the composition, the want of chemical analogies, uncertainty in the method of preparation, suffice to induce us to come to a negative conclusion in the determination of *species*.

It is in this point of view that I have made a profound study of the crystallised substances resulting from the combination of ammonia with carbonic acid, in the presence of water; it has enabled me to make very important reductions in the catalogue of ammoniacal carbonates drawn up by M. H. Rose.

To obtain these carbonates, M. H. Rose employs two distinct methods of preparation: sublimation and crystallisation from various solvents.

The second method appears to me the only one likely to give unvarying results. I have often proved it: the combinations produced are always simple both in composition and crystalline form. If, in one sole instance, when forming crystallised bicarbonate of ammonia, complication appears to take place, it is only apparent, and the examination of the crystalline forms enables me to ascertain the existence of a simple species.

As for the method of sublimation, I do not think that it can give unvarying results, and the matters thus produced have none of the characters essential to the homogeneousness of chemical substances,

not even that simplicity of composition which renders it probable. When we have frequently distilled ammoniacal carbonates, we are soon convinced that carbonic acid and ammonia have a very feeble affinity for each other, let the temperature be ever so slightly raised even in the presence of water.* Whatever be the salt which is being acted on, the first effect of heat is the disunion of the elements; they afterwards reunite in a manner which varies according to the temperature of the recipient, the quantity of water, &c., so that we might, without proof to the contrary, consider all these sublimed matters as mixtures of sesquicarbonate and bicarbonate of ammonia in various proportions. To these elements we should add a certain quantity of anhydrous carbonate, whose existence is proved by the experiment of M. H. Rose and M. Bineau.

Sometimes these sublimed matters resolve themselves, either to the naked eye or with the microscope, into a mixture of discernable crystals, and they are easily recognised by an eye accustomed to the forms of sesquicarbonate and bicarbonate of ammonia; but ordinarily they present no distinct crystalline form. The different degree of their volatility is proved only by the different distances that they occupy in the distillatory vessel, in relation to the point heated. Finally, no special physical property, no clear reaction would lead us to regard these ephemeral substances† as distinct species. Why should they not be mixtures? Nothing proves the contrary; and without any other consideration the complication of their formula, and the absence of analogies, leads us to that idea.

It must likewise be remembered, that the least difference in the quantities of water or nitrogen, shewn by analysis, is sufficient to cause considerable variation in the formulæ representing their composition. This explains why M. H. Rose was led to suppose the existence of a tricarbonate of ammonia in the bodies which he considers as definite combinations, although it is incompatible with all chemical analogies. At any rate, such an hypothesis is very improbable. If, on a point of science on which M. Rose has exhibited so much skill, I venture to contradict him, it is because I have a method of analysis at once so simple, and so accurate, as to leave little fear of error.

The above train of reasoning prevents me from considering that their can be more than two distinct *species* among the hydrated combinations of carbonic acid and ammonia; namely, sesquicarbonate and bicarbonate. As for the neutral carbonate, it probably exists only at the ordinary temperature. A solution of sesquicarbonate made in a great excess of ammonia, precipitates sesquicarbonate only when treated with alcohol.

I.—**Sesquicarbonate of Ammonia.**—Its formula $3CO^2$, $2(NH^4O)$, $3HO$, is analogous to that of soda, $3CO^2$, $(2NaO, 3HO)$.

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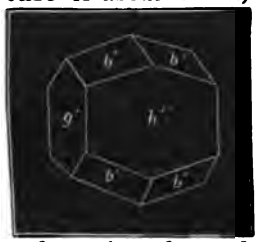
* This is so true, that at 140° F. the two gases behave as though they did combine, for the mixture does not condense, nor exhibit any contraction.

† They shew every sign of partial decomposition when pressed between blotting paper for examination or analysis.

The best method of obtaining it consists in dissolving at a temperature of about 86° F., commercial carbonate of ammonia in concentrated ammonia, and permitting the liquor to crystallise. It may likewise be dissolved in alcohol. It rapidly forms large, perfectly transparent crystals, which may easily be measured provided the operation is performed very rapidly for these crystals alter very easily, not in consequence of efflorescence, but of the loss of water and ammonia, being transformed into bicarbonate. They represent a right rectangular prism, the angles of which support the corresponding rhombic octohedra. It is a combination of the forms (111) (010) (100), according to Miller's notation.



Its parameters are:

$$a : b : c :: 1{\cdot}448 : 2{\cdot}186 : 1.$$

Its angles are.

	Found.	Calculated.
b^1b^1, (the two adjacent facettes in front) .	115° 45′	115° 45′
b^1b^1, (the two adjacent lateral facettes) .	138° 40′	138° 40′
b^1b^1, (the two opposite facettes) . . .	100° 35′	100° 40′
b^1h^1 " " . . .	110° 40′	110° 40′
b^1g^1 " " . . .	122° 10′	122° 6′
g^1h^1 " " . . .	90°	90°

The plane angle of the rhomboïdal prism is 112° 52′.

The analyses of this substance gave me very discordant results, because it is very difficult to dry without decomposing it; but I take M. Rose's formula, which agrees with the mean composition of the products which I have examined.

If the crystals are a little moist, there is an excess in the analysis; if, on the contrary, dessiccation caused the formation of a little bicarbonate of ammonia, the carbonic acid will be slightly in excess.

The following analyses are placed in the order in which they were made with drier and drier crystals:

	I.	II.	III.	IV.
Water . .	272·0	230·4	167·1	163·5
Ammonia .	152·7	184·8	137·0	134·6
Carbonic acid .	299·8	365·7	265·2	273·2
	724·5	780.9	569·3	571·3
Loss	0.9	Excess 0·2	Loss 0·0	Loss 0·0

Which gives in hundredths,

	I.	II.	III.	IV.
Water . .	37·5	29·5	29·2	28·6
Ammonia . .	21·1	23·7	24·2	23·6
Carbonic acid .	41·4	46·8	46·6	47·8
	100·0	100 0	100·0	100·0

	Mean.		Calculated.
Water . .	31·2	5 HO	31·0
Ammonia . .	23·2	2(NH³)	23·5
Carbonic acid .	45·6	3 CO^2	45·5
	100·0		100·0

leading to the formula 3 CO^2, 2(NH^4O), 3 HO.

These analyses were made in nitrogen to prevent any oxidation of the ammoniacal vapors, and account has been taken of the residues, however small, left by the salt after evaporation.

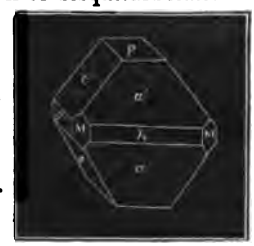
III.—Bicarbonate of Ammonia, (C^2O^4, NH^4O, HO).—M. H. Rose admits the existence of three bicarbonates: the first with the formula C^2O^4, NH^4O, HO, corresponding to the bicarbonate of potassa, with which it would be isomorphous. He obtained it by treating with alcohol or subjecting to spontaneous evaporation a solution of commercial bicarbonate of ammonia.

A second bicarbonate: C^2O^4, NH^4O, $\frac{2}{3}$ HO, by dissolving commercial carbonate hot and suffering it to cool.

Finally, a third, C^2O^4, NH^4O, 2 HO, by the distillation of this same commercial carbonate. The latter not being regularly crystallised, and possessing no character which can cause it to be considered a distinct species, its composition, although simple, removing it from all known alkaline carbonates, it is very probable that it is a mixture.

There would thus remain only the two species which I shall show to be in every respect identical, although of very different appearances. The following are the substances of which I made analyses and determinations.

No. 1.—I obtained the bicarbonate of ammonia, which served me for a type, by saturating a concentrated solution of sesquicarbonate of ammonia with carbonic acid. Magnificent crystals were deposited, unalterable by the air, and which could be measured with remarkable precision. They consisted of the facettes of a rectangular octohedron a^1, e^1, the base P, and the facettes M of the right rhomboïdal prism, the facettes h^1, g^1, of the right rectangular prism. It is, therefore, a combination of the forms (101)(011)(100)(110). The parameters are:



$$a : b : c :: 1 : 1{\cdot}653 : 2{\cdot}475.$$

The angles

	Observed.	Calculated.
Pe^1 . . .	123° 45′	123° 45′
Pg^1 . . .	90°	90°
Pa^1 . . .	112°	112°
Ph . . .	90°	90°
PM . . .	90°	90°
MM . . .	117° 30′	117° 40′
a^1e^1 . . .	101° 58′	102°
Me^1 . . .	142° 30′	142° 31′

$M a^1$	115° 40′	115° 30′
$a^1 a^1$	111° 26′	
$e^1 e^1$	136°	

MM. Miller and G. Rose find the same values, attributing them, with M. H. Rose, to a bicarbonate $\frac{2}{3}$ of an equivalent of water. The specimens which were used for these measurements were analysed and were found to contain 1 equivalent of water.

No. 2.—By cooling considerably a solution of commercial carbonate of ammonia, flat prismatic crystals are obtained, some of the facettes and angles of which were crooked; but I succeeded in measuring the angles:

$a^1 a^1$	111° 30′
$e^1 e^1$	135° 57′

This is both by form, and, as will be seen further on, by composition, the same bicarbonate (containing but 1 equivalent of water of crystallisation.)

No. 3.—I have endeavored to produce the bicarbonate of ammonia which, according to M. H. Rose, should be isomorphous with bicarbonate of potassa, but of which he has not published the measure. I found that by employing one of the processes which he advises, precipitating with alcohol, I always obtained crystals identical with No. 1. I found on them the inclinations:

Pe^1	124°
Pa^1	112°
Pg^1	90°
Ph^1	90°
MM^1	117° 39′
$e^1 e^1$	136°

which are incompatible with the oblique form and angles of bicarbonate of potassa.

No. 4.—Finally, to obtain, by another method, the compound with $\frac{2}{3}$ equivalent of water of M. H. Rose, I, according to his directions, caused commercial carbonate of ammonia to crystallise in previously heated liquors. I found, by this experiment, that it would be easy to fall into an error; for the crystals which I obtained had quite a different appearance. The facettes M were developed in such a manner as to render dominant the form (110), whereas the facettes e^1 and a^1 were scarcely marked. The crystal appeared flattened parallel to the facette h^1; but the angles and even the facettes remained the same:

Pg^1...	90°
MM	117° 15′
Me^1	about 143°
$a^1 a^1$	111° 26′
$e^1 e^1$	136° 10′

Some of the facettes were so much furrowed as to render the measurement of many of the inclinations impossible.

In one sole instance, while observing the transformation of the sesquicarbonate into bicarbonate of ammonia in a limited and humid

atmosphere, I saw the crystalline masses of sesquicarbonate lose their form; new facettes attached themselves, so to speak, to their superfices, and pure bicarbonate remained. I never found but one of these crystals which had any reflecting facettes; it appeared to me that its inclinations gave it some relation to an oblique prism, and thus allowed of the supposition that it was isomorphous with bicarbonate of potassa. But I was unable to repeat these incomplete measurements.

The following are the analytical results obtained with the specimens which I have been describing :—

	No. 1.		No. 2.	No. 3.	No. 4.
	a	*b*			
Water . . .	185·5	232·8	143·9	126·1	250·2
Ammonia . .	169·4	211·7	139·3	111·0	
Carbonic acid .	442·7	546·3	358·4	288·9	317·9
	797·6	990·8	641·6	526·0	568·8
Excess ...	2	0	Loss...1·7	0	1·5

or,

	No. 1.		No. 2.	No 3.	No. 4.
	a	*b*			
Water . . .	23·2	23·5	22·4	23·8	44·1
Ammonia . .	21·3	21·4	21·7	21·2	
Carbonic acid .	55·5	55·1	55·9	55·0	55·9
	100·0	100·0	100·0	100·0	100·0

Which leads to the formula, $C^2 O^4$, NH^4O, HO.

2HO	22·8
NH^4	21·5
$2CO^2$	55·7
	100·0

All these matters are therefore identical.

I wished to ascertain whether bicarbonate of potassa crystallised from a solution of neutral carbonate saturated with carbonic acid, would be identical with that ordinarily obtained by evaporating a solution of commercial neutral bicarbonate. The symmetrical base of the oblique prism was wanting in the small crystals which I obtained; but M. de Senarmont has kindly given me the measures which he had made from complete ordinary crystals. It will be seen that the inclinations are the same: I shall call the base *P*, and the lateral facettes of the oblique rhomboïdal prism *M*, *a* and *o* the facettes formed on the angles of the prism corresponding to the oblique axis, and *h* a facette forming a tangent to the angle terminated in the same manner.

We have,

	Found.	According to M. de Senarmont.
Pa . . . . }	125° 25′	128° 49′ } 125° 38′
Po . . . . }		156° 49′ }
oh	126° 50′	126° 46′
ha	127° 45′	127° 40′
MM	137° 57′	about 138°

The analysis of these crystals gave me—

		Found.		Calculated.
Water	101·8	8·91	HO	8·98
Carbonic acid . .	250·5	21·93	CO^2	21·96
Carbonate of potassa	790·1	69·16	CO^2 KO	69·06
	1142·4	100·00		100·00
Loss . .	1·7			

From the facts mentioned in the memoir, I conclude that these only exist two hydrated ammoniacal carbonates which can be admitted as distinct species, these are:

$3CO^2$ $2(NH^4O)$ 3HO, rhombic prism (analogous to soda);

$2CO^2$ (NH^4O) HO, rhombic prism (analogous to carbonate of potassa).

These analogies exist in the composition, and not in the crystalline form. It is therefore probable that bicarbonate of potassa and bicarbonate of ammonia are dimorphous.

Annales de Chimie et de Physique, January, 1854.

CHEMICAL *and* NATURAL HISTORY *of* LUPULINE. By M. J. PERSONNE.

THE hop, *Humulus Lupulus*, furnishes to commerce, as is well known, a product which is very important in therapeutics, and especially in the manufacture of beer, a product which presents itself under the form of small cones or short spikes, formed by the union of the female flowers on a short axis.

The fruits and scales which constitute these cones are covered, the fruits on the surface and the scales at the bottom of their outer side, by a multitude of small yellow resinous and odorous corpuscules, which are easily detached by rubbing the ripe dry cones. This yellow powder constitutes the Lupuline, and most important part of the hop. It is this alone to which should be attributed all the properties, that is to say, the bitter and aromatic flavor of the plant: for by depriving the scales and fruit of this yellow powder, they are completely deprived of all taste.

The importance of this body, which has been designated both *Lupuline* and *Lupulite*, has long been known. The first examination was made by Dr. Ivey, of New-York; the next year, MM. Payen and Chevallier made a more complete analysis of it; finally, in 1827, M. Raspail shewed, in a memoir on the organisation of Lupuline, the complete analogy of this body with pollen, and called it the *pollen of folliaceous organs*.

This assimilation of Lupuline to pollen on the one hand, and on the

other, the small quantity of matter which chemists have subjected to examination, not having enabled them to study the bodies which they obtained, I have determined to recommence this study,

Form, structure and development of Lupuline.—Lupuline, obtained from ripe cones, is a yellow powder, whose tint varies from a greenish yellow to a golden yellow, and a deep orange yellow, according to the length of time which has elapsed since it was gathered. The grains vary in size from 20 to 30-hundreths of a millimetre.

These grains, when fully developed, are in the form of an acorn with its cup; but the comparison only applies to its external form. In fact, the surface of the two portions of the Lupuline is in one single piece only, the upper part has in one place a slight depression, and this slight curve makes it similar in shape to the acorn. These two parts of the Lupuline present an apparently similar structure; they both appear to be composed of irregular cellules, which seem, nevertheless, to be arranged with a certain regularity. It is by the base of the cup that the grain is fixed on to the bracteæ, and the cellules which compose it secrete the materials contained in the cavity of the grain.

When observing the origin and development of this singular acorn, one of the most curious anatomical and physiological phenomena offered by science is witnessed. Lupuline is found to commence with a very short hair, whose globulous extremity is composed of several utriculi; this upper part is formed into a circular bulb by its numerous utriculi; the summit appears depressed by the gradual elevation of the edges; it is at length transformed into a very elegant cup, striped longitudinally both externally and internally, and covered inside with the cuticle which forms the superior part of the grain. The pedicle having remained stationary during this increase, causes the cup to appear as though closely attached.

Then commences the secretion of the liquid, which, spreading between the surface of the cup and the cuticle which lines it, gradually raises this latter, and forms it externally like the finger of a glove. Thus the Lupuline assumes the acorn form which I have mentioned, and has attained its perfect development.

It is thus clear that M. Raspail is mistaken in likening it to pollen; and that the position which Lupuline occupies in the plant, the period of its full development, &c., are so many facts which contradict the opinion entertained by physiologists as to the nature and functions of Lupuline.

Finally, as this body only developes itself completely on the bracteæ or floral scales and on the ovary, whereas, on the leaves of the stalks it remains in a rudimentary state, and rapidly withers, I think, with MM. Payen and Chevallier, that it is only an organ intended to protect the fruit from moisture by means of the resinous matter which it secretes, in the same manner that some buds are protected by a peculiar resinous matter.

Chemical History of Lupuline.—Lupuline furnishes with boiling water two different groups of bodies; some volatile, obtained by distil-

lation in this vehicle; the others fixed, or a least not volatilising with steam.

The volatile products are,—an acid and an essential oil.

The acid is obtained by saturating the distilled acid water with carbonate of soda, evaporating to dryness, treating the residue with sulphuric or phosphoric acid, and distilling the oily liquid obtained; after several rectifications a liquor is collected which boils at about 347° F., and distilling unaltered at about that temperature.

This acid is a colorless, slightly oleaginous, very fluid liquid, with a strong and persistent odor of valerianic acid; its flavor is acid and sharp; it produces a white stain on the tongue, like the powerful volatile fatty acids. It does not become solid at 4° F.; on the contrary, it remains perfectly liquid; it burns easily with a smoky flame. Its density at 59° F., was found to be 0·9403.

It forms, with baryta, a difficultly crystallisable salt, which has a giratory movement when projected in small pieces on the surface of water.

Its composition in hundredths was found as follows:—C = 58·64; H = 9·91; O = 31·45; which gives the formula of hydrated valerianic acids C^{10}, H^{10}, O^4. The analysis of the salts of silver, copper and baryta, all lead to the same result. Thus, the salt of silver contains in hundredths; Ag = 51·81; C = 28·49; H = 4·38; the salt of copper; CuO = 29·599; C = 44·97; H = 6·86; the salt of baryta; BaO = 45·04; C = 35·24; H = 5·337.

This acid is consequently valerianic acid, of which Lupuline gives quantities which vary from 1 per cent. to 0·61, that is to say, about half.

The volatile oil is lighter than water; sometimes of a beautiful green, which color it loses by rectification; its odor is something similar to hops, it has no acid reaction; but when exposed to the air it becomes resinous and acid. It boils at about 284° F., and distils for a few minutes between 302° and 320° F.; but the temperature rises very rapidly, and soon exceeds 572° F., so that it is impossible to isolate products whose boiling points should be constant.

I obtained two liquids whose boiling points were very far apart, for the first was collected between 302° and 320° F., and the second about 572° F.; nevertheless, these liquids gave, when analysed, the same numbers in hundredths. The quantity of carbon, in a great number of analyses, never varied more than 0·26 or 0·27 per cent., the hydrogen and oxygen remaining always in the same proportion. The analysis of these bodies leads to the formula C^{22}, H^{16}, O^2.

The liquids shew no change at a cold of 2° F. Sulphuric acid dissolves them and turns them acid; water separates them from the solution, and the aqueous liquid retains a copulate acid, forming a soluble salt with baryta. Nitric acid transforms them into valerianic acid and a resinous matter. Potassa in solution appears to take no effect upon them; but if they are allowed to drop into melted potassa, a liquid hydrocarbon and carbonate, and valerianate of potassa are obtained.

This reaction with potassa explains the real constitution of this essence, and places it by the side of essence of valerian. The hydrocarbon thus obtained possesses, in fact, the formula C^{10} H^{8}; and on subtracting this formula from that given above, there remains C^{12} H^{10} O^{2}, which is the valerol obtained from essence of valerian by M. Gerhardt. The great quantity of resinous matter contained by Lupuline, renders it more difficult to isolate this solid body than in the case of valerian.

The only difference existing between the essence of hops or Lupuline and valerian, is, that the hydrocarbon of essence of hops is not the Borneïne of essence of valerian; it will not give solid Borneo camphor; and its odor more nearly approaches thymene. It easily undergoes a molecular condensation by the action of heat.

The solid resinous mass, exhausted with water, still retains a great quantity of the oxygenous body or *valerol*. If, after being intimately mixed with hydrate of lime, the mixture is distilled at a moderate temperature, so that the mass may not carbonise, an oily liquid is obtained with a penetrating odor, whence rectification separates a liquid boiling at about 190° F., colorless, of a penetrating etherial odor, without action on colored reagents, but soon becoming acid by exposure to the air. Its density is 0·8009 at 68° F.

Potassa turns it brown, and renders it resinous; it reduces nitrate of silver with the greatest ease. Chromic and nitric acids convert it into valerianic acid; soda lime gives valerianate of soda and hydrogen. Its composition, in hundredths, was found to be, C = 69·68; H = 11·60; whence the formula C^{10} H^{10} O^{2}, which is that of the valerianic aldehyde, obtained by M. Chancel, by the distillation of valerianate of lime.

There only remains, to terminate the chemical history of Lupuline, to describe the non-volatile products. The most important are, an organic acid and a bitter nitrogenous matter, soluble in water; but I have not yet succeeded in isolating them, so as to be able to examine them sufficiently.

Comptes Rendus, No. 7, February 15th, 1854.

On Glycerine *and its* Application *to the* Various Branches *of* Medical Science. By M. P. A. Cap.

I wish to call the attention of the Imperial Academy of Medicine to a substance which is not exactly a new one, for its discovery dates from the close of the last century, but which has only lately been applied to art, manufacture, and medical science. I speak of glycerine, whose very singular, although not strongly marked, physical and chemical properties were for a long time, in a manner, forgotten. As if science should seek for efficacious agents only among those bodies which exhibit decided energy of action! as if therapeutics could only find active means among substances capable of exciting in the organism violent reactions and a very clear antagonism! In this way water would be

but an uninteresting substance in nature, and nitrogen would be of anything but its present importance in the atmosphere, and in the composition of most organic bodies.

Glycerine was discovered in 1779 by the celebrated Scheele. This chemist, examining the water which had served for the preparation of simple plaster, that is to say, the saponification of lard by litharge, first observed that this water, concentrated by evaporation, furnished a matter with a soft, sweet taste, of a thick consistence, unctuous to the touch, with but little odor, without either acid or alkaline reaction, and he gave to it the name of *sweet principle of oils*. This matter, which was for some time thought to be of a gummy nature, and which seemed only to have negative qualities, attracted, at first, but little attention from chemists. At a later period, M. Chevreul's beautiful investigations into the nature of the fatty bodies generalised the production of this substance, which he named *glycerine*, and shewed that it was a constant product of saponification. He established the principle that the whole of one class of natural fatty bodies, under the influence of alkalies, were decomposable for the production of soap on the one hand, and glycerine on the other. Thus, this matter is produced in abundance, not only in the production of metallic plasters, but in the manufacture of ordinary soap and in that of stearine, that curious and still recent branch of industry, to which we owe stearine candles.

Notwithstanding the abundance in which glycerine is produced in these various operations, no one had yet found any useful employment for it. The mother-liquors of soap works and stearic acid manufactories were daily thrown away and lost. In fact, the glycerine was found in them mixed with so large a quantity of water, that it was difficult to remove it economically. On the other hand, the fatty bodies employed in this last manufacture, contained many foreign substances which were much opposed to the purification of the glycerine derived from this source. The principal difficulty consisted in getting rid of the very disagreeable odor inherent to the mother-liquors from the fabrication of stearic acid. By treating them with a current of carbonic acid, as recommended in works on chemistry, so as to remove the lime which they contain, no success is attained as to depriving them of this dreadful odor, because the lime is combined with the volatile acids (sebacic, butyric, and valerianic), which are found in very considerable quantity in them, and which are disengaged at an elevated temperature.

Thus, after having subjected these mother-liquors to a slight evaporation, and treating them with carbonic acid, if a solution of oxalic acid is poured in, a considerable precipitate is immediately obtained; and, if the temperature is raised at the same time, the disengagement of the volatile fatty acids becomes more abundant. This observation leads me to the processes by means of which I have been able to produce glycerine in a complete state of purity, processes which I shall now describe in a few words.

I begin by concentrating, by means of evaporation, a given quan-

tity of the mother-liquors of soap works or of stearic acid manufactories, then I determine, by means of oxalic acid, the proportions of lime which they contain. I then add a quantity of sulphuric acid equivalent to that of the oxalic acid necessary for the saturation of the lime. The resulting sulphate of lime is precipitated in an insoluble state. The liquor is decanted and boiled in a wrought iron pot, covered with a thick plate of lead. Care must be taken at the same time to agitate the liquid powerfully by means of an appropriate stirrer moved by mechanical means. The fatty acids volatilise, the liquor begins to lose its color, and soon loses likewise its disagreeable odor. When it has attained a density of 10° of the areometer it is allowed to cool, and filtered through a cloth to separate a fresh quantity of sulphate of lime. The excess of acid is saturated with a little carbonate of the same base, and the evaporation is continued, still rapidly agitating the liquor. When it has attained by means of evaporation to 24° areometer, a fresh proportion of sulphate of lime is deposited; it is allowed to cool, passed through a cloth, and the deposit washed with water slightly alcoholised.

It is evaporated a third time, continuing the stirring, until the liquor has attained 28° when hot, and 30° when cold. By cooling, a little more sulphate of lime is deposited, which is separated by another filtration. The product is then without odor, the taste is soft and saccharine, it is unctuous, and its color is slightly buff. In this state it is treated cold with washed animal charcoal, filtered, and the glycerine is obtained entirely without odor, without color, and of a syrupy consistence.

Arrived at this point of concentration (28 to 29 areometric degrees), glycerine is fit for most uses in manufacture and medicine. However, it contains a large proportion of water, which is removed with difficulty after a prolonged concentration. When it reaches 31° it has lost from 20 to 25 per cent. In this state, if a piece of cotton is immersed in it, it burns exactly like oil, with a reddish flame. If the temperature is raised, thick vapors are at first disengaged, then it decomposes, forming several volatile products and a mass of charcoal.

I will not detain your attention on the investigations to which glycerine has given rise as a chemical subject. M. Chevreul's works have established that the greater part of the natural fatty bodies, such as stearine, oleïne, and butyrine, are a species of salts formed of an anhydrous fatty acid, fixed or volatile, and glycerine likewise anhydrous, which is a constitution analogous to the ethers; in the same manner that cetine may be transformed into margaric and oleïc acids and ethal. M. Pelouze produced, with glycerine and the acids of sulphur or phosphorus, sulphoglyceric and phosphoglyceric acids. More recently, M. Berthelot has combined glycerine with the fatty acids, properly so called, with several organic acids, and even with mineral acids. He has thus produced crystallised or liquid neutral bodies, to which he has given, for example, the names of *acetine, valerine, benzoïcine,* or *sebine,* according as they resulted from the combination of glycerine with acetic, valerianic, benzoïc, or sebacic acids. These

bodies, when treated with alkalies, saponify and regenerate the acid which gave rise to them, isolating the glycerine : a decisive and brilliant confirmation of the theory promulgated by M. Chevreul as to the general constitution of the natural fatty bodies.

In a therapeutical point of view the physical properties of glycerine are very interesting, properties so singular that it is astonishing that manufactures, as well as the medical sciences, should not sooner have turned them to use. Glycerine neither belongs to the gummy nor the fatty bodies; it is a neutral body *sui generis*, ordinarily liquid and uncrystallisable. Like water, it mixes in all proportions with aqueous liquids, alcohol and vinegar; it dissolves most of the bodies that can be dissolved by water; it is slightly hygrometric and manifests no reaction, either acid or alkaline. Like oil, it is unctuous to the touch; it does not evaporate in contact with air; it does not decompose even at the highest temperature. Applied to living tissues, it lubricates and renders them supple *without greasing them;* it mixes in certain proportions with lard and the fatty bodies; it dissolves the volatile oils; it is not susceptible of becoming rancid nor of spontaneous fermentation.

These are, doubtless, remarkable and very strange properties, for glycerine unites most of those which characterise two bodies in some respect antagonistic, oil and water. It was, therefore, natural to suppose that this substance would play a part alike new and varied in the arts and manufactures as well as in the medical sciences; and this is now, on all sides, on the point of being realised.

Without stopping on the numerous purposes which glycerine will serve for in the industrial arts, and which I intend treating of in a special work, I shall limit myself to mentioning the new and interesting resources which it affords to the medical sciences; not that this substance exerts a very marked action on the organism, and likewise it has been but little tried internally, but it is very different with its external application.

We have said that glycerine lubricates the organic tissues and renders them supple; we must add, that it has been found useful in most skin affections. The observations of all the medical men who have tried it shew that glycerine easily penetrates into the pores of the skin, softens this organ, and retains on its surface, in consequence of its hygrometric properties, a species of permanent moisture, very efficacious in cases of dryness and thickening of the dermis. It heals cracks in the nipple and skin; it preserves its softness, and calms erethism. Dr. Dallaz says that it is the best known cosmetic. Dr. Trousseau has proved the good effects of glycerine in superficial complaints of the skin, especially in prurigo, which resembles a scabby disease. He likewise speaks of the known efficacy of this substance in certain complaints of the ears which proceed from cutaneous irritation, extending from the exterior to the interior of the auditory apparatus. Glycerine, according to this professor, is useful in all affections of the skin which would be irritated by the use of fatty bodies or exciting applications. The same doctor has found great benefit from its use in

cutaneous phlegmasia of a pruriginous nature, which so often affects in so distressing a manner the genitals, anus, or their adjuncts. Dr. Bazin has made frequent and successful use of it in eczema, zona, acne, ichthyose, and generally in all the skin diseases whose principle does not reside essentially in an alteration of the internal system. He finds, in this case, that glycerine is preferable to the most vaunted irritant cosmetics, such as tar-water, alkaline solutions, and especially corrosive sublimate.

On its peculiar cosmetic properties perfumery has founded a very successful means of acting on the cutaneous tissue. But pharmacy could not rest inactive on such a subject. On the 21st of July, 1851, I deposited in the Academy of Sciences a sealed packet, particularly relating to the application of glycerine to industrial and medicinal purposes. Since then I have placed this substance at the disposal of several medical men, with a request that they would employ it in their practice, and this circumstance caused the observations which I have just quoted.

But I must hasten to my own personal remarks, which apply to the part which I consider glycerine ought to take in pharmacy. It is evident that its unctuous state, its slightly hygrometric properties, its analogies to water and oil, and finally, its perfect harmlessness, render this substance fit for many various uses. It blends with wonderful ease with every form of medicament. It may be used either alone or with other therapeutic agents. It mixes in all proportions with water for baths, injections, fomentations, and lotions of all descriptions. Applied to burns or other wounds, it keeps them from the air, and maintains the suppleness of their edges. When added to cataplasms, it preserves their softness, and, which is very important, it prevents them from adhering by their edges to the surfaces on which they are applied. This combination of properties presents, as may be seen, in glycerine, a new and valuable *excipient* to be added to the scanty list of bodies of this nature now at the disposal of art—an excipient which appears to hold a place between water and oil, for it participates in most of the qualities of both. Glycerine unites as well with aqueous and alcoholic liquids as it does with lard, ointments, pomades, and soaps. It will serve as a base for liniments, ointments, and embrocations: it will mix with extracts, tinctures, alcoholates, and medicinal wines; a few drops of glycerine added to a pillular mass will prevent it from drying, &c.; it will consequently assist in most uses of medicine, surgery, and veterinary science, adding to any preparation with which it is mixed the advantage of its lenitive, sedative properties, softening the tissues, and preparing them for the absorption of the medicinal substances to which it is united.

But this is not the limit of its pharmaceutical uses. Glycerine dissolves the vegetable acids, the deliquescent salts, the sulphates of potassa, soda and copper, the nitrates of potassa and silver, the alkaline chlorides, potassa, soda, baryta, strontia, bromine, iodine, and even oxide of lead. It dissolves or suspends the vegetable alkaloïds in the same manner as the aqueous liquids, and at the same time the resulting

products may be used for the same purposes as though mixed with oil. Thus the salts of morphia dissolve in it completely, even cold, in all proportions. Sulphate of quinine, in the proportion of 1-10th, dissolves in it when hot, but when cold separates in clots which, when triturated with the supernatant liquid, give it the consistence of a cerate, very useful for frictions and embrocations. It is the same with the salts of brucine, strychnine, veratrine, and most preparations of the same order, which enables us to consider that we have now, if not medicinal oils with a vegetable alkaloïd base, at least a series of new preparations which will fulfil a perfectly analogous use in therapeutics.

These circumstances appear to authorise me in establishing in pharmacy a new order of medicaments, in which glycerine plays the part of excipient. Conforming to the most rational classification and nomenclature, and adopting as radical the term *glycerol*, I shall give to this order of products the name *glycerolic*, and to the principal species that of *glycerolates*. The place of this new order of pharmaceutical products will be after the liquid hydrolates, oleolates, and saccharolates, and the melleolates: that is, the medicaments in which the same part is taken by water, oil, sugar, or honey. For example: a glycerolic solution of hydrochlorate of morphine, or sulphate of quinine or of nitrate of strychnine, would consequently be a *glycerolate* of morphine, quinine, or strychnine, whose relative proportions would be determined either by the codex or the magistral formula of the physician; and those medicaments, if you approve of them, will take their place in dispensaries and in medical practice, with other analogous compounds, obtained by means of the other excipients hitherto possessed by the art of pharmacy.

If I am not mistaken, and if the applications of glycerine which I have merely just pointed out, appear to the profession worthy of serious consideration, Chemistry will have rendered another great service both to arts and medicine: 1st. By bringing to perfection the preparation of a product hitherto surrounded by difficulties which I have succeeded in removing. 2nd. By exhibiting a substance as yet without any importance, and which has hitherto been rejected as useless. 3rd. By giving a value to a chemical product which up to the present time has been without any. 4th. By providing pharmacy with a new excipient whose applications appear to us even now as numerous as they are novel, but which, without doubt, will be multiplied infinitely by further experiments. I intend myself to prosecute the study of glycerine, its combinations, and its applications to medicine, and shall hasten to lay fresh communications on this subject before the Academy.

Journal de Pharmacie et de Chimie, February, 1854.

ANALYTICAL CHEMISTRY.

On the SEPARATION *of some* METALLIC OXIDES. By M. FLAJOLOT.

Impossibility of Separating Copper from Zinc and Nickel by Sulphuretted Hydrogen.—In the course of the year 1851, several veins of grey copper were found in the province of Algeria, containing many varieties of this mineral. I then undertook to analyse all these varieties, but hardly had I commenced my task when I found that it was impossible to separate the copper completely from the zinc and nickel by precipitating it with sulphuretted hydrogen from an acid liquor, for the sulphuret of copper always took with it appreciable quantities of the sulphurets of the other metals.

Soon after, this impossibility was put beyond doubt by the experiments of MM. Rivot and Bouquet, which likewise indicated a means of separating copper from zinc; but the process described by these chemists being inapplicable to the separation of copper from nickel, the problem of the analysis of grey copper was not yet solved, and I had still to seek, among those compounds which had not as yet been used in chemical analysis, for some reagents which might be able to effect this separation.

Means of Separation.—Hyposulphite of soda and hydriodic acid gave me remarkably accurate results. The second reagent will separate copper not only from zinc, nickel and cobalt, but likewise from arsenic and antimony; so that it is very easy to isolate copper by this means from almost all ores.

Precipitation of Copper by Hyposulphite of Soda.—By means of hyposulphite of soda the separation of copper from all substances whose acid solutions are not precipitable by sulphuretted hydrogen, may be performed with an accuracy seldom attained in chemical analysis. When hyposulphite of soda is poured into an acid solution of a salt of copper, maintained at the boiling point, the whole of the copper is precipitated in the state of sulphuret (Cu^2S) with a formation of sulphate of soda and one of the acids of sulphur, which, with the same quantity of oxygen as hyposulphuric acid contains a larger proportion of sulphur. The following is the *modus operandi*.

The solution containing the copper to be precipitated should contain no hydrochloric acid, and very little nitric acid; for this purpose it must be evaporated with sulphuric acid so as to expel the other two acids. Water is then added, and into this solution, which is kept boiling, hyposulphite of soda, dissolved in water, is poured until it ceases to form a black precipitate. When the precipitate is collected at the bottom of the matrass, and the supernatant liquor no longer contains sulphur in suspension, it is a proof that the whole of the copper is precipitated. This precipitate of copper is collected on a filter, washed with boiling water, and treated by the usual method to convert it into oxide of copper. This precipitate is the sub-sulphuret of copper (Cu^2S); it is not very bulky; it is quickly washed and does not oxidise by washing, so that it is not even necessary to add sulphuretted hydrogen

to the water used for washing it. However, this extra precaution may be taken without inconvenience.

The other metals contained in the liquor separated from the sulphuret of copper may be precipitated by the ordinary means, after having boiled the liquor with nitric acid, so as to destroy the sulphuretted acid formed by the reaction of the alkaline hyposulphite on the salt of copper.

The hyposulphite of soda of commerce is not pure enough for this operation, but it becomes so by the addition of a little carbonate to its solution and filtration.

I have said that it is better that the solution from which the copper is to be precipitated should not contain either hydrochloric or nitric acid, but this is not indispensable. By adding sulphuric acid, which is always necessary, and pouring in hyposulphite of soda, as above described, the whole of the copper will be precipitated; but in this case a much greater quantity of reagent will be required than in the first case, for hydrochloric acid causes the formation of chloride of copper (Cu^2Cl), which is only decomposed with difficulty and by means of a very considerable excess of the precipitating reagent.

Nitric acid and hyposulphite of soda mutually destroy each other with great violence when hot and before the precipitation of the least particle of copper. The effect of this acid is consequently to cause the loss of a great quantity of reagent; for this reason it is better to remove the greater part previous to commencing the operation.

VERIFICATION OF THE PROCESS.

Proofs of the Exactness of this Method.—1.—I dissolved in nitric acid:—

Pure copper	0·908 grms.
Oxide of zinc . . .	0·392

The copper was precipitated as above described and the sulphuret of copper was dissolved in aqua regia. The solution was neutralised with carbonate of soda, then ammonia was added; I then boiled it with potassa, and oxide of copper was precipitated. The oxide of copper thus obtained is much less voluminous, and more easily washed than that which was precipitated from a non-ammoniacal liquid. The liquor, separated by filtration from the sulphuret of copper, was treated with nitric acid and the zinc precipitated with carbonate of soda.

I thus obtained, taking as the equivalent of copper the number 395·6:

	grms.
Oxide of copper . . .	1·137 containing 0·9073 of copper.
Oxide of zinc . . .	0·392

2.—I dissolved in aqua regia:

Pure copper	1·112 grms
Oxides of nickel and cobalt	0·514 „

I added, when the solution was complete, sulphuric acid diluted with an equal volume of water, and I boiled it until the other two acids were expelled, inclining the neck of the matrass to avoid projections; then on operating as above, I obtained:

	grms.
Oxides of nickel and cobalt	0·515

Oxide of copper . . . 1·392 containing 1·1118 of copper.

3.—Finally, I dissolved in aqua regia 1·086 of copper with oxides of manganese, iron, zinc, cobalt, and nickel; I separated the copper as in the two former instances, and I found 1·268 grms. of oxide corresponding to 1·0262 of metallic copper.

These verifications prove that the method of precipitating copper with hyposulphite of soda is strictly correct.

Precipitation of the Copper by Iodine dissolved in Sulphurous Acid.—The second method for the precipitation of copper is founded on the insolubility of iodide of copper (Cu^2I) in water acidulated with sulphuric or nitric acid, and containing an excess of sulphurous acid. The whole of the copper will not be precipitated from its solutions by pouring in sulphurous acid and iodide of potassium, for a slight excess of this reagent always redissolves too much of the iodide of copper for it to be neglected. Neither will hydriodic acid succeed if the copper solution contains hydrochloric acid; but if into sulphate or nitrate of copper is poured, first a large quantity of sulphurous acid, then hydriodic acid, the copper will be precipitated, so that only imponderable traces remain in solution.

Hydriodic acid is easily prepared when required for this purpose by dissolving iodine in sulphurous acid. The result is a mixture of hydriodic and sulphuric acids, and the latter in no way hinders the success of the operation.

The following are the details of the experiment:

The matter containing the copper should be dissolved in nitric acid: the solution is evaporated so as to drive off the greater part of the excess of acid; water is then added, and if antimony is present tartaric acid must likewise be employed. After a sufficient digestion, should there be a residue it is filtered, after which a sufficient quantity of sulphurous acid is added, then the iodine dissolved in sulphurous acid. It is necessary to be careful to pour in the iodine solution in small portions, and to stop when it ceases to form a precipitate, for too great an excess of hydriodic acid might redissolve a little iodide of copper, although almost an inappreciable quantity. The liquor should not be filtered until after standing for at least twelve hours. Whilst washing the iodide of copper the water must be poured upon the filter with some precaution, for the precipitate has a great tendency to rise and run over the edge of the filter. Should this accident happen it may be repaired by collecting on another filter, after washing, the small quantity of iodide of copper that may have run over.

The precipitate may be collected on a filter, dried and weighed, as is done with sulphuret of antimony; when the dessication is finished the weight is determined, and the quantity of copper contained in it is deduced by calculation. But it is better to redissolve it, and to precipitate the copper by potassa. For this purpose it may be attacked by aqua regia, exactly in the same manner as though it were sulphuret of copper. It may be put, with the filter, into a glass, and ammonia poured on it, which dissolves it rapidly. It is exposed to the air for some hours so as to completely oxidise the product; it is then filtered,

and the liquor boiled with potassa until the oxide of copper is completely precipitated. There is another method of dissolving iodide of copper, which I prefer. It consists in dropping the iodide into a matrass through a filter, and pouring in water; the filter is then itself introduced, and a current of chlorine is directed into it. The solution takes place quickly; when it is complete the liquor contains chloride of copper (Cu Cl), iodic acid and hydrochloric acid. The oxide of copper is precipitated with potassa.

VERIFICATION OF THE PROCESS.

Proof of the exactness of the Process.—1.—I dissolved, in nitric acid,—

Pure Copper	1·065 gr.
Oxide of Zinc	0·452

The solution was evaporated almost to dryness, then diluted with water, and treated as above described. After having precipitated and separated the copper, I passed sulphuretted hydrogen into the filtered liquor, so as to assure myself that no more copper remained in solution. A slight precipitate of sulphur was formed, scarcely colored of a greyish yellow by sulphuret of copper. I collected it on a filter and calcined it. Its weight was inappreciable by the balance; but, by means of a very dilute solution of sulphuret of sodium, I found that this precipitate contained 2–5ths of a milligramme of copper, a quantity which would never be of any consequence in an analysis.

The iodide of copper converted into oxide gave 1·334 gr., containing 1·0645 of copper, and by precipitating the zinc with carbonate of soda, I found, after the calcination of the carbonate of zinc, 0·452 of oxide.

2.—I dissolved in aqua regia,—

Pure Copper	0·980 gr.
Oxide of Nickel	0·501

I added sulphuric acid, and boiled it so as to drive off the whole of the hydrochloric and nitric acids; then I precipitated the copper as in the preceding case. I obtained,—

Oxide of Copper . .	1·227 gr., containing 0·979 of copper.
Oxide of Nickel . .	0·500

The solution, separated by filtration from the iodide of copper, was treated with sulphuretted hydrogen as above, and I found that there remained in solution 3–5ths of a milligramme of copper.

3.—Finally, I dissolved 1·224 gr. of copper in nitric acid. I added carbonate of manganese, iron, oxide of zinc, sulphate of nickel, nitrate of cobalt, arsenious acid, and metallic antimony. The excess of acid was evaporated, and I added water and tartaric acid; when completely dissolved, I precipitated the copper in the state of iodide. This iodide of copper gave 1·557 of oxide, containing 1·2428 gr. of metallic copper. The other metals were not estimated; I was satisfied with seeking copper in their solution, and I did not find even a trace. The iodide of copper contained a little iron, which was separated in the state of sesquioxide by ammonia. The weight of this oxide was only 0·001 gr., a quantity which may be overlooked in most analyses.

It results from these experiments that iodine dissolved in suphurous acid precipitates copper in the sulphate and nitrate, with the addition of an excess of sulphurous acid, more completely than sulphuric acid precipitates baryta from its solutions; for, in 200 grammes of solution of nitrate of copper, there only remained after the precipitation of the copper by hydroiodic acid, 0·0004 gr. of this metal; whereas, in an equal quantity of chloride of barium precipitated by sulphuric acid, I found nearly 0·002 gr. of sulphate of baryta.

This means of separating copper is then as exact as the greater part of our most rigorous analytical processes.

Separation of Copper and Mercury.—The preceding process will not serve for the separation of copper from mercury. This separation may be made by neutralising the solution of the two metals with carbonate of soda, adding cyanide of potassium in sufficient quantity to redissolve the cyanide of copper at first precipitated, then pouring in hydrosulphate of ammonia, which does not precipitate the mercury.

Separation of Copper and Bismuth.—When the copper is precipitated in the state of iodide, and bismuth is found in solution, almost the whole of this metal is precipitated with the iodide of copper, to which it gives a brown color. These two metals will be easily separated by the same process which I am about to describe for the separation of manganese and cobalt.

Separation of Manganese and Cobalt from Nickel or Zinc.—The process which I propose for the separation of manganese and cobalt from nickel or from zinc, is easy of application, and gives very accurate results. It is founded on the property possessed by carbonate of manganese of being insoluble in cyanide of potassium, whereas the carbonates of cobalt, nickel, and zinc are, on the contrary, very soluble in this reagent.

On making this remark, the first idea which presents itself to the mind, is to precipitate the metals by carbonate of soda, and to add cyanide of potassium. But the carbonate of manganese will always retain carbonate of potassa which no excess of cyanide of potaasium can remove. But if the reagents are introduced in an inverse order, the operation will succeed perfectly. The solution is neutralised by pouring in carbonate of soda which precipitates nothing, then an excess of cyanide of potassium is introduced, free from carbonate and cyanate of potassa (the cyanide of potassium should be prepared by saturating potassa with hydrocyanic acid). A grey precipitate is formed, which soon becomes green, and retains that color indefinitely. This precipitate may at length be dissolved in a very great excess of cyanide of potassium. If carbonate of soda be added and the liquor boiled, the whole of the manganese is precipitated in the state of carbonate. It is not even necessary to add a very great excess of cyanide of potassium, nor to allow it to digest for any length of time; it is sufficient, after having added a suitable quantity of cyanide, to heat the liquor with the precipitate for some hours, then to introduce the carbonate of soda. If the liquor is boiling when this last reagent is added, the cyanide of manganese loses its green color as if by enchant-

ment and becomes white, being transformed into carbonate. It is collected on a filter, washed, calcined, and weighed.

To obtain cobalt when there is no nickel, the excess of cyanide of potassium must first be destroyed by an acid, then carbonate of soda is added, it is evaporated to dryness and calcined in the air. When redissolved in water, oxide of cobalt remains.

VERIFICATION OF THE PROCESS.

I dissolved in hydrochloric acid,—

Red oxide of Manganese	0·536 gr.
Oxide of Cobalt	0·672

By separating them as above mentioned, I found,—

Red oxide of Manganese	0·535 gr.
Oxide of Cobalt	0·670

The error in the quantity of oxide of cobalt may have arisen from the same quantity of oxygen not being absorbed in the second calcination as in the first.

I again dissolved 0·447 gr. of calcined oxide of manganese in hydrochloric acid, with the oxides of zinc, cobalt, and nickel. On separating the manganese as above, I found 0·478 gr. of oxide; the other metals were not estimated.

The metallic oxides and all the reagents mentioned in this Note were perfectly pure; I prepared them myself with the greatest care.

Annales de Chimie et de Physique, December, 1853.

On the Action *which* Zinc *and* Iron *exert on the* Salts *of* Sesquioxide *of* Chrome. By M. Henri Loewel.

The action which metals exert on the solutions of salts with a base of sesquioxide of chrome, appear hitherto to have been but little studied by chemists. No special work has been published respecting it, and the best chemical treatises contain no distinct information on this subject.

In a former work on chrome, which I was obliged to leave some years ago to employ myself with other investigations, I observed many interesting facts concerning the action in question, which have never been described. Not being certain whether I shall be able soon to resume this work, I now give a short summary of these facts, so that they may have their right date in case other chemists should make the same researches.

I.—Action of Zinc on Solutions of Chrome Alum. 1.—Having dissolved pure crystallised chrome alum in twice or four times its weight of boiling water, when cold, pour this green solution into a phial almost full of large grains of distilled zinc, and then close the phial with a good cork, containing a small bent glass tube, so as to collect the gas under a glass; bubbles of hydrogen gas will soon appear.

By operating in this manner, and using a solution of 50 grms. of crystallised chrome alum in 100 to 150 grms. of water, the bubbles of

hydrogen gas will succeed each other rapidly for two or three hours; then the disengagement becomes slower by degrees. After twenty-four hours there will only appear a bubble occasionally. The solution of the chrome salt, at first of a dark opaque green, becomes bluer, and after two, four, or six days (according to the surface of the zinc exposed to it, and in proportion to the warmth of the temperature), it becomes transparent, of a magnificent pure blue color, and a very voluminous precipitate will have separated in gelatinous flocks, becoming blended only by degrees.

The quantity of hydrogen disengaged is variable. In a certain number of experiments I collected from 6 to 16 cubic centimetres per grm. of chrome alum employed.

II.—Action of Zinc on Solutions of Sesquichloride of Chrome.

2.—By dissolving green crystallised sesquichloride of chrome,—($Cr^2 Cl^3$ + 12 HO), containing no free hydrochloric acid, in from three to five times its weight of water, and pouring this solution on grains of zinc, in a phial closed as above, the same phenomenon will be observed as with the solution of chrome alum. There is a disengagement of hydrogen gas, becoming slower after some hours; the opaque green solution becomes of a greenish blue, and finishes in about the same time by becoming limpid, and taking a very fine pure blue color, green gelatinous flocks separating from it, and becoming a mass by degrees.

The volume of hydrogen gas which I collected in the experiment which I made, varied between 4 and 15 cubic centimetres for each grm. of salt employed.

Blue Liquors obtained. **3**.—In experiment § 1, the sulphate of sesquioxide of chrome was transformed into sulphate of protoxide, which gives the beautiful blue color to the liquor. This liquor contains, besides, sulphate of zinc and sulphate of potassa, contained in the chrome alum. It generally deposits crystals of a double sulphate of zinc and potassa on the grains of zinc.

In experiment § 2, the sesquichloride of chrome was transformed into protochloride, whose fine blue solution contains, besides, chloride of zinc.

These two liquors possess all the properties which M. Peligot has attributed to solutions of protochloride of chrome.

When removed from the phials they become green in contact with atmospheric air.

When passed under bell glasses filled with oxygen gas over mercury, they absorb this gas with such great avidity, that the liquors rise rapidly in the bell glass; they become immediately green, passing to the state of sesquioxide and oxychloride of chrome.

By adding to a mixture of water and the violet colored anhydrous sesquichloride of chrome, the smallest possible quantity of either of these blue liquors, the violet sesquichloride dissolves instantaneously with a disengagement of heat.

Poured into a solution of corrosive sublimate, they produce a white precipitate of protochloride of mercury.

Poured into a phial containing a solution of caustic potassa, a dark brown precipitate is produced, consisting of hydrated protoxide of chrome mixed with oxide of zinc. By closing the phial with a cork with a bent glass tube, to collect the gas, and heating to ebullition, there is a disengagement of oxygen gas, in consequence of the decomposition of the water by the hydrate of protoxide of chrome, which passes to the state of intermediary oxide $Cr\,O + Cr^2\,O^3$.

I also observed that these blue liquors, poured into a solution of protochloride of tin, immediately occasion a precipitation of metallic tin in powder.

4.—Thus, in experiment § 1, the zinc removes from the sulphate of sesquioxide of chrome contained in the alum, 1 equivalent of oxygen and I equivalent of acid,

$$Cr^2\,O^3\,3SO^3 + Zn = 2\,(Cr\,O,\,SO^3) + Zn\,O,\,SO^3.$$

(Chemists admit that, in this case, 1 equivalent of water is decomposed. Its oxygen combines with the zinc and its hydrogen *in the nascent state* removes 1 equivalent of oxygen from the sesquioxide, thus again forming 1 equivalent of water.)

In experiment § 2, the zinc removes from the sesquichloride of chrome, 1 equivalent of chlorine,

$$Cr^2\,Cl^3 + Zn = 2\,(Cr\,Cl) + Zn\,Cl.$$

But, in this case, the phenomena produced during the reaction are more complex than is indicated by these formulæ. There is a certain quantity of water decomposed whose hydrogen is disengaged, and the oxygen passes to the zinc. The oxide of zinc formed, enters into solution, and displaces an equivalent quantity of sesquioxide of chrome, which, towards the close of the reaction, separates in dark green flocks. In the first experiment this sesquioxide of chrome remains combined with a certain quantity of sulphuric acid, and forms a gelatinous basic sulphate. In the second experiment, the green flocks contain a certain quantity of chlorine or hydrochloric acid, and form a gelatinous oxychloride.

If, to make the experiments § 1 and § 2 very acid solutions are taken, by adding sulphuric acid to that of the chrome alum, and hydrochloric acid to that of the sesquichloride, the zinc is then powerfully attacked, and the liquors become hot. When the powerful disengagement of hydrogen gas ceases, and the solutions become cold, they very quickly become limpid and blue, without any notable separation of gelatinous green sub-salts. It would appear, in this case, that the free acid, by attacking the zinc, gives greater energy to its electro-positive properties; and then the reaction takes place as indicated by the above formulæ. When operating with these solutions thus rendered acid, we necessarily obtain blue liquors, which contain much greater quantities of sulphate and chloride of zinc than when operating with the solutions of the neutral salts.

5. In experiment, § 2, when the limpid liquor has taken a fine pure blue color, if, instead of then stopping the operation, and unclosing the phial, the solution of protochloride of chrome is left in contact with the zinc in the apparatus described, a slight disengagement of hydrogen

will continue. This disengagement is very slow at low temperatures, approaching 32° F., more rapid at 59° and 77° F. It may be observed, after some time, when the dark green flocky precipitate has become a mass, that the solution begins to deposit a pale grey powder. This grey pulverulent deposit slowly increases in volume as the slight disengagement of hydrogen continues. The blue liquor, still preserving its azure tint, becomes paler by degrees, and after four or six months, sometimes longer, it becomes quite colorless. Then, on opening the phial, the colorless liquor is found to contain no chrome, but merely chloride of zinc. The powder deposited by the solution, being well washed and dried, is of a pale grey color. According to my analysis, it consists of hydrated oxychloride of chrome. Treated with slightly dilute, boiling sulphuric acid, it gives a green solution of sulphate of sesquioxide of chrome, disengaging hydrochloric acid gas. I have not yet exactly determined the decomposition, but I have reason to believe that it neither contains protoxide of chrome, nor intermediary oxide $CrO + Cr^3O^4$.

By leaving the reaction of experiment § 1 to continue in the same manner, when the sulphate of sesquioxide of chrome has been transformed into blue sulphate of protoxide, the same phenomena present themselves. There is a continued slight disengagement of hydrogen, and, after some time, the solution deposits, by degrees, a greenish grey powder on the zinc; but this reaction is much slower than with the solution of chlorine. In one experiment the blue liquor was not totally decolored in less than eighteen months; in another it took nearly two years. These colorless liquors only contained sulphate of zinc and sulphate of potassa. The greenish grey powder deposited was a basic sulphate of sesquioxide of chrome.

Thus, in this second phase of the experiments, the sulphate of protoxide, as well as the protochloride of chrome, which were formed in the first phase, are decomposed by the effect of a slow and very complex action of galvanic currents. The sulphate loses a portion of its acid, which dissolves the zinc, whereas, its base oxidises further to pass again into the state of sesquioxide, and form a basic salt. The protochloride of chrome loses chlorine, which combines with the zinc, and at the same time it oxidises to form an insoluble oxychloride. All these oxidations are made at the expense of the water, whose hydrogen is disengaged.

6. When the grains of zinc are put in contact with the solutions of chrome alum, or sesquichloride of chrome, in phials or other vessels which are unclosed or not tightly corked, so that the atmospheric air can enter, the results observed are not the same as those above described. A disengagement of bubbles of hydrogen gas is observed; but, after some time, the solution becomes a gelatinous mass of green basic sulphate of sesquioxide or oxychloride of chrome, without turning blue. This is easily explained by the great affinity which these blue salts have for oxygen. As they are formed under these circumstances they attract oxygen from the atmosphere, and pass again, one into the state of sesquioxide, the other into that of oxychloride; the zinc

removing the acid from the first or the chlorine from the second, at length expels them from the solution.

(To be continued.)

PHYSICS.

THERMO-CHEMICAL *Investigations Concerning* COMBINATIONS *Formed in* MULTIPLE PROPORTIONS. By M. P. A. FAVRE.

(*Continued from page* 301.)

That brought into play in the formation of some Oxygenous Combinations of Nitrogen.—In the thermo-chemical researches which I made in conjunction with M. Silbermann, we determined the heat disengaged by the decomposition of protoxide of nitrogen.

We found that 1 grm. of oxygen, in separating from the nitrogen of the protoxide of nitrogen, disengages 1090·5 units of heat. Thus the formation of protoxide of nitrogen must be accompanied by an absorption of heat.

We thence deduce, for the heat *absorbed* during the formation of 1 equivalent of protoxide of nitrogen, 8724 units of heat.

This result is deduced from the phenomena observed during the decomposition of protoxide of nitrogen by heat alone.

It would have been interesting to determine likewise the quantity of heat brought into play in the combination of nitrogen and oxygen, in order to form the binoxide of nitrogen; but this gas is not decomposable by heat alone.

The only means of arriving at the solution would probably consist in burning charcoal in binoxide of nitrogen previously heated sufficiently for the combustion to be effected regularly, as is the case with the protoxide of nitrogen. The apparatus which M. Silbermann and I used for decomposing the protoxide of nitrogen by heat,* may be applied to this experiment, by placing the charcoal in the internal platinum tube.

Before this experiment is realised, I will set forth the results furnished by the union of binoxide of nitrogen with oxygen, for forming nitrous and nitric acid.

There will finally remain to be determined the heat brought into play by the formation of binoxide of nitrogen, and the conversion of the latter into hyponitric acid to complete the series of the calorific phenomena which are produced in the oxidation of nitrogen, giving rise to compounds the study of which is most interesting as regards the law of multiple proportions.

I hope that this gap will soon be filled up.

Combination of Binoxide of Nitrogen with Oxygen to produce dilute Nitric Acid.—For arriving at this result, I relied on the action which copper exerts on dilute nitric acid, which reaction is expressed by the following equation :—

$$4(NO^5) + 3Cu + x\,Aq. = 3(CuO, NO^5) + NO^2 + x\,Aq.$$

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* "Annales de Chimie et de Physique," 3 Série, t. xxxvi., p. 11.

From the volume of binoxide of nitrogen resulting from this reaction, which volume I collected completely and measured, I was able to deduce the weight of copper dissolved.

The action taking place in the eprouvette of the calorimeter, we may deduce from the heat disengaged the effect due to the passage of a known quantity of nitric acid to the state of binoxide of nitrogen in decomposing; we know indeed the heat disengaged by the oxidation of the copper, and that which results from the union of the binoxide of copper with nitric acid to form dilute nitrate of copper.

We have :— $R = A + B - x$

Whence

A = 65655 formation of 3CuO anhydrous

R = 20652 formation of $3(CuO, NO^5)$ in dilute solution.

Experiments proved that 1 grm. of binoxide of nitrogen resulting from the decomposition of nitric acid corresponded to a disengagement of,—

I.	. . .	2184·5
II.	. . .	2171·6
III.	. . .	2247·2
IV.	. . .	2153·5
V.	. . .	2200·5
VI.	. . .	2190·2
VII.	. . .	2171·4
Mean.		2188·4 units of heat.

Whence

$$R = 2188{\cdot}4 + 30 = 65652 \text{ units of heat.}$$

And

$$x = 20655 \text{ units of heat.}$$

This number expresses the quantity of heat *absorbed* by the passage of 1 equivalent of nitric acid to the state of binoxide of nitrogen.

The following are some details concerning the precautions which were observed in performing the foregoing experiment :—

Some copper turnings, first roasted and then reduced by hydrogen, were introduced into an eprouvette with a neck, and a solution formed of equal quantities of water and pure quadrihydrated nitric acid was prepared. The acid solution thus formed did not perceptibly disengage heat when it was diluted with water, as we ascertained previously. The experiment proved that the reaction of this acid on copper disengaged only chemically pure binoxide of nitrogen, a circumstance which must be attributable especially to the absence of very great heating, owing to the mercurial mass of the calorimeter in connection with the eprouvette in which the reaction occurred.

About 30 cc. of nitric acid, diluted as we have said, was introduced into an eprouvette, which was placed in the muffle of the calorimeter; the progress of the mercurial column was then studied; when this progress was regular, we removed the eprouvette containing the acid liquid, the temperature of which was that of the calorimeter. By means of this liquid, and by operating rapidly, we first washed the

eprouvette which contained the metallic copper, and then filled it almost to the neck; we rapidly adapted the cork, fitted with the disengagement tube, and introduced the whole into the calorimetric muffle. Account was taken of the few cubic centimetres of air remaining in the apparatus.*

After about ten minutes, the reaction was detected by the disengagement of the first bubble of binoxide of nitrogen gas; the disengagement increased in activity, reached its maximum in about ten minutes, then became slower, and stopped altogether in from twenty to twenty-five minutes after the introduction of the eprouvette. So long a duration required great precautions in the observations and operations, as I have already said.

Combination of Binoxide of Nitrogen with Oxygen to produce dilute Nitrous Acid.—To obtain the calorific number sought for, I availed myself of the reaction which dilute nitric acid exerts on a nitrate, which may be formulised by the following equation:—

$$3(NO^5, MO) + 3(NO^5) + x\,Aq. = 3(NO^5\,MO) + NO^5 + 2NO^2 + x\,Aq.$$

From the want of any possible experiment relative to the direct saturation of nitrous acid by the gases, in order to determine the heat disengaged by the formation of nitrites (the decomposition of a nitrite by heat not being capable of furnishing the solution, since we obtain only products of the decomposition of nitrous acid), I have admitted, as a result which must be an approximation to the truth, that the combination of nitrous acid with a given base disengages the same quantity of heat as the combination of nitric acid with the same base. (See in support of this hypothesis the table of the combinations of the acids with the bases, "Annales de Chimie et de Physique," 3 série, t. xxxvii, p. 419). It follows that in the equation above written, the heat disengaged in the various reactions may be considered as resulting solely from the formation of 1 equivalent of nitric acid at the expense of 3 equivalents of nitrous acid, with the formation of 2 equivalents of binoxide of nitrogen.

The nitrite employed was pure nitrite of silver; but as this salt is insoluble, this circumstance must be taken into consideration for bringing the results to what they would have been, in operating on a soluble nitrite. To make this correction, it is sufficient to know the calorific effect produced by the precipitation of a known quantity of nitrite of silver.† I easily arrived at this by treating a solution of nitrite of baryta with a known weight of nitrate of silver in solution.

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* The employment of a calorimeter with two muffles greatly simplified the preliminary operations; it is only since these experiments that I have constructed a calorimeter which realises this improvement, and some others besides.

† We commenced at first by adding dissolved nitrate of silver, not weighed, to the solution of nitrite of baryta, which did not require to be pure, but simply to contain no free baryta, until the appearance of a precipitate, in order to be quite certain of the state of saturation of the liquor, from this moment all the nitrate of silver added is converted into nitrate of silver, which is precipitated.

On one hand,

1 gr. of nitrite of silver, decomposed by dilute nitric acid,* disengages,—

I.	37·90
II.	37·96
III.	37·34
Mean	37·73 units of heat.

By multiplying 37·73 by 462 gr., the weight of 3 equivalents of nitrite of silver, we have 17431 units of heat.

On the other hand,

1 gr. of nitrite of silver disengages in precipitating,—

I.	49·98
II.	49·89
Mean	49·93 units of heat.

Whence

$49·93 \times 462 = 23068$ units of heat disengaged by 3 equivalents of nitrite of silver in being precipitated.

The sum $23068 + 17431$

gives $R = 40499$ units of heat.

Now the heat disengaged by the reaction results from the heat disengaged by dilute NO^3 becoming NO^5 in solution., less the quantity of heat which seems to have been absorbed by $2NO^3$ dilute, becoming $2NO^3$.

Indeed $3NO^3 + xAq = 2NO^2 + NO^5 + Aq.$

Although these two elements are unknown, we may arrive at the result sought for, by remarking that $2NO^2$ being fictitiously transformed into $2NO^4$ dilute, would disengage a quantity of heat which may be known according to a determination reported above.

Let A be the quantity of heat disengaged by $2\,NO^3$ becoming $2\,NO^4$ dilute, a quantity which is known, we shall have

$$3x = R + A,$$

x expressing the heat disengaged by 1 equivalent of nitrous acid in passing to the state of nitric acid.

Now

$$A = 2 \times 20655 = 41310,$$

We shall have, therefore,

$$x = 27269 \text{ units of heat ;}$$

a number which represents the heat *disengaged* by 1 equivalent of nitrous acid passing to the state of nitric acid.

By subtracting this last number from that which applies to the

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* When nitrite of silver is decomposed with dilute nitric acid we are informed of the complete decomposition of nitrous acid, set at liberty, by the disappearance of the very slight blue coloration existing at first. We avoid the oxidation of the binoxide of nitrogen resulting from the reaction, in the interior of the calorimetric eprouvette, by directing into the latter a current of carbonic acid saturated with moisture to expel the air and to prevent it from penetrating during the operation.

transformation of binoxide of nitrogen into nitric acid, we have a negative result.

$$= -6614 \text{ units of heat.}$$

Thus the passage of binoxide of nitrogen to the state of nitrous acid is accompanied by an *absorption* of heat represented by the above number.

It is remarkable to see the binoxide of nitrogen (a very stable body, and acting in certain respects the part of a simple body), *absorb* heat in forming nitrous acid, the same as we have seen nitrogen absorb heat in forming protoxide of nitrogen.

Protoxide of nitrogen and nitrous acid present the common character of being decomposable by heat: the former into nitrogen and oxygen, the latter into nitrogen and hyponitric acid. It must be borne in mind, moreover, that hyponitric acid and binoxide of nitrogen resist the action of heat, as M. Silbermann and I have proved in former researches.

The whole of the results relating to the heats brought into play in the oxidation of nitrogen at various degrees, do not enable me yet to conclude on the number to represent the calorific equivalent of nitric acid; that is to say, the calorific effect resulting from the union of 1 equivalent of nitrogen with 5 equivalents of nitrogen. Indeed, I have only determined the heat *disengaged* by the binoxide of nitrogen passing to the state of nitric acid, or NO^2 transformed into NO^4, represented by 20655 units of heat. When experiment shall have made known the heat brought into play by the formation of binoxide of nitrogen, and when this precise datum shall be added to the experiments detailed in this work, we shall be able to solve completely some interesting problems relative to the quantities of heat disengaged by the reaction of the elements of which gunpowder consists.* But at present the results at which I have arrived appear to me of a nature to warrant some interesting reflections, as regards the theory of the deflagration of powder, and the chemical part to be performed in the phenomena presented by fulminating powders.

Making allowance for the rapidity of inflammation of the various mixtures capable of constituting fulminating powders, it is certain, 1st., that the quantity of gas developed by a given weight of powder; 2nd, that the temperature produced at the moment of the reaction, are two elements which may influence the force of expansion of the powder. With an equal speed of inflammation (consequently the cooling by contact with the weapon being equal), we may be able to have very different forces of expansion for the same weight of various powders, even when the gaseous volume measured in the same circumstances, is identical in each case. It is sufficient for that, that the heat disengaged by the chemical reactions should be very different.

* I am quite aware how much this deficiency is to be regretted, and I wish that I had been able to supply it immediately: unfortunately this experiment requires the employment of the same apparatus as were used for the vivid combustions of charcoal, and it would have required arrangements which circumstances did not admit of my making at the time.

For ordinary powder, admitting the following reaction,

$$NO^5, KO + 3C + S = KS + 3CO^2 + N,$$

135 grms. give 8 vols. of gas (the volume of the equivalent of oxygen being = 1).

It is clear that the complete decomposition of the nitrate of potassa should give rise to a considerable absorption of heat, and that this effect must considerably diminish the heat disengaged by the formation of 3 equivalents of carbonic acid and of 1 equivalent of sulphuret of potassium; a calorific effect which the prior determinations enable us to calculate.

Here is at most the account of the heat disengaged by the combustion of 1 grm. of powder, admitting the composition and the reactions as shewn in the above equation, and *in the hypothesis in which the decomposition of nitric acid would give rise to an absorption of heat equal to the heat disengaged by the passage of* NO^2 *to the state of* NO^5, or 20655 units of heat for 1 equivalent of NO^5; this number should probably be a minimum, which will make the final result of our calculations too high.

The reaction R comprises the following effects:

D=119523, heat absorbed by the complete decomposition of crystallised nitrate of potassa into nitrogen, oxygen, and potassium.

A=145440, formation, of 3 equivalents of carbonic acid.

B=45638, formation of 1 equivalent of anhydrous sulphuret of potassium.

Whence

$$R = A + B - D = 71555 \text{ units of heat.}$$

This number, divided by 135, gives

530 units of heat

disengaged by the combustion of 1 grm. of ordinary powder.

If we now pass to the consideration of the calorific effects produced by the reaction of sulphur and charcoal on chlorate of potassa, it will be easy to see, *à priori*, that they must be enormous, and that they must contribute to render the powder explosive. Indeed, with the exception of the heat absorbed by the separation of chloric acid and potassa, and of the decomposition of the potassa, all the other reactions give rise to disengagements of heat; for chloric acid, in being decomposed, disengages a considerable quantity of heat, as takes place with nitric acid, as my experiments prove.

It was already known that powders made with chlorate of potassa were explosive; this effect was attributed only to the great speed of the inflammation. Now, it is evident that the speed of inflammation should increase in the case of reactions which disengage more heat, as is the case with chlorate powders.

Moreover, all the elements of the calculation of the heat produced by the combustion of a chlorate powder exist.

Let us suppose this powder to be formed of 1 equivalent of chlorate, 1 equivalent of sulphur, and 2 equivalents of carbon.

Admitting that the reaction is represented by the equation

$$ClO^5, KO + S + 2C = KCl + SO^3 + 2CO^2$$

150 grms. give six volumes of gas (the volume of 1 equivalent of gas being = 1).

This powder, compared with the preceding, will disengage then less gas for an equal weight; but it is easy to see that the heat developed must render the expansive force much greater than in the preceding case.

The following are the elements of the calculation :—

D=23256, calorific equivalent of crystallised chlorate of potassa.
E=76238, calorific equivalent of dissolved potassa.
A=65232, calorific equivalent of dissolved chloric acid.
B=96960, twice the calorific equivalent of carbonic acid.
C=35528, calorific equivalents of gaseous sulphurous acid.
C'=100960, calorific equivalent of crystallised chloride of potassium.

Whence

$$R = B + C + C' - (D + E + A) = 199188 \text{ units of heat.}$$

This number divided by 150, gives 1328 units of heat disengaged by the combustion of 1 grm. of chlorate powder.

On comparing the reactions and referring them to 1 grm. of each powder, we see that,—

1 grm. saltpetre powder would disengage 550 units of heat,
1 „ chlorate powder „ 1328 „

I may remark, however, that the foregoing calculations, supposing even that the calorific effects of all the reactions were well known, still could give only an approximative idea of the phenomena, for it is known that in the deflagration of the powder, the reactions are not so clear as the foregoing equations indicate.

The conditions of deflagration varying, the gaseous products and fixed residues of the combustion may vary also. Admitting even for the moment that it was possible to construct a calorimeter capable of satisfying this kind of determination, it would still be necessary to analyse the products of the deflagration after each calorimetric experiment.

It is, therefore, much preferable to analyse in each particular case the products of the reaction, and to apply the calorific numbers in consequence of the proportions of the combinations formed. The solution of the problems will depend on these various elements collectively.

MM. Pouillet and Peligot, in a work still unpublished, have studied the products of the combustion of powders of various proportions, burned in very different conditions, by determining each time the volume of the gases and the composition of the products of combustion.

I trust that these calorific elements, which I hope soon to render more complete, may be of some utility when applied to the analyses which the gentlemen whom I have just named will doubtless publish.

(To be continued.)

RESEARCHES *on* CHEMICAL AFFINITY. By M. F. MARGUERITE.

IN a mixture of two salts in solution, the two following conditions may be met with :—

1st. The least soluble combination possible may not pre-exist among the salts employed; then, by virtue of the law of Berthollet, a double decomposition takes place, which, by immediate precipitation or by evaporation, produces this least soluble salt.

2nd. The least soluble combination may pre-exist among the salts employed; the action of insolubility is accomplished · it cannot therefore cause any decomposition in the liquor.

In the first case, the law of Berthollet comes into action; the consequences are easily foreseen.

In the second case, we do not know the state of the salts in solution. This is precisely the point which we now wish to ascertain.

If a mixture of chlorate of potassa and chloride of sodium is put in contact with water, the question is to know whether the solution contains salts employed and constituted according to the law of Berthollet—namely, chlorate of potassa and chloride of sodium, or the four following salts :—

Chlorate of potassa, chloride of potassium.
Chloride of sodium, chlorate of soda,

resulting from a partial decomposition of the primitive salts. It may be proved that the liquor contains the four salts, or, what is the same thing, that the chloride of sodium decomposes the chlorate of potassa in the following manner:—

A saturated solution is formed at the ordinary temperature of the least soluble salt, the chlorate of potassa, to which the chloride of sodium is added in crystals.

If this second salt only changed its state, and dissolved without any other reaction, the properties of the liquor with regard to the first salt would undergo no modification, and above all, its affinity for it would not augment.

Nevertheless, a liquor saturated with chlorate of potassa will dissolve an excess of this salt after the addition of chloride of sodium.

A series of experiments which I cannot describe here, shews numerous examples of the solution of an excess of salt in its saturated solution under the influence of another salt; and it must be remarked that whenever this extra solution takes place, there may be produced, by double decomposition, a more soluble salt than the least soluble of the salts employed, namely, the opposite result from that of the law of Berthollet.

This phenomenon of the extra solution thus finds a simple and natural explanation; the least soluble salt being transformed into another more soluble salt, is dissolved in the liquor which refused to admit it because it was saturated.

A proof of this double decomposition is found in the changes which are exhibited by the reaction on litmus of the salts after their mixture.

Thus, a solution of sal-ammoniac, which is acid to litmus paper,

manifests an alkaline reaction after the addition of carbonate of baryta, lime, or strontia.

This alkalinity of the liquor can only be attributed to the carbonate of ammonia, which is formed by double decomposition, producing chloride of barium, calcium, or strontium, whose reaction is almost neutre, and inappreciable in these circumstances.

There can be no doubt as to the production of carbonate of ammonia (a piece of turmeric paper hung over the liquor enables us to prove it), and that of chloride of barium or calcium; for oxalate of ammonia shews the presence of baryta or lime in the solution in a quantity which exceeds the natural solubility of the carbonates of baryta and of lime.

It is difficult to multiply these experiments, for it almost always happens that the reactions peculiar to the mixed salts, or those of the combinations which are supposed to be formed, compensate for and destroy each other in such a manner that the reaction indicating the double decomposition is almost concealed.

It is therefore necessary to have recourse to another order of facts to find the proof of these double decompositions, and the precise indication of the nature of the salts in solution.

Sal-ammoniac is precipitated from its saturated solution by a very small quantity of nitrate of ammonia.

If, in a saturated solution of hydrochlorate of ammonia, a different salt is dissolved—chlorate of potassa for instance—no apparent change is produced in the liquor, and we do not know whether any double decomposition takes place, or even whether the hydrochlorate of ammonia remains intact.

Nitrate of ammonia can decide the question; for if it precipitates no hydrochlorate of ammonia, it is because there is no longer any in the liquor. The following is what occurs:—

A much more considerable quantity of nitrate of ammonia than usually acts, precipitates no hydrochlorate of ammonia from its saturated solution when it contains chlorate of potassa. Whence I conclude that the hydrochlorate of ammonia has been destroyed by the chlorate of potassa; and that, consequently, the liquor contains chloride of potassium and chlorate of ammonia. Moreover, the sal-ammoniac precipitated from its saturated solution by nitrate of ammonia, is redissolved under the influence of chlorate of potassa; which is another proof of the transformation of the sal-ammoniac into chloride of potassium.

Analogous reactions take place with nitrate of baryta and chlorate of potassa, sulphate of potassa and nitrate of soda, nitrate of potassa and hydrochlorate of ammonia, sulphate of soda and nitrate of ammonia, and chloride of sodium and nitrate of ammonia.

For, after the mixture of these compounds, the salts which should displace nitrate of baryta, sulphate of potassa, nitrate of potassa, sulphate of soda, and chloride of sodium, are without action upon them.

Is not the double decomposition clearly shewn, and is it not evident that the original salt has been modified, since it is no longer preci-

pitated by the salt which would certainly displace it if it remained in the liquor?

Thus, the solution of an excess of salt in its saturated liquor, under the influence of another salt; the changes which the litmus paper shews in the new reactions of the salts after their mixture; the continuance of the original salt, or rather of its elements in the liquor, notwithstanding the addition of a salt which ought to precipitate it, are so many characters which establish that the collective solution of two salts does not consist only in the change of their respective states, but in a reciprocal reaction which gives rise to two new salts.

The experiments described in the Memoir from which I am making an extract, prove that all these double decompositions take place according to a general principle which may be formulised in the following manner:—

When, by the mixture of two salts which have satisfied the law of insolubility, there is formed a more soluble salt than the least soluble among them, the action of the water always determines its formation in certain limits.

It is, then, the affinity of the solvent—the force of solubility, as it may be called—which forms the elements into groups, according to its tendency to form a combination more soluble than the least soluble, in the same manner that the repulsion of the solvent, or the force of insolubility, determines the formation of a less soluble salt than the least soluble of the salts employed.

As we have seen, these two forces are in constant antagonism, whence it results that the effects of each are not absolute, but merely relative.

I shall now cite some examples, which shew that the force of solubility does not cease acting even when it might be supposed to be quite overcome by the force of insolubility.

The sulphate and carbonate of baryta, the oxalate and corbonate of lime are considered as completely insoluble salts, for the solubility of sulphate of baryta is only 1–200,000th; carbonate, 1–14,137th; oxalate of lime, 0–0 or x; carbonate of lime, 1–16,000th; and it would seem that, when one of these combinations is precipitated, the mother liquor should only retain a quantity in proportion to its solubility. Nevertheless, these compounds are not precipitated in certain circumstances in the presence of salts which are able to convert them into more soluble compounds.

In a theoretic point of view, and for the purpose of analysis, these results should be taken into consideration, for they shew how powerful the influence of the solvent is on the disposition of the elements, in a word, on chemical affinities.

An alcoholic medium clearly shews the effects of the elective affinity of the solvent with regard to the elements engaged in insoluble combinations.

I shall confine myself to the following example:—It is well known that by adding alcohol to a liquor containing sulphate of lime, that salt is precipitated. However, if to this liquor be added a certain

quantity of nitrate or hydrochloride of ammonia, nitrate of soda or of potassa, chloride of sodium or of potassium, the alcohol no longer precipitates the sulphate of lime.

In these circumstances, the alcohol having an affinity for nitrate of lime or chloride of calcium, which it can dissolve, determines its production, destroying the sulphate of lime, which, no longer existing in the liquor, is not precipitated.

It is again because of the principle of solubility that these decompositions take place; for there is the formation of a more soluble salt than the least soluble of the salts employed.

I must add that this evident influence of the solvent on the union of elements, by double decomposition, is sufficiently powerful to cause partially the simple and direct displacement of an energetic base by a feeble base, and the elimination of a strong by a weak acid; in other words, to cause the division of bases and acids.

From this it would appear that the solvent always decides the combinations which should be formed, and that the reciprocal affinities of bases and acids may have but little influence on the result of double decompositions; so that it would be more correct to attribute the formation of any salt to the direct affinity or repulsion of the medium for the soluble or insoluble compound, than to attribute it to the peculiar or relative affinities of the elements, which are moreover known to be easily controverted in double decompositions.

Comptes Rendus, December 12th, 1853.

ELECTRO METALLURGY.

ELECTRO-DEPOSITION *of* ALUMINIUM *and* SILICIUM.

By GEORGE GORE, ESQ.

To obtain the aluminium, I boil an excess of dry hydrate of alumina in hydrochloric acid for one hour, then pour off the clear liquid, and add to it about one-sixth of its volume of water; in this mixture I place an earthen porous vessel containing one measure of sulphuric acid to twelve measures of water, with a piece of amalgamated zinc plate in it. In the chloride of aluminium solution I immerse a piece of copper of the same amount of immersed metallic surface as that of the zinc, and connect it with the zinc by means of a copper wire, and set it aside for several hours; when on examining it, I found it coated with a lead colored deposit of aluminium, which, when burnished, possessed the same degree of whiteness as platinum, and did not appear to tarnish readily by immersion in cold water or in the atmosphere, but was acted upon by sulphuric or nitric acids, either concentrated or dilute.

I found that if the apparatus was kept quite warm, and a copper plate much smaller than the zinc plate was used, the deposit appeared in a very short time, in several instances in less than half a minute. Also I found that if the chloride solution was not diluted with water, the deposit was equally, if not more, rapid.

I have also succeeded in obtaining a quick deposit of aluminium in a less pure state by dissolving ordinary "pipe-clay" in boiling hydrochloric acid, and using the supernatant clear solution undiluted with water in the place of the beforementioned liquid. A similar deposit of aluminium was also obtained from a strong aqueous solution of acetate of alumina, likewise from a saturated aqueous solution of ordinary "potassa alum," but rather slowly; with each of the solutions named, the deposit was hastened by putting either one, two, or three small Smee's batteries in the circuit.

To obtain the deposit of silicium, I dissolved monosilicate of potassa (formed by fusing together 1 part of silica with 2¼ parts of carbonate of potassa) in water, in the proportion of 40 grs. to 1 oz. measure of water, proceeding in like manner as with the alumina solutions, the process being hastened by interposing one pair of small Smee's battery in the circuit. With a very slow and feeble action of the battery, the color of the deposited metal was much whiter than that of the aluminium, closely approximating to that of silver; its other properties I have not yet had time to examine.

Phil. Mag. March 1854.

INDUSTRIAL CHEMISTRY.

On ALUMINIUM *and its* CHEMICAL COMBINATIONS.

By M. H. SAINTE-CLAIRE DEVILLE.

IT is well known that M. Wöhler obtained pulverulent aluminium by treating chlorine with potassium. By modifying M. Wöhler's process, the decomposition of chloride of aluminium may be regulated so as to produce a sufficient incandescence for us to see the particles of this metal agglomerate and resolve themselves into globules. By taking a mass composed of the metal and chloride of sodium (it is better to employ sodium), and heating it in a porcelain crucible to a bright redness, the excess of chloride of aluminium will be disengaged, and there will remain a saline mass with an acid reaction, in the middle of which will be found larger or smaller globules of perfectly pure aluminium.

This metal is as white as silver, malleable and ductile in the highest degree. However, on working it, it will be found to resist considerably, and its tenacity might be supposed nearly that of iron. It hardens, but warming again restores its ductility. Its fusing point is very near that of silver. Its density is 2·56. It may be melted and poured in the open air without sensible oxidation. It is a good conductor of heat.

Aluminium is completely unalterable either in dry or humid air; it does not tarnish, and remains brilliant where freshly cut zinc or tin lose their polish. Sulphuretted hydrogen has no action on it. Neither cold nor boiling water will tarnish it. Nitric acid, whether weak or concentrated, nor weak sulphuric acid employed cold, will take any effect upon it. Its real solvent is hydrochloric acid; it disengages

hydrogen, and sesquichloride of aluminium is formed. Heated to redness in gaseous hydrochloric acid, it produces dry and volatile sesquichloride of aluminium.

It will easily be understood that a metal as white and unalterable as silver, which does not tarnish in the air, which is fusible, malleable, ductile and yet tough, and which has the singular property of being lighter than glass, would be most useful if it could be easily obtained. If we consider, besides, that this metal exists in considerable proportions in nature, that its ore is argil, we may well desire that it should become of general use. I have much hope that it may be so; for chloride of aluminium is decomposed with remarkable facility by common metals at a high temperature, and a reaction of this nature, which I am now endeavoring to realise on a larger scale than a mere laboratory experiment, will decide this question in a practical point of view.

Comptes Rendus, No. 6, February 6th, 1854.

AGRICULTURAL CHEMISTRY.

On some AGENTS *for the* PRESERVATION *of the* NITROGENOUS MATTERS *in* MANURES. By M. PAYEN.

AT no time have the results of scientific investigations been received so willingly, nor tried by agriculturists with so much zeal, as now.

Having been a witness of the happy results of this in several farms, and seeing the interest which my former experiments have excited, I have resolved to deserve the confidence which agriculturists have placed in me, by continuing my investigations on the effects of various agents used to render slower the fermentation of nitrogenous manures, and to lessen the unpleasant emanations and the amount of ammoniacal loss.

In the experiments which I am about to lay before the Academy, I proposed to determine by the same experimental means :—

1st.—The analogies or differences offered by the comparative influences of the hydrate and carbonate of potassa in the conditions in which I studied, likewise comparatively, the influences of the hydrate and carbonate of lime.

2nd.—To prove the preservative action which might be exerted on urine by sulphuric acid, in the same conditions in which it had already proved itself so powerful in stopping or delaying the putrefaction of blood.

3rd.—Likewise to examine, by means of comparative analyses, the effects, in the same conditions, of wood and coal soot employed in some places to diminish the bad emanations from urine; one of my colleagues, M. Quatrefages, wished me to make this determination so as to appreciate the utility of the first of these agents for this purpose.

4th.—To ascertain the effect of salt added to manures, and thus likewise to fulfil the desire of M. de Gasparin.

5th.—Finally, to investigate what effect was produced by alum employed to prevent ammoniacal emanations from urine?

This series of experiments commenced on the 6th of October on cow's urine. All the liquid was divided into portions of 50 cubic centimetres; one of them, as a point for comparison, was analysed in the normal state after having been mixed with oxalic acid* in slight excess and 0·3 of fine sand, then evaporated to dryness.

The other samples were mixed with proportions, which will be indicated further on, of different substances, then abandoned for thirty-six days to spontaneous reactions in incompletely closed vessels, and comparatively with a sample of the same urine without any addition.

At the end of this time each of the liquids was evaporated on a sand bath, the division of the matter being facilitated, towards the close, by means of an equal volume of plaster.

During the whole time of these spontaneous reactions the temperature varied between 64° F and 75° F.

The following Table shews the results obtained, calculated for 100 cubic centimetres, and arranged according to the order from the greatest preservation to the least, of this series of analyses.

	Cow's Urine.	Nitrogen in 100 cc.	Loss pr cent of nitrogen.
1st.	100 cc. + 2 grs sulphuric acid ($HO\,SO^3$)	0·955	0·0
	100 cc. + oxalic acid and fine sand evaporated directly	0·930	2·6
	100 cc. + 1 grm sulphuric acid .	0·915	4·1
2nd.	100 cc. + 2 grs hydrate of potassa .	0·873	8·4
	100 cc. + 1 grm „ . .	0·498	47·8
3rd.	100 cc. + 2 grms coal soot . .	0·140	85·3
	100 cc. + 2 grms wood soot . .	0·102	89·3
4th.	100 cc. + 1 grm carbonate of potassa .	0·081	91·5
	100 cc. + 2 grms „ .	0·072	92·4

Some clear conclusions and some remarkable analogies may be drawn from the results methodically inscribed in this table.

We will divide the distinct results into four groups, corresponding to the four species of agents employed.

The first group comprehends sulphuric and oxalic acids: it is seen that the presence, as well as the reaction of 2 grms of sulphuric acid in 100 cubic centimetres of urine preserved this urine for thirty-six days from all loss of ammonia; half the quantity preserved 96 per cent., or only allowed the loss of 4 per cent. of the whole nitrogen; finally, oxalic acid, in an immediate evaporation, preserved more than 0·97 of the nitrogen of the urine.

* Some time before we had used oxalic acid to fix nitrogen or ammonia, M. Ville had communicated to me a similar application which he had made of it in some investigations into the composition of rain waters.

The second group shews the effect of hydrate of potassa or caustic potassa ; 2 grms of this hydrate which sufficed to preserve from loss for thirty-six days and during the final evaporation in the sand-bath, more than 91 per cent. (0·916) of the nitrogen, manifesting an efficacy analogous to that of lime in similar circumstances, whereas an insufficient dose of one half the quantity, of the same hydrate of potassa, only preserved 0·52, that is, suffered 548 per cent. of loss in the ammoniacal substances or corresponding nitrogen.

The third group is composed of the soot of coal and wood ; in the proportion of 2 per cent., they respectively permitted the loss by putrefaction during thirty-six days and subsequent evaporation, of 85 and 89 per cent. of the original nitrogen. These substances were able to modify the odor of the urine without sensibly stopping the ammoniacal emanations.

The fourth group contains only carbonate of potassa employed in doses of 100 and 200ths. In these proportions the carbonate has rather augmented than diminished the losses, for the loss rose to 92·4 per cent.

It is worthy of remark that the influence of the alkaline carbonate acts in the same manner as carbonate of lime, and accelerates the ammoniacal loss, whereas caustic potassa in the same proportion has, as hydrate of lime, a very powerful preservative influence.

The next series of experiments and analyses were undertaken for the purpose of verifying comparatively the effects of sulphuric acid and soot, then to determine the influence which common salt may exert in different proportions ; finally, to ascertain the effect of alum.

100 cubic centimetres of urine were analysed directly in the normal, liquid state, or previously saturated with oxalic acid, and mixed with fine sand.

The reactions in other mixtures lasted during thirty-one days in incompletely closed vessels. The mixture with alum was evaporated and analysed after six days. The temperature of the surrounding air varied from 64° to 73° F. during the spontaneous reactions.

The following are the results, classified as before. The analyses were made in the same conditions as the preceding.

	Cow's Urine.	Nitrogen in 100 cc.	Loss pr cent of nitrogen.
1st.	100 cc. + 2 grs sulphuric acid . .	1·500	0·0
	100 cc. + 1 „ potassa alum (aft. 6 days)	1·476	1·6
	100 cc. after dessiccation, with oxalic acid and fine sand	1·454	3·0
2nd.	100 cc. analysed in the normal, liq. state	1·428	4·8
3rd.	100 cc. + 5 grms of common salt . .	1·422	5·2
	100 cc. + 2 grms „ . .	0·240	86·0
4th.	100 cc. evaporated alone . . .	0·193	87·0
5th.	100 cc. + 2 grms of coal soot . .	0·166	88·9

By these results it will be perceived that sulphuric acid again holds the first rank among agents for the preservation of the nitrogenous matters of urine; oxalic acid and alum are placed very near it; it is true that with regard to the latter, the duration of the experiment was limited to six days instead of extending over thirty-one. The direct analysis of the liquid and normal urine lost a little more, or 4·8 per cent.

In the two experiments with common salt the proportion of this salt had a very great influence; for 5 grms in 100 cubic centimetres retained for thirty-one days and after evaporation on the sand-bath, nearly 95 per cent. of nitrogen; whereas 2 grms in the same volume of urine had scarcely any effect: they only retained 14 per cent. in the same circumstances; and the urine without any addition left for the same time and evaporated in the same manner retained 13 per cent. of its nitrogen.

It may likewise be observed in these comparative experiments and analyses, that 2 grms of coal soot to 100 cubic centimetres were without any useful effect, and only retained 12 per cent. of the original nitrogen.

The next series of experiments was undertaken with the view of comparing the influence of hydrate of lime, sulphuric acid, and oxalic acid. It likewise shewed the notable influence of temperature on these reactions as might have been expected.

The temperature of the surrounding air was about 59° F. in the day and 37° F. during the nights, from the 2nd of December to the 26th. The evaporation in the sand-bath followed this.

Cow's Urine.	Nitrogen in 100 cc.	Loss pr cent of nitrogen.
100 cc. of urine + 2 grms sulphuric acid .	1·575	0·0
100 cc. evaporated after the addition of oxalic acid and sand	1·571	0·31
100 cc. analysed in the normal state . .	1·570	0·31
100 cc. + 1 grm of hydrate of lime . .	1·555	0·90
100 cc. left without any addition for 24 days .	1·135	27·70

The inspection of this Table shews that the preserving property of sulphuric and oxalic acids are very powerful, and very nearly equal; that hydrate of lime comes next, and occupies a high rank among antiseptic agents, in those circumstances where all the losses were much lessened by the low temperature of the days, and especially of the nights.

In the application on a large scale of sulphuric acid for the preservation of urine and manures, this energetic agent would be especially useful when we wish to use mixtures as manure on land already sufficiently provided with carbonate of lime; there would be much benefit in adding the acid as soon as possible after the emission of the urine, so as to arrange the proper amount. But even when ammoniacal com-

pounds have been formed by fermentation, the acid added in proper proportions (probably then from 3 to 5 per cent), would fix the ammonia and prevent, or, at any rate, retard, the fermentation, as well as the ultimate loss.

In these circumstances sulphuric acid would be preferable to lime, which must be always added to fresh urine; for, introduced after a greater or less advanced fermentation, this base would occasion great loss by removing the carbonic acid, and setting the ammonia at liberty.

Comptes Rendus, No. 2, January 2th, 1854.

PATHOLOGICAL CHEMISTRY.

Chemical Experiments *on the* Liquids *of* Persons *who take* Preparations *of* Iodine *internally*. By Dr. Namias.

The beautiful investigations of M. Bernard, relative to the elimination of iodine by the saliva, are well known. The following are some experiments of Dr. Namias, with the view of ascertaining some pathological particulars relative to this elimination, both by the saliva and the urine. We only give the conclusions at which he arrives.

1.—Iodide of potassium more easily penetrates the circulation than the iodised emulsions.

2.—M. Foucard and M. Marchal were wrong in concluding, from the small quantity of iodine which appeared in the urines, that the iodised emulsion remained longer in the economy than iodide of potassium.

3.—Iodised emulsions sometimes act more powerfully in certain cases, because of peculiar properties which belong to them.

4.—The presence of iodide of potassium may be ascertained in the urine and saliva, but for a longer time in the first than in the second.

5.—The contrary of the above is observed in certain complaints of the kidneys; and as then the supplementary action of the salivary glands is insufficient to free the organism from this heterogeneous principle, it follows that the urine often contains it as long as eighteen days after having ceased taking it.

6.—The kidneys play the principal part in the expulsion of iodide of potassium; the continuance in the system of this heterogeneous substance may, therefore, be considered as a symptom of some complaint of those organs.

7.—Whenever physicians prescribe this agent, they ought to examine the urine, to discover how long it remains in the system; if for long, it should be discontinued or administered in small doses.

8.—It may happen, either from the too long persistence of this therapeutical agent in the organism, or from some other peculiar condition, that iodine may finally enter the composition of the immediate organic materials.

Journal de Chimie Médicale, January, 1854.

THERAPEUTICS.

On the VARIETIES *of* MANNA *not* PRODUCED *by the* ASH.

By Dr. X. LANDERER, Honorary Member of the Pharmaceutical Society of Great Britain, Pharmacien to H. M. the King of Greece, &c.

IN a classification of the varieties of Manna, the following substances are usually included:—

1. *Manna laricina sive Brigantina*, a saccharine substance found upon the leaves of *Larix Europæ*.

2. *Manna cedrina* appears in small globules on the branches of *Pinus Cedrus*. It is brought from Mount Lebanon, where a very small quantity of two or three drachms fetches from 30 to 40 piastres.* In Syria it enjoys considerable reputation as a remedy in the *Marás* or phthisis, and is an ingredient in electuaries, &c.

3. *Manna celastrina.*

4. *Manna quercina.*

5. *Manna Australis*, produced by *Eucalyptus resinifera.*

6. *Manna cistina sive labdanifera*, a variety, with us in Greece, of the utmost rarity. On one occasion, in traversing a plain of considerable extent, abounding with *Cistus salviæfolius, C. villosus*, and *C. Creticus*, I found some drops of a saccharine exudation adhering to the stems of these plants, which I suppose should be regarded as Cistus-Manna.

7. *Manna alhagina.*

8. *Manna tamariscina.*

Upon the last two species of Manna I will offer a little information.

The Tamarisk-Manna, also known as *Manna Israelitarum*, is produced through the puncture of *Coccus manniparus*, an insect inhabiting the trees of *Tamarix mannifera*, which grows abundantly in the neighborhood of Mount Sinai. The monks from the monasteries of the district collect the saccharine secretion which exudes as a thick, transparent syrup, covering the smaller branches from which it flows. The collection of the Manna takes place in August; it requires to be performed very early in the morning, at which time, owing to the coolness of the night, the saccharine juice has become to some extent congealed. Later in the day, the solar heat causes it to drop upon the ground. When collected it is usually stored away in large earthen vessels, which are preserved in cellars during the entire year. To strangers visiting the monasteries of Sinai, the Tamarisk-Manna is sold in little vessels of tinned-iron. One of these I purchased of a pilgrim who had been in Palestine. The Manna was a yellowish, granular, syrupy mass, very sweet, and intermixed with the little leaves of the Tamarisk. It dissolved in water or in alcohol, and the aqueous solution readily fermented; the alcohol obtained by distillation had a peculiar odor, resembling that derived from the fruits of *Ceretonia siliqua*, which contains Butyric Acid. The Manna is eaten in Palestine and in the neighborhood of Sinai as a delicacy, and is reputed efficacious in diseases of the chest.

* The Turkish *piastre* is worth about 4*d.* sterling.—ED. *Ph. J.*

The Alhagi-Manna is the exudation of *Hedysarum Alhagi*, Linn., a plant indigenous to Arabia, and growing also in the maritime districts of Greece.

In the latter country it is, however, extremely rare to find the saccharine matter exuded, though by no means unfrequent in Asia Minor, and especially in Arabia and Palestine.

Extensive plains are there entirely covered with the *Hedysarum*, which one may observe appears to afford the Manna chiefly through the wounds occasioned by the browsing of the sheep and goats. It is collected by the leaders of the caravans and by the Arabs who cross the deserts, and who avail themselves of this Manna as nutriment. It is this substance which appears to me to be the *mel ex aëre* of Pliny, and the *humor melleus* of Theophrastus.

In conclusion, I will add a few words respecting the Cedar-Manna, which occurs as a great rarity on the branches of the cedar of Lebanon in the form of transparent resinous drops, indubitably the result of the punctures of an insect. The monks of the Lebanon collect this resinous matter, preparing with it various *mantsuns* and *melheme* (*i. e.*, electuaries and ointments), which are sold to strangers visiting the monasteries.—*Pharmaceutical Journ.* March, 1854.

OXYGEN GAS *in* CHOLERA. *Extract of a Letter from* DR. THOMAS O'BRIEN, Calcutta, *to* DR. CORRIGAN.

'I REGRET to see that cholera is again in England, and I suppose has by this time reached Ireland. This awful scourge is always here, but the people do not at all think it contagious; and natives attacked by it are received into any of the hospitals, and placed with all classes of patients, either medical or surgical. The saline treatment seems to have become general here, but I cannot say much for its success. I never give opium. I give calomel freely, combined with camphor. The mixture I give is sp. vin. camphorat., creosote, brandy, and soda water. I have always given oxygen gas, which I did when in the Galway union, where this treatment was equally successful, as you can see by my reports to the Board of Health. This gas is easily given by shaking powdered chlorate of potassa on a heated surface, and making the patient inhale the fumes. When the vomiting is severe, I have found firing over the stomach and abdomen avert it, and produce heat; but the oxygen gas is that upon which I would place most reliance. Transfusion has been often tried, but without success. Should you have an opportunity of trying the gas, will you do so? and let me know the result. I always give as much cold water as can be swallowed, and where the evacuations are copious, I have injected chicken broth with O'Beirne's tube; this has sometimes been retained for a considerable time."

[Oxygen gas may be readily generated by intimately mixing chlorate of potash with 1-6th its weight of black oxide of manganese, and throwing the mixture on an iron shovel heated to dull redness.—NOTE OF EDS. OF "D. H. G."]

"Dublin Hospital Gazette."

Guaco, *an* Antidote *for* Snake Bites.

A recent announcement of the successful employment of a drug called *Guaco* in some cases of deafness, has induced inquiry for it in London.

We think it desirable to point out that the name *Guaco* is applied in Central and South America to *various* medicinal plants, which are considered either as prophylactics, or as antidotes to the bites of venomous serpents.

"Great misconception," writes Dr. B. Seemann,* "appears to prevail, and much has been written about the plant, which ought to be considered the true *Guaco*. But the fact is, that nearly every country has its peculiar *Guaco*. At first, the name was, probably, confined to only one species; when, however, in newly-discovered regions, the original plant was not found, the appellation was transferred to another that happened either to resemble it in appearance or to possess similar properties."

Humboldt and Bonpland, in their *Plantes Equinoxiales*,† described the *Guaco* of New Granada, under the title of *Mikania Guaco*, Nat. Ord. *Compositæ*, tribe *Eupatoriaceæ*. To this plant, which has occasionally been imported into Europe in a dried state, Guibourt, Martiny, and other pharmacologists, restrict the name *Guaco*. Other species of *Mikania*, also called *Guaco* or *Huaco*, and but very imperfectly known, have been enumerated by Dr. Pablo de la Llave as employed medicinally in Mexico. ‡

The genus *Aristolochia* likewise contributes many species of *Guaco*. We have before us the woody stems of several kinds from Honduras, which we feel no hesitation in referring to this genus. Even the common Virginian Snakeroot had a reputation, as its name implies, as a remedy for the bite of a serpent; and many other *Aristolochiæ* are mentioned by Dr. Lindley, as considered to possess similar virtues. The same author states, on the authority of Dr. Uslar, that a species of *Convolvulus* is used in Mexico under the common designation of *Guaco*. §

Such being the uncertainty attaching to the vernacular name, it would surely be better, in the science of medicine, to dismiss it entirely; and that if prescribed, it should be under a definite botanical name.

We do not know which of the species of *Guaco* afforded the good effects in deafness above alluded to.—*Pharmaceutical Journal*, March, 1854.

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* "Hooker's Journal of Botany," March, 1853, p. 76.

† Vol. ii. p. 84, pl. 105

‡ "El Mosaico Mexicano," tom. ii. p. 299, quoted by Seemann, *op. cit.*

§ "Vegetable Kingdom" (1846), p. 631.

On a METHOD *of* DETECTING *the* ADULTERATION *of* VARIOUS OILS *with* OIL OF SESAME. By M. BEHRENS.

MM. GUIBOURT and Reveil have tested the facts contained in a note addressed by M. Behrens to the Secretaire Général of the Société de Pharmacie, and give the following report of it.

This work treats of a process for the detection of the adulteration of various oils with oil of sesame, the reagent proposed is a mixture of equal parts, by weight, of commercial sulphuric and nitric acids; the test is performed by putting 10 grammes of the oil to be tested in contact with 10 grammes of the acid mixture. The coloration produced must be *instantly* observed, for at the end of one or two minutes the mixture darkens in color and becomes quite black.

The following are the colorations manifested by the contact of oils with the reagent :—

Sesame	oil	coloration	dark grass green
Olive	„	„	light yellow
Linseed	„	„	brownish red
Almond	„	„	peach blossom
Castor	„	„	little changed
Colza	„	„	reddish brown
Poppy	„	„	brick red.

All these colorations have been tested by us and verified. M. Behrens mentions that olive oil, mixed with a fourth part of sesame oil, takes a beautiful green color; but we have proved that the sensibility of the reagent is even greater than expressed by the author. In fact, we can prove the presence of one tenth of sesame oil in pure olive oil of ascertained origin.

Olive oil from Aix, also called *virgin oil*, which is naturally green, becomes of a pale tint when mixed with this reagent; if it contains a small quantity of sesame oil, the natural green tint is considerably augmented, and becomes green like tranquil balsam.

The oils of almonds, linseed, and poppy, which we procured from the *Pharmacie Centrale* of the civil hospitals, prepared in the establishment, and of whose purity there could be no doubt, likewise took a beautiful green coloration when, having added a little sesame oil to them, we treated them with the reagent.

Sulphuric acid, which was proposed in 1841 by Heidenreich, hyponitric acid or nitrous nitric acid proposed by M. Boudet and M. Fauré, have nothing in common with the process of which we speak, which only applies to adulteration with sesame oil; the others, on the contrary, serve to prove the mixture of various oils, either by the colorations which they produce or the time which they take to solidify.

The adulteration of the oils of olives, almonds, and castor with sesame oil may be profitably done, especially at Marseilles, where sesame oils arrives from Egypt free of duty.

To conclude, we are of opinion that the process indicated by M. Behrens may be of great service, as he renders the discovery of adulterations with sesame oil both rapid and certain.

Journal de Pharmacie et de Chimie, Nov. 1853.

New Method *of Ascertaining the* Purity *of* Compounds *of* Quinine.

By M. Pagliari.

This process consists in placing in a metal spoon a very small quantity of the preparation of quinine, and holding it over burning charcoal. In a very short time the matter dissolves, leaving a residue of which we shall speak further on. The greatest care must be taken in the examination of the residue, the peculiar color of which serves for distinguishing each of the compounds.

Characters presented by the following preparations:—

Very pure sulphate of quinine, *residue of a pale ruby red color.*

Very pure quinine, *residue the color of oil of sweet almonds.*

Citrate of quinine, *residue pale lemon color;* with an excess of acid, *residue deep yellow.*

Valerianate of quinine, *residue the same color as the medicament itself.*

Should any of these preparations be adulterated with any foreign matter, the residue, after fusion, will have neither polish nor transparency, and will consist of a black, porous matter. If cinchonine or salicine be present, which, when melted alone, lose their transparency and polish, the same thing will occur when they are mixed with quinine. To give more certainty with respect to the presence of salicine, concentrated sulphuric acid should be added, which communicates a red color to it.

These tests should be tried on small quantities of matter, about half a grain, which is exposed to the fire until it fuses, with the exception of sulphate of quinine, because this salt presents at the first a greenish color, which, by prolonging the action of the fire, passes to ruby red.

The author hopes that these experiments will put a fresh obstacle in the way of the adulteration of such useful medicines as the salts of quinine, and he trusts that chemists will make known the results of their trials, so as to augment and spread their observations, however simple they may be.

Journal de Pharmacie d'Anvers.

Pure Oil *of* Bitter Almonds *not* Poisonous.

Dr. Douglas Maclagan, in the "Monthly Journal of Medical Science," thus gives his opinion concerning the pure oil of bitter almonds:

1. The poisonous action of unrectified oil of bitter almonds is essentially due to the hydrocyanic acid which it contains. 2. The oil, really free from hydrocyanic acid, in doses of a few drops, does not act as a poison on animals generally. 3. Experiments on rabbits with half a drachm and under, invariably shew that, if quite free from hydrocyanic acid, such doses do not cause fatal effects. In larger doses (a drachm and upwards), it proves fatal to rabbits, but with variation as to the rapidity of death. 4. In dogs, doses even as large as three drachms produce merely a little vomiting. 5. If oil of bitter almonds

is to be regarded as a poison at all, it is not one of great activity; and it cannot even be styled a poison, without including under this denomination many other substances, such as oil of cloves. 6. The use of the purified oil to make flavoring condiments is open to no objection which would not apply to ordinary aromatic volatile oils; and the spirituous solutions sold for this purpose are, if made of properly purified oil, not dangerous. 7. Since, by due care, the oil can be entirely freed from hydrocyanic acid, great culpability will attach to the sale of preparations made with unrectified oil.

Dr. Maclagan states, that pure oil of bitter almonds is now prepared on a large scale at some of the wholesale establishments in London.

PHARMACY.

On the CINCHONAS *and on the Questions which, in the Present State of Science and Commerce, are more Immediately Connected with them.*

By MM. A. DELONDRE and BOUCHARDAT.

(Concluded from page 383.*)*

Flat Huanuco Cinchona (Peru.)—This cinchona is collected in the forests of Huanuco, north of Lima, and reaches us in serons, weighing from 70 to 75 kilogrammes. No species more nearly resembles the Bolivian cinchona, and those who collect it sell it for the real calisaya. This is doubtless the cinchona which Ruiz and Pavon have designated under the name of *Nitida*, and to which they attribute such great superiority.

The external surface is fawn colored, with longitudinal cracks, less uniform and marked than on the calisaya barks; the texture of the internal surface is even, but not so close; the transverse fracture is of a redder yellow, the fibres are short, but are not easily detached. The bitter flavor developes itself very quickly on chewing it, but it is slightly sharp and styptic. The thickness of the barks is from 6 to 10 millimetres.

This cinchona, notwithstanding its similarity to calisaya in appearance, only produces about 6 grms. of sulphate of quinine, and 12 grms. of cinchonine per kilogramme.

Rolled Huanuco Cinchona with epidermis (Peru)—This bark is produced by the branches of the tree, whose trunk gives the flat cinchona which we have just described; a great deal of this used to be imported, and it was known by the name of Grey Lima Cinchona. It has long been much preferred. To the sample intended for the Faculty of Medicine we have affixed the ticket which came with it from the forests of Huanuco, *Cascarilla encanutada con orbes, de las ramas, nombrada pata de Gallinazo, que antiguamente fue muy apetecida.* This bark differs very little in appearance from the rolled calisaya; the epidermis is thinner, rough, cracked in every direction, of a dirty white color. The interior is even, with fine fibres, of a reddish yellow

color. The fracture is fibrous internally, and resinous under the epidermis.

It produces 2 grms. of sulphate of quinine and 8 or 10 grms. of cinchonine per kilogramme. The thickness of the barks is from 3 to 6 millimetres.

Pale yellow Huanuco Cinchona (Peru.)—We received some serons of this cinchona at Valparaiso, and we only preserved the one sample intended for the Faculty of Medicine with the ticket, which had been sent to us from the forest at the time, as with the preceding samples.

When we treated it on a large scale it gave us 6 grm. of sulphate of quinine, and 10 grms. of sulphate of cinchonine.

The thickness of the barks is from 4 to 10 millimetres, the external surface is of a dirty yellow color, with some projecting thorns, and very slightly longitudinal furrows; the interior is of a paler yellow, with a smooth texture; the fracture is fibrous.

Ligneous Carthagena Cinchona (New Granada).—We retain the name by which this species has long been known in commerce; it is the one originally used in the manufacture, and which we naturally prefer. It is twenty years since it used to come in small quantities to our ports from New Granada. It furnishes 20 grms. of sulphate of quinine, without any trace of cinchonine. The barks are distinguished by the length of the fibre. The external surface retains its epidermis, which is very thin and adherent, of a yellow fawn color, with a few white exfoliations. The internal color is of a yellowish fawn, resembling that of the Bolivian Calisaya; although very even we can see the long fibres of its texture. The bitter flavor is easily developed, is not at all styptic, and is very persistant.

Rose-colored Cinchona (New Granada).—We have only had a few serons of this cinchona, and have never seen any resembling it; it comes from the Province of Ocana, New Granada, near the shores of the River Madalena. We retain the name of *rose-colored*, by which our young friend, Ossian Henry, jun., designated it; according to the analysis which he made, it contains 18 grms. of sulphate of quinine, and 4 grms. of cinchonine per kilogramme. This bark is likewise from New Granada, and completes the seven good species, forming the number discovered by Zea and Laubert. There are others mentioned by Zea, but the varieties which we have met with belong especially to the orange yellow species.

The thickness of the rose colored cinchona is from 2 to 6 millimetres, the bitter flavor is easily developed and is not astringent, the external surface is of a deep rose-color, with deep longitudinal furrows, with some transverse roughnesses, marking the seat of the epidermis which has been removed. The internal surface is a little paler and yellower, the fracture is clear, the fibres fine, the texture smooth, without being very close, and it is slightly resinous externally.

Fine Grey Cinchona from Loxa (Equator.)—This cinchona comes in serons of 40 to 50 kilogrammes from Payta, or Guayaquil; it is found in the forests of the Province of Loxa; the barks, which are rolled, are about the thickness of a little finger, sometimes thicker. The

epidermis is of a dull grey, which causes it to be called *negrilla* by the Indians, covered with lichens, covered all over with close fissures. The fracture is slightly resinous externally and fibrous internally. The texture is smooth and rather close, the bitter flavor is astringent and aromatic. We found in it 2 grms. of sulphate of quinine and 10 of sulphate of cinchonine per kilogramme.

Small Grey Cinchona Condaminea (Equator.)—This cinchona, like the preceding, comes from the interior of the Province of Loxa, by the port of Guayaquil, in serons, and sometimes in cases of 40 or 50 kilogrammes. The barks are rolled, very thin, sometimes the size of a goosequill, covered with a silver grey epidermis, covered with furrows, The fracture is clean and resinous externally, shewing internally a very few fibres, which are very fine, and of an extremely close texture; the bitter flavor is fresh and not styptic. It produces about 8 grms. of sulphate of quinine, and 6 grms. of sulphate of cinchonine, per kilogramme.

This cinchona has always been known in commerce by the name of *very small grey cinchona from Lima*, because it came from Lima to Spain, the government of which country monopolised its sale; for this reason it received the name of *quina regia*. Laubert calls it *Condaminea*, as do MM. Humboldt and Bonpland.

We have before mentioned how much we regretted that in other parts of America the bark of the trunk only should be taken, and that the branches should be left; here it is exactly the contrary; and we cannot avoid wondering why, with regard to this and another species which we are about to describe, the small branches only should be taken, leaving the bark of the trunk, which must be so very rich, according to the amount of alkaloïds which we found in the branches.

Grey Cinchona of Jaen (Peru).—This cinchona is found in the forests of Jaën, at a short distance from Loxa. The rolled barks produced from the branches are from 3 to 9 millimetres in diameter. This cinchona is remarkable for the general whitish color of the epidermis; this appearance has given it the name of *ash-colored cinchona*. The epidermis is thin, smooth, shiny, and adherent to the dermis. The internal color of these barks is a pale orange yellow in the smaller ones, becoming of a reddish orange in the larger barks. The large barks are sometimes broken, the fragments forming almost flat pieces. The fracture is fibrous, the fibres long and flexible, of a rather open texture; the bitter flavor is very powerful to the tongue, but without astringency. This cinchona comes over in serons of 40 to 50 kilogrammes, free from any mixture with other barks.

In 1838 M. Bouchardat analysed the cinchona Jaën, and he extracted an alkali from it, which had all the properties assigned to aricine by Pelletier and Corriol. After the publication of this analysis, M. Manzini thought that he had found in this bark a new alkaloïd, which he called cinchovatine; but a German chemist, who had never heard of M. Bouchardat's work, proves that this cinchovatine is identical with aricine.

M. A. Delondre, on subjecting the cinchona Jaën to the large process

for manufacture, extracted from it 4 grms. of sulphate of cinchonine and 10 grms. of sulphate of quinine, differing in no respect from that of the cinchona calisaya, perfectly soluble in the proportion of alcohol and ammonia indicated by MM. Bussy and Guibourt, and now considered a sufficient proof of the purity of sulphate of quinine.

These contradictory results require an attentive examination, which we hope shortly to be able to give to them.

Is aricine modified in the course of manufacture on the large scale? Is there any other unperceived circumstance which has led us to these different results? Whatever may be the cause it is clear that the result is of importance.

The six species that we are now about to describe should be known, that they may be avoided when offered for sale.

Yellow Cinchona of Cuzco (Peru).—This cinchona comes from the Province of Cuzco, in serons weighing from 70 to 75 kilogrammes, by the port of Islay. M. Delondre and Dr. Weddell met with it in the forests of Santa Anna, and it was so like that from which MM. Pelletier and Corriol had extracted aricine, that we thought it must have the same properties; but a more careful examination, and the results of analysis, shewed us our error. This cinchona gave very pure sulphate of quinine, but only amounting to 60 centigrammes per kilogramme, so that we were obliged to include it in the list of useless barks.

Our five-and-twenty years' experience leads us to think that the cinchona containing aricine comes from the forests of Bolivia, and should be the *pubescens* of Dr. Weddell as well as that of Cuzco, which would prove that at great distances the cinchonas of the same botanical species may give different products, according to the nature of the soil and other circumstances. This latter is of a rust color externally, not quite so dark internally, with a clear fracture, and a close texture, with short fibres. The bitter flavor is slowly developed, and has a disagreeable, mouldy, styptic taste. The cinchona, with the base of aricine, has internally very fine fibres, and is of a yellow fawn color, similar to that of the calisaya. The external surface is of a dull yellow color, with a thin smooth epidermis, strongly adherent to the dermis; the fracture is in fine fibres internally and suberous externally.

Amongst serons of the real calisaya there are often found some of this species; but having made some complaints on the subject, we ceased to receive them. Doubtless, this cinchona may be found in the forests of Bolivia, but the company holding the monopoly are careful only to export the best qualities of calisaya.

Brown Cinchona from Cuzco (Peru).—This cinchona is also found in the forests of the Province of Cuzco, and its appearance has led to the exportation of large quantities of this bad cinchona into Europe.

The external surface is smooth, of a dull brown color; the internal surface is not quite so dark; the fracture is clear, and shews no fibres; the texture is as close as hard wood; the thickness is from 4 to 10 millimetres; the bitterness, although slight, is styptic, and disagreeable. With difficulty we extracted 5 centigrammes per kilogramme.

Grey-rolled Cinchona from Quito (Equator).—Some serons of this

cinchona came with some red and yellow cinchonas from Quito, by Guayaquil. It is a rolled bark, with a very rugged thick epidermis adhering to the dermis, furrowed in every direction, with many protuberances. The fracture is clear and almost granulous; when cut it appears like a hard wood, capable of being polished; the internal surface is smooth, shiny, and of a yellowish brown; the bitter flavor is slowly developed, rather sharp, and very styptic; the thickness is from 5 to 8 millimetres. We procured 60 centigrammes of sulphate of quinine from it.

Cinchona from the Coast of Africa.—A long time ago we heard of a species of cinchona whose virtues surpassed those of South America, with which fevers were cured on the Coast of Africa, and which came from near Sierra Leone. We obtained a case of 60 kilogrammes from MM. Menard and Kestner. Our analysis was far from agreeing with this fame, for we only obtained from it 60 centigrammes of sulphate of cinchonine per kilogramme.

These barks are in large pieces; the epidermis somewhat resembles the bark of the chesnut-tree; when the asperities have been scraped off it is very thin, with longitudinal furrows, easily detached in long fibres; its color is a dirty yellow, the internal surface is almost white, and the fracture shews long, thick, lemon colored fibres. The bitter flavor is quickly developed, is not styptic, and leaves a very persistent disagreeable taste.

Pale Red Cinchona (New Granada).—With the serous of rose colored cinchona from New Granada, came another containing three samples, likewise from the province of Ocana, first described and analysed by M. Ossian Henry. Jun. The sample of which we now speak is in barks of from 3 to 4 millimetres in thickness, with a dull grey epidermis, similar to the grey cinchona of Huanuco, with longitudinal and transverse fissures; the internal surface is a pale red color; the texture is fine, and the fracture is in short fibres, easily detached by the nail, like the calisaya, with a resinous coating under the epidermis. The bitter is styptic and disagreeable, and slowly developed. This bark gave 18 centigrammes of sulphate of quinine, and 2 centigrammes of sulphate of cinchonine.

White Cinchona (New Granada).—This second sample, likewise from the province of Ocana, is in flat, very wide barks, without epidermis, of a fine close texture, cutting like a wood which would take a polish: it crackles between the teeth; the bitter flavor is slowly developed and is disagreeable, but not astringent. The internal and external surfaces are both of a dull white, the fracture almost white as with ash wood. It produced 6 centigrammes of sulphate of quinine, and 12 centigrammes of sulphate of cinchonine per kilogramme.

False Cinchonas.—After having described the good, middling, and bad cinchonas, MM. Bouchardat and Delondre describe the false cinchonas found in commerce. They are as follows:—*Reddish brown barks*, and *the bark the color of wine lees*, from New Granada; *red bark with epidermis*, from the Argentine Republic; *red and white barks*, from Brazil. They conclude as follows:—We trust that we

have shewn that the alkaloïd which forms a great part of the base of the cinchonas, preferred for a century and a half, was *cinchonine*, which we now carefully remove from the quinine, thus submitting to a prejudice which requires that we should, without giving us any sufficient reason.

All the experiments made at our request, and at the expense of M. Delondre, during the last year, by M. Hudellet, Physician at Bourg-en-Bresse, both at the hospital and in private practice at Dombes, where paludian fevers are endemic, as well as by Dr. Beauregard in the Canton of Eure, near Havre, have proved, that in *no single case* was sulphate of quinine more efficacious than sulphate of cinchonine. These gentlemen have each published their comparative observations, which are very interesting, and we need not insist further on the results which they have communicated to us. The same observation was made by Dr. Wahn, head of the Civil and Military Hospital of Cherchel (Algeria), and whose memoir on this subject is not one of the least curious to read. M. Bouchardat has many facts by him which he intends publishing to decide this important question.

It is very clear that there are no substitutes nor compounds whose *uncertain* trials are rewarded, authorised, and encouraged, and which are frequently very expensive, which can be compared to the *certain* efficacy of cinchonine, even if we allow that it comes below quinine itself in value.

We must likewise mention the alcoholic extract of cinchona, namely, the *whole of the alkaloïds* contained in the entire cinchona, its tannin and aromatic principles, leaving only the ligneous substance and insoluble matters. The physicians of whom we have spoken have obtained very permanent tonic and febrifuge results with a wine prepared with this extract, when the patients have been too weak to bear the energetic action of large doses of sulphate of quinine.

The wine may be prepared with 4 grms. of extract dissolved in 60 grms. of hot alcohol at 36°, mixed with a litre of wine, and then filtered.

The alcoholic extract should contain from 30 to 35 per cent. of alkaloïds. The worst cinchonas give almost as much resin as the best species, but these resinous extracts contain but small quantities of quinine or cinchonine.

We cannot express ourselves too strongly against the thoughtless or interested declamations which have thrown into temporary disfavor the cinchonas of New Granada and other parts of South America, for we have produced from most of them quite as pure quinine as from those from Bolivia.

Moreover, it is certain that the quinidine which has been made the pretext of these declamations is only found in considerable proportion in a few rare species, and that it would be difficult to determine with precision in what it differs from quinine, either in composition or effects.

Répertoire de Pharmacie, January, 1854.

PROCEEDINGS OF SOCIETIES.

Chemical Discussion Society, Jan. 26, 1854.

Mr. Prout Girdwood in the Chair.

Several specimens of iodised collodions were exhibited, and their respective merits discussed, and a number of beautiful specimens of microscopic photographs exhibited and commented upon.

Mr. Greenish introduced the subject of the chemistry of prescriptions for discussion, and brought before the Meeting a phial containing equal proportions of tincture of iodine and tincture of opium, which had been prescribed as a liniment. The tincture, when mixed, formed a thick gelatinous compound, which it was perfectly impossible to apply as intended. Decomposition had ensued, and a precipitate was formed consisting of the iodurette hydriodate of morphia and the gummo resinous portion of the opium, which entirely altered the character of the agents, and rendered the compound useless as a liniment.

Mr. Spencer exhibited a sample of extract of belladonna, which contained a salt of copper; a steel knife, placed in a solution of the extract, soon became coated with copper; he did not believe any salt of copper had been added to brighten its color, but simply that it had been carelessly prepared in a copper pan; he inquired if any member present had met with any similar fact, and suggested that in the preparation of vegetable extracts pharmaceutists could not be too careful in using pans properly coated either with tin or enamel.

Mr. Blyth stated that he had met with a similar contamination in extract of belladonna. He discovered it quite accidentally by leaving a spatula in a solution of the extract, which, on taking out, he found coated with copper. As the extract was of a very good color, he thought it might have been added purposely to preserve the green tint, and examined all the extracts from the same manufacturer, which were all of a good color, but he did not find it in any other extract. He thought, perhaps, from the coincidence, that there might be some acid in the belladonna which acted more readily on a copper pan than in other vegetable juices. He now always examined all samples of extracts for copper before making the selection. He quite agreed with Mr. Spencer as to the necessity of using pans which could not be acted on by vegetable juices in the preparation of extracts.

Mr. Greenish read a prescription for making an ointment composed of Ung. Hyd. Nitratis and Creosote; it was very troublesome to mix in the first instance; but when, apparently, a satisfactory result had been obtained, slow decomposition ensued, resulting in a kind of fermentation; the lid of the pot being pushed off, and, after a lapse of twenty-four hours, no odor of Creosote remaining.

Mr. Williams explained the cause of the decomposition, and stated, that if Creosote was dissolved in Glacial Acetic acid, and the solution added to strong Nitric acid, and the mixture left for some days, a great number of crystals of oxalic acid were produced, while carbonic acid gas was evolved. This result could not be obtained by adding the Creosote direct to the nitric acid, as the action was then so violent as to cause more complicated results. He imagined that the lard, &c., in the ointment acted in a similar manner to the Glacial acetic acid in his experiment, viz. by diluting the active agent, and moderating the subsequent reactions—thus, he conceived that the ultimate result of adding Creosote to the Ung. Hyd. Nit. was the production of oxalate of Mercury; and the carbonic acid formed, at the same time, caused the ointment to swell, and so become unfit for use. If no free acid had existed in the ointment no such result would occur.

(To be continued.)

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

CENTIGRADE TESTING *and the* PREPARATION *of* TEST SOLUTIONS.

BY WILLIAM ACKLAND.*

THE application of centigrade testing serves to ascertain the amount of caustic alkali, or of the carbonate of the alkali, contained in the crude potassa or soda of commerce, and of the strengths of ammonia and the acids, and is generally employed by the manufacturer and chemist from the simplicity of the manipulation, the rapidity of the execution, and the accuracy of the results obtained. Many modifications of the process exist; but as the method I propose to describe requires no calculations in all the ordinary operations required by the manufacturer or chemist, its introduction at the present time may not be out of place; and if I here introduce names or measures as yet not fully understood, I do so with a full consideration of their value, as no plan of centigrade testing can be complete unless based on some decimal system. The one I propose to employ has the bulk of one gallon of water for its UNIT; and the division of the gallon is as follows:

1 Decimillem	=	1-10,000th	of a gallon	or	7 grains.
1 Millem	=	1-1000th	„	„	70 „
1 Centem	=	1-100th	„	„	700 „
1 Decem	=	1-10th	„	„	7000 „
1 Gallon	=		„	„	70,000 „

This division of the gallon is of essential advantage to the manufacturer, as it enables him to ascertain the amount of a chemical substance in a solution, in any number of gallons, from the knowledge of the contents of a centem, by merely adding two 0's, and multiplying by the number of gallons.

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* Read before the Chemical Discussion Society, March 9th, 1854.

Apparatus required for Centigrade Testing.—The apparatus necessary for centigrade testing consists of—

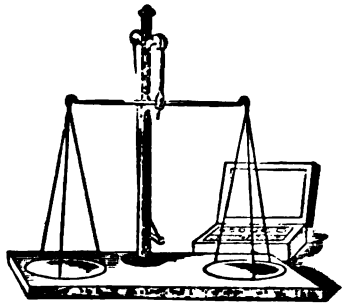

Fig. 1.

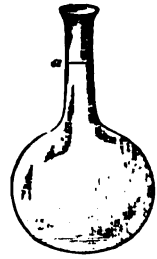

Fig. 2.

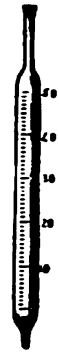

Fig. 3.

1. A balance capable of turning with the 1-50th of a grain when loaded with 500 grains, fig. 1.

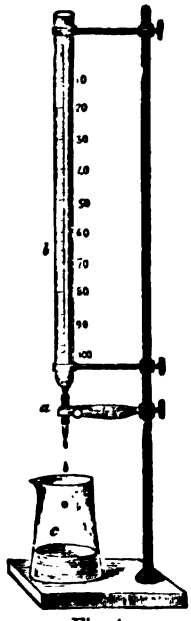

Fig. 4.

2. A set of decimal weights, from 500 grains to the 1-10th of a grain.

3. A decem bottle containing exactly the tenth of a gallon of water when filled up to a mark on the neck, fig. 2.

4. A pipette, fig. 3, divided, to shew decimillems.

5. Pipette marked "SULPHURIC ACID," with a mark on the stem, shewing the amount of sulphuric acid to be diluted in the decem bottle, in order that, when tested, the results shall be given in percentages. Similar pipettes, marked MURIATIC ACID, NITRIC ACID, POTASSA, CARB. POTASSA, SODA, CARB. SODA, ACETIC ACID, fig. 10.

6. An Alkalimeter divided into 100 divisions, each division equal to 1 decimillem. The alkalimeter lately recommended by Mohr, fig. 4, is the most convenient for general purposes. As permanganate of potassa attacks the vulcanised rubber valve made use of in Mohr's alkalimeter, when a solution of that substance is employed, Bink's alkalimeter, fig. 5, must be used instead; but in either case the divisions must be of equal value; and as the accuracy of the results depends so much on the correct sub-divisions of the different pieces of divided apparatus, none but engine-divided instruments should be employed.*

* Engine-divided instruments, prepared under the superintendence of the Author, and every article required for Centigrade Testing, may be obtained of Messrs. HORNE, THORNTHWAITE, & WOOD, 123 and 121, Newgate Street, London.

7. A test-mixer, fig. 6, divided into 100 divisions, each division representing the 1-100th part of the whole bulk up to 100. The

Fig. 5.

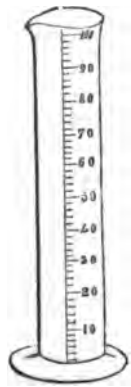

Fig. 6.

Fig. 7.

Fig. 8.

test-mixer may be of any size, from 16 ozs. to half a gallon, according to the quantity of test-acid required, and the magnitude of the operations about to be performed; but the most convenient size for general use is one holding about 30 ounces.

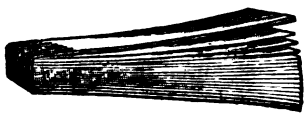

Fig. 9.

8. Phillips' jar, fig. 7, holding about 8 ounces.

9. Glass stirrer, fig. 8, about 6 inches long.

10. Books of neutral test-paper, fig. 9. This test-paper is the most useful for the detection of both acids and alkalies, and is far more delicate than either litmus or turmeric. The color is of a pale reddish violet tint; acids change this color to a bright red, and alkalies to a blue; whilst, if the solution is neutral, the color is, of course, unacted on.

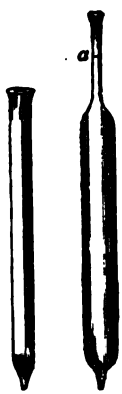

Fig. 11. Fig. 10.

11. A centem pipette, fig. 10, so graduated that when filled up to a mark *a* on the stem, and then drained, and the last drop blown out, it will DELIVER exactly the 1-100th of a gallon.

12. Pipette, fig 11, with fine point, so as to deliver drops, useful for adding small quantities of water in adjusting the height of the fluid in the test-mixer and decem bottle to the exact level of the division.

13. A weight marked NaO, fig. 12.

14. A weight marked KO.

15. A weight marked CO^2.

These weights are useful to facilitate the weighing out the quantities of the sample of crude soda or potassa, or other substances, about to be submitted to analysis, and obviate the necessity of the use of a number of smaller weights.

Fig. 12.

16. The different standard solutions hereafter described.

17. A solution of blue litmus. As this solution will not keep good for more than a few days in summer, it is necessary to prepare only small quantities at a time. This solution is formed by boiling one dram of litmus in one ounce of water for ten minutes, and when cold pouring off the clear fluid for use.

Standard Solutions.—In the preparation of standard solutions some definite plan of proceeding is very essential; and that based on the atomic theory at once recommends itself to our notice, as it is very evident that 17 grains of ammonia would be exactly neutralised by 36½ grains of muriatic acid, 40 grains of sulphuric acid, or 54 grains of nitric acid; and if we prepare equal bulks of solution of each substance in those proportions, equal measures of such solutions will possess an equal power of neutralisation; and from these facts, by operating on known quantities of a substance in solution, we can deduce its exact per-centage.

In the choice of strengths for standard solutions various conditions are to be considered, such as the permanency of some solutions, the solubility of certain substances, and even the expense of others: viewing all matters on this point, I think the best strength for our solutions would be formed by taking the atomic number of the substances on the hydrogen scale and multiplying this number by 5, or the atomic number on the oxygen scale and multiplying by ·4; then taking a weight in English grains of the substance equal to the product, and dissolving that quantity in water until the solution occupies the bulk of a tenth part of a gallon or a decem. To illustrate this, let us suppose a standard solution of nitrate of silver is required; the atomic number of nitrate of silver is 170; this number multiplied by 5 gives 850 for a product, and if we take 850 grains, dissolve it in water, and dilute the solution until it occupies the bulk of one tenth of a gallon, we should obtain a solution of the standard strength.

To prepare a Standard Solution of Oxalic Acid.—Weigh out 350 grains of pure crystallised oxalic acid, and dissolve it in 15 ounces of distilled water contained in the decem bottle, fig. 2, aiding the solution by applying heat; when the crystals are all dissolved, set aside to cool down to 62°, and on this temperature being reached, add water to complete the measure, or until the solution rises to a level with a line marked on the neck of the bottle, agitate so as to mix well, then preserve in a well stoppered bottle.

It is very difficult to weigh out the exact quantity of oxalic acid required to make a solution of the standard strength, from the variable quantity of water of crystallisation contained in each sample, and from the difficulty of depriving the crystals of this water, as the temperature

necessary to effect this is so high that, unless very great care is observed, the hydrate is decomposed and an acid of variable strength remains. To guard against this source of error, we have taken a larger quantity of acid than is likely to be required to form our future solutions; and we must now proceed to ascertain how much this solution is above the standard strength. To do this, we must make a solution of a substance that can be obtained of considerable purity, and which involves no great care in its after preparation. Such a substance is anhydrous carbonate of soda: as I find ordinary bicarbonate of soda obtained from different sources, on being converted into carbonate by being heated to redness, and solutions made from the same quantity of each sample thus prepared, yielded results which did not differ to any appreciable extent; indeed, both carbonate and bicarbonate of soda can be obtained possessing so little impurities that the errors of experiment would be greater than the errors thus introduced.

To prepare a Standard Solution of Carbonate of Soda.—Take a porcelain crucible capable of holding two ounces of water, and fill it nearly full with either crystallised carbonate or bicarbonate of soda, and then expose it to a dull red heat over an argand spirit lamp, or wire gauge gas flame, supporting the crucible by a crucible jacket. Continue the application of heat for ten minutes after the water of crystallisation has been driven off, then cover the crucible and allow the contents to cool down to about 80° F. Now weigh out 265·9 grains in a capsule previously balanced in the balance, fig. 1. Transfer this weighed quantity to the decem bottle, fig. 2, previously filled about two thirds full of distilled water; agitate so as to promote the solution, and apply heat if necessary; when the solution is complete, add water until the bottle is filled to within a quarter of an inch of the mark *a* on the neck, then allow the mixture to cool down to 62° F. Finally, add water drop by drop from the pipette, fig. 11, until the bottom surface of the curve of the fluid rises to an exact level with the line of the neck of the bottle, shake so as to mix thoroughly, then preserve in a stoppered bottle for use.

Having by the above process obtained a standard solution of carbonate of soda, we now proceed to ascertain how much the oxalic acid solution is in excess of our standard. To effect this; take the centem pipette, fig. 10, and fill up to a mark on the stem with the solution of carbonate of soda; drain this measured quantity into the Phillips' jar, fig. 7, blowing out into the jar the last drop that forms at the point of the pipette, and add ten drops of litmus solution, to produce a distinctly blue color.

Now fill up the alkalimeter, fig. 4, to the top division marked *o* with the acid solution, then hold the jar containing the alkaline fluid under the point of the alkalimeter, and press the two studs *a* with the finger and thumb; this pressure will open the tube and allow the liquid to flow into the jar; occasionally stop the flow of liquid by removing the pressure of the finger and thumb, and give the jar a rotatory circular motion to facilitate the evolution of the carbonic acid; continue this movement of the jar and addition of the acid,

until the solution begins to lose its bright blue color and to assume a red tint at that point where the acid falls; when this is observed, proceed cautiously, adding the acid only by single drops at a time until the solution assumes a wine red tint, which indicates that the point of neutrality has been reached; but this point is rather difficult to be seen by the uninitiated, owing to the action on the litmus of the evolved carbonic acid being somewhat similar to that of oxalic.

To guard against any error on this point we must not depend solely on color, but test the mixture after each addition of acid with neutral test-paper. For this purpose, dip the point of the stirrer, fig. 8, into the mixture and draw a line with the wetted point across the test-paper. If the paper turn red, but gradually change to blue on being slightly heated, the action is due to carbonic acid, and more oxalic acid must be added until the solution is so neutralised that it has no power of changing the color of the neutral test-paper. Having attained this point, read off the number of decimillems of the acid solution used, multiply 350 by this number, point off two figures on the right hand side of the product for decimals, and the remainder is the number of grains to be taken of the sample of acid from whence the first solution was made, to form a decem of solution of standard strength; rejecting as useless the first solution made with the 350 grains.

In order to give an example, suppose that a solution of oxalic acid has been made containing 350 grains to the decem, and a solution of carbonate of soda as before described, and that 100 decimillems of the soda solution was completely neutralised by 78 decimillems of the acid solution. Now multiply 350 by 78 and we obtain 27,300 for a product; we point off two figures on the right hand, the sum left is 273; and if we take 273 grains of the sample from whence we obtained our 350 grains, dissolve it in water, and dilute with water in a decem bottle until the solution rises to a level with a mark on the neck, we shall have a solution of the standard strength; and it is a fact of some importance, that, if we preserve in closely stoppered bottles the bulk of the oxalic acid from whence our sample was taken, a solution of the required strength can at any future time be made by weighing out the quantity indicated by the result of the first experiment and dissolving it in a decem of water.

Preparation of a Standard Solution of Ammonia.—Take the ordinary commercial liquid ammonia, and dilute it with about 16 times its bulk of water; the bulk of the mixture to be made depending on the quality of test-ammonia required.

The next operation is to ascertain the exact strength of the mixture, so as to determine how much more water is needed to reduce the solution to its proper strength. To effect this, we make use of the standard solution of oxalic acid, by taking a centem pipette, fig. 10, and filling it up exactly to a mark on the stem with the acid fluid. Carefully drain this measured quantity into a Phillips' jar, fig. 7, and then blow out the last drop which forms at the point of the pipette, add 10 drops of litmus solution to color it, and place the jar with its contents on a table on which has been laid a sheet of white paper.

Now fill the alkalimeter, fig. 4, with the ammonia mixture above described, until the bottom surface of the curve of the fluid rises exactly level with the line marked *o*, then open the valve by pressing on the spring *a* with the finger and thumb, and allow the ammonia mixture to flow into the jar containing the acid fluid, occasionally stopping the flow from the alkalimeter by removing the pressure from the spring, and giving the jar a shaking rotatory motion with the hand; continue this alternate shaking and adding until the red color begins to change to a purple tint. Proceed now cautiously, adding single drops only at a time, until the red color changes to a peculiar violet tint, which indicates that the exact point of neutrality has been reached.

As the color of the tint which the solution assumes when just neutralised is easily recognised when once seen, but very difficult to describe, and as the uninitiated may be at a loss in this matter, the use of the neutral test-paper will remove the difficulty, as a slip of this test-paper, on being dipped into the solution, will be changed to a red color if more ammonia has to be added, and to a bright blue if too much has been used; but if the exact point of neutrality has been reached, the color of the slip of test-paper will remain unchanged.

Having by the above plan found the exact point of neutralisation, bring the eye level with the surface of the fluid in the alkalimeter and read off the number of decimillems of ammonia employed. Now take the test-mixer, fig. 6, and pour the ammonia mixture into it until it occupies as many divisions as the number of decimillems of ammonia used in neutralising the acid in the last experiment; then fill up to 100 with water, taking the precaution that if any heat is produced on adding water to the mixture, to fill up only to about 99, and then to agitate well, and allow the whole to cool to 62° F., when the remaining adjustment must be made, so that the mixture shall exactly rise to the 100th division.

If this operation is carefully performed, the solution obtained will be exactly the strength required. To ascertain if such is the case, take 10 decimillems of the ammonia solution, and 10 decimillems of oxalic acid solution by using the pipette, fig. 3, and mix them together. If the ammonia is exactly the strength required, the mark produced on the neutral test-paper by the mixture just described will be of the same tint as the paper itself. If the tint produced is red, more ammonia must be added to the solution; and if blue, more water.

When the ammonia is of the exact strength, it is to be carefully preserved in stoppered bottles, and is employed in making standard solutions of acids, and in determining their per centage. The test-ammonia, prepared, as abovenamed, will keep of a uniform strength for months, if preserved in well-stoppered bottles.

To ascertain the per centage of pure Anhydrous Soda in Soda-ash.—Weigh out from an average sample of the soda-ash, previously pulverised, 155·9 grains, or a weight equal to the brass weight marked NaO, fig. 12; transfer it to the decem bottle, add about 12 ounces of water, and agitate until all the soluble matter is dissolved; then

fill up the bottle with water to the mark on the neck; again agitate thoroughly to preserve a perfect admixture of the solution.

Now take the centem pipette, fig. 10, and fill it up to the mark on the neck with the soda-ash solution; transfer this measured quantity to a Phillips' jar, fig. 7, blowing out the last drop which forms at the point of the pipette; add 10 drops of litmus solution; fill the alkalimeter, fig, 4, up to *o* with standard oxalic acid solution, and proceed to neutralise the soda solution with the oxalic acid by allowing the acid to flow from the alkalimeter in a gentle stream at first, then more and more carefully, until the blue color begins to change; now agitate frequently, and add the acid more cautiously, until the color changes to a wine red, and the point of neutrality is reached.

As the change of color produced by the carbonic acid evolved is similar to that of oxalic, we must employ other means than the mere change of color to determine when the exact point of neutrality is reached. This is effected by the use of neutral test-paper, fig. 9. The point of a stirrer, fig. 8, is dipped into the solution, and a wet line drawn by it across one of the leaves of the test-paper. If the color produced on the test-paper by this application is blue, more acid must be added from the alkalimeter, until the mark produced is slightly red. The first red marks produced are evanescent, more especially if the test-paper is warmed, indicating the presence of carbonic acid; hence it is necessary to guard against this source of error by warming the test-paper after each additional line has been made, and to continue the addition of the acid (now only one drop at a time) until the stain produced no longer disappears on being warmed, but remains of the same color as the paper. If too much acid has been used, the operation must be again commenced from the beginning. When the neutralisation is complete, read off the number of decimillems of the acid which has been employed to neutralise the soda, and this number is equal to the per centage of caustic soda present in the soda-ash under trial.

To ascertain the per centage of Anhydrous Carbonate of Soda.—To astertain the per centage of anhydrous carbonate of soda in soda-ash, proceed as before directed; but the quantity in this case must be 265·9 grains, or a weight equal to the two weights marked respectively, NaO and CO^2. When the point of neutralisation has been reached, the number of decimillems of the test-acid employed will denote the per centage of pure anhydrous carbonate of soda present.

To ascertain the per centage of Potassa, or Carbonate of Potassa.—The determination of the per centage of pure potassa, and of carbonate of potassa, is precisely similar; but the weight of the sample to be taken when the per centage of pure potassa is required, must be equal to the weight-mark KO, or 235¾ grns.; and if the per centage of pure anhydrous carbonate of potassa is required, a weight equal to two weights marked KO and CO^2, or 345¾ grns.; and the number of decimillems of the standard test-acid employed to completely neutralise the alkali, denotes the per centage in either case.

To determine the per centage of Liquid Sulphuric Acid, of the specific gravity of 1·848 in a given sample.—Fill the pipette, fig. 7,

marked "Sulphuric Acid," up to the mark on the stem, with the acid under examination. Drain this measured quantity into the Phillips' jar, fig. 7, rinse out the pipette with three or four successive quantities of water, then fill the jar about half full with water, and add 10 drops of litmus solution. Fill the alkalimeter, fig. 4, up to the *o* with the standard test-ammonia, and proceed to neutralise the acid with the test-ammonia from the alkalimeter, as before directed; when the color begins to change, add the test-ammonia in single drops at a time, and agitate with a circular rotatory motion; continue adding until the acid is completely neutralised, which is immediately apparent by the former red tint assuming a peculiar reddish violet tinge, which, if once seen, is immediately recognised again.

For the purpose of more readily observing the change of color, the Phillips' jar is laid on a sheet of white paper, and the operation performed near a window with a good amount of light. When the neutralisation is complete, the number of decimillems of the test-ammonia employed denotes the per centage of the acid.

If the per centage of anhydrous sulphuric acid is required, proceed as directed for liquid sulphuric acid, but multiply the number of decimillems of ammonia used by ·8167, and the product is equal to the exact amount of anhydrous sulphuric acid in the sample.

Muriatic Acid.—The per centage of muriatic acid of the specific gravity of 1·200 in any sample, is found in the same manner as directed for the determination of sulphuric acid; but the pipette marked "Muriatic Acid," must in this case be filled with the acid under trial up to the line marked on the stem; and the number of decimillems of ammonia employed to neutralise it will also in this case give the per centage of the acid.

If the per centage of pure anhydrous muriatic acid is required, proceed as before directed, but multiply the number of decimillems of ammonia used by ·4078, and the product is equal to the per centage of anhydrous muriatic acid in the sample.

Acetic Acid.—In precisely a similar manner, if vinegar or acetic acid is taken, and a pipette marked "Acetic Acid," filled and neutralised with ammonia, as directed for sulphuric acid, the number of decimillems of ammonia used will also, in this case, be equal to the per centage of acetic acid in the sample used. As the action of acetic acid on litmus is very slight, the change of color produced, when the point of exact neutrality is reached, is difficult to be appreciated; recourse must, therefore, be had to the neutral test-paper, as indicated.

Nitric Acid.—If the per centage of nitric acid of the specific gravity of 1·521 is required, the operations are precisely similar, but the pipette marked "Nitric Acid," must be used in this case, and filled with the sample of the acid up to the line on the stem, and the number of decimillems of ammonia employed to neutralise, will also give the per centage of acid under trial.

If the per centage of anhydrous nitric acid is required, proceed as above directed, but multiply the number of decimillems of ammonia

used by ·8575, and the product gives the per centage of pure anhydrous nitric acid in the sample.

Anhydrous Soda in solution.—If the per centage of anhydrous soda, *in a solution of that substance*, is required, fill the pipette marked "Soda" with the solution, color blue with litmus solution, and proceed to saturate it with standard oxalic acid solution from the alkalimeter previously filled up to *o* with that fluid, observing the same precautions as mentioned for carbonate of soda; and the number of decimillems of acid solution used will be equal to the per centage.

Anhydrous Potasse, and in solution.—In exactly the same manner, if the per centage of the dry substance be required in a solution of either carbonate of soda, carbonate of potassa, or of potassa, a pipette marked with either of the substances which the fluid contains, on being filled with the solution up to the mark on the stem, and then drained into a Phillips' jar, the solution colored blue with litmus, and neutralised with oxalic acid, as before directed, then the number of decimillems of oxalic acid solution used will give the per centage.

Ammonia.—The per centage of liquid ammonia of the specific gravity of ·875 is found in precisely a similar manner, by taking a pipette marked "Ammonia," and filling it up to a mark on the stem with the solution, diluting with water, adding litmus to color, and neutralising with oxalic acid as mentioned for soda; and the number of decimillems of the acid solution used gives the per centage.

If the number of decimillems of acid solution used is multiplied by ·347, the product is equal to the per centage of dry ammonia in the sample.

Chlorimetry.—Chlorimetry is a process by which the per centage of chlorine in the chloride of lime of commerce is found: the process most easily managed is based on the rapid peroxidation of a solution of arsenious acid in water; this action is so energetic that, if the solution of arsenious acid is colored blue by a solution of sulphate of indigo, the addition of a solution of chlorine will not act on this color so long as the slightest trace of arsenious acid remains unchanged into arsenic acid.

To prepare Arsenious Acid solution for Chlorimetry.—Weigh out 99· grns.* of pure arsenious acid, and put it into the decem bottle, fig. 2; add to it 5 ozs. of pure muriatic acid, and dissolve by the application of heat; when the solution is complete, add 10 ozs. of distilled water, agitate so as to mix thoroughly, then allow the whole to remain until it has cooled down to 62° F.; when this has taken place, add water until the solution rises to the mark on the neck of the decem bottle, then pour out into a stoppered glass bottle for use.

To ascertain the per centage of Chlorine in Chloride of Lime.—Weigh out 710 grns. from a fair average sample of the chloride of lime to be examined, place this quantity in a porcelain mortar and triturate with a few drops of water, then add water, gradually con-

* NOTE.—Five times the atomic number of arsenic acid is not soluble in a decem of water.

tinuing the trituration until the whole becomes a homogeneous fluid mixture without lumps, and about 4 ozs. of water have been added. Pour the whole into the decem bottle, and add 4 ozs. more water to the remaining powder in the mortar, triturate well, and then pour this quantity also into the decem bottle, add more water to any residue that may remain in the mortar, and add this to the former quantities; continue to do this until the decem bottle is nearly full and no more powder remains in the mortar, add water to fill up the bottle to the mark on the neck, agitate so as to mix intimately, and fill the chlorimeter, fig. 5, up to *o* with this fluid. Now take a centem of the arsenious acid solution, by using the centem pipette, put it into a Phillips' jar, fig. 7, add 10 drops of a solution of sulphate of indigo; agitate the jar so as to impart to the contents a rotatory motion, then drop in the chloride of lime solution from the small beak of the chlorimeter, still keeping up the circular agitation, until the blue tinge disappears on that spot where the chlorine solution falls; now add the solution drop by drop until the blue tinge given to the arsenious acid by the indigo disappears; but care must be taken to stop the addition of the chlorine solution the moment this color vanishes; stand the chlorimeter upright, so that whatever fluid is adhering to the sides may drain down, then the number on the division at which the remaining fluid stands is equal to the per centage of chlorine in the sample.

The divisions on the chlorimeter are different from those usually employed: this alteration is introduced to obviate the necessity of the calculations attendant on the old plan.

Valuation and dilution of Acids, Ammonia, &c.—The money value of samples of acids and ammonia of different specific gravities is frequently required by the manufacturing chemist, and as the specific gravity gives no true indication of the chemical strength, the process of centigrade testing herein advocated will prove useful, as the results obtained are in per centages of a commercial article, and, what is of equal importance, all the results are obtained without the slightest calculation. In order still further to assist the dissemination of scientific information devoid of any abstruse formula, I have introduced the COMPARATIVE RULE, from which the money value of samples of fluids of different strength can be ascertained without calculation: thus, if nitric acid of 98 per cent. is worth 8*d.* per lb., what is the value of an acid of 55 per cent.? And the rule gives 4½*d.* per lb. as the answer.

The comparative rule also enables a manufacturer to determine at a glance, without the slightest calculation, how much of a strong fluid and how much water are necessary to be used to form a weaker fluid of a given measure of the required strength. For example: A dyer wishes to make 104 lbs. of sulphuric acid of 6 per cent. from acid of 96 per cent.; what amount of the strong acid must be taken to form the required measure of the weak acid? The answer obtained by the mere setting and inspection of the rule is 6½ lbs.

Description of the Comparative Rule, and directions for use.—This instrument is made of box-wood, ivory, or brass. It is divided

and numbered on both sides; the side marked A B C being for determining the comparative value of different samples of the same acid or ammonia, and is marked VALUE. The slide B and the side E are divided and numbered from 10 to 100, the side A from 6 to 60, and the side C 1 to 10. The sides D F, marked DILUTION, are divided in a similar manner, but in a contrary direction.

The first thing is to acquire a perfect knowledge of the value of the different numbers on the sides of the rule, and this once acquired, the operation of using it will be rendered easy. If the 10 on the side marked F represent 10 lbs., the next short division will be 11 lbs., and so on to 20, 30, up to 100; but if we set the value of 1 on the 10, then the next short division is one and one-tenth, 20 will be two, and 100 ten; and in the same manner, if we call 60 on the side D 60 lbs., 100 will be 100 lbs.; but if we call 60 six, then 100 will be ten only.

For the side marked VALUE, the same remarks apply. Thus, if 10 on the side C is made to represent one penny or one shilling, then 1 will be one-tenth of that sum, and 5 one-half; and if 6 on the side A represent 6d., then the fourth *long* division past the 10 will be 12d., 20 will be 20d., and so on for the rest, bearing in mind that each *long* division on the dilution side, up to 200, is equal to 5; from 200 to 600 each long division is equal to 10, and that each short division up to 200 is equal to 1, from 200 to 400 is equal to 2, from 400 to 600 equal to 5.

A few examples will fully illustrate the means of finding any number on the rule. Thus, let it be required to find 164 on the DILUTION side; look first for 150, then count two of the long divisions towards 200, which gives 160, and 4 short divisions will be 164, the number sought.

The number 372 is required. Count 7 long divisions from 300 towards 400, which is 370, and the next short division is equal to 2, making 372.

The number 425 is required. Count two long divisions from 400 towards 500, which gives 420, and the next short division gives 5, making 425, the number required.

Examples to find the comparative value of two samples of the same fluid of different per centages.—The per centages of the various substances can be found, as before indicated, by centigrade testing.

We shall speak of one acid only in connection with each example; but the reader will perceive that the same mode of operation applies to all the fluids to which the centigrade process is applicable.

Suppose nitric acid, of 92 per cent., is worth 9½d. per lb.; what is the value of the same acid of 30 per cent.?

Set 92 on B against 9·5 on C, and against 30 on B is 3·1d., the answer on C.

Sulphuric acid, of 90 per cent., is worth 2d. per lb.; what is the value of an acid of 36 per cent.?

Set 90 on B to 20 on A, then against 36 on B will be 8 on A, and as in this case we represent the 20 on A as equal to 2d., 8 is therefore

eight-tenths of a penny ; the acid is therefore eight-tenths of a penny per lb.

Ammonia, of 96 per cent., is worth 6d. per lb.; what is the value of ammonia of 40 per cent. ?

Set 96 on B to 6 on C, then against 40 on B will be found the answer—2·5 or 2½ on C.

NOTE.—In every operation the per centages must be found on the slide B or E, and not on the sides.

Examples for making a weak Solution of Sulphuric, Nitric, or Muriatic Acid, or Ammonia from a stronger one.—Sixty pounds of acid of 40 per cent. are required to be made from sulphuric acid of 72 per cent.

Set 40 on E to 60 on F, then against 72 on E will be found 33⅓ on F ; shewing that if we take 33⅓ lbs. of acid of 72 per cent. and dilute it with water until it makes 60 lbs., we shall obtain the quantity of dilute acid of the required strength.

How much muriatic acid of 80 per cent. is required to make, when diluted with water, 56 lbs. of muriatic acid of 10 per cent. ?

Set 10 on E to 560 on D, then against 80 on E will be found 70, the answer on D ; but as we must in this case call 560 fifty-six, we must also call 70 seven ; therefore 7 lbs. of the strong acid will make 56 lbs. of the strength required.

A carboy holding 90 lbs. is to be filled with nitric acid of 30 per cent., how many pounds of acid of 84½ per cent. must be employed to form the required weight of solution when diluted with water ?

Set 30 on E to 90 on F, then against 84½ on E will be found 32 on F ; consequently, 32 lbs. will be required.

How many pounds of ammonia of 99 per cent. is contained in 100 lbs of 49 per cent. ?

Set 49 on E to 100 on F, then against 99 on E will be found 49½ on F ; consequently, the answer is 49½ lbs.

I have thus shewn how acids, alkalies, ammonia, can be tested, and the comparative value known, and how the per-centage of chlorine in bleaching-powder may be obtained, by the judicious use of instruments, by any common workman, without the slightest knowledge of figures ; and this process can be carried on and developed to an infinite extent, applying to many of the chemicals, &c., the values of which are at present only partially known. Indeed, the many uses to which graduated instruments are now being applied, calls forth the most strenuous efforts to render their graduation accurate ; and I trust my exertions to promote this object have not been in vain.

CHEMICAL TABLES. COMPILED BY THORNTON J. HERAPATH.

No. I.—DENSITIES OR SPECIFIC GRAVITIES.

(Concluded from page 395.)

When an * is attached to a name, it must be understood that the *preceding* numbers have been merely copied from some standard work, and are not the result of the Author's experiments.

Substances.	Density at 60° F.	Authority.
Stone, Collalo	2·423	Rennie.
„ Craigleith sandstone	2·360	Tredgold.
„ „	2·452	Rennie.
„ Cutler's	2·113	Moseley.
„ Dundee	2·517	Tredgold.
„ „	2·530	Rennie.
„ Granite	which see.	
„ Grinding	2·143	Tredgold.
„ Kentish-rag	2·675	„
„ Ketton roe-stone	2·058	„
„ „	2·498	Kirwan.
„ Liege	2·636	Cresy.*
„ Limerick limestone	2·598	Rennie.
„ Lorraine	2·529	Cresy.*
„ Marble	which see.	
„ Millstone	2·484	Templeton.
„ Paving	2·4158	„
„ „	2·708	Moseley.*
„ Pannarth limestone	2·653	Watson.
„ Portland roe-stone	2·113	Tredgold.
„ „	2·423	Rennie.
„ „	2·570	Templeton.
„ Pumice	0·629	R. Tredgold.
„ Purbeck	2·599	Rennie.
„ „	2·601	Templeton.*
„ „	2·680	Watson.
„ Rotten	1·981	Cresy.*
„ Ray	2·470	„
„ Sandstone, for paving	2·4158	„
„ Scythe	2·609	„
„ Touch	2·415	„
Stone, Whinstone	2·760	Watson.
Stromnite	3·903	Trail.
Strontia, anhyd.	3·0 to 4·0	H. Davy.
„ „	3·932	Karsten.
„ „	4·611	Filhol.
„ crystal hyd.	3·635	„
„ do. with 9 at aq.	1·396	„
„ carbonate of	3·605	Mohr.
„ „	3·6245	Karsten.
„ „	3·6600	Gray.*
„ „	3·6750	Moseley.*
„ calcareous, sulphate of,	2·750	Thomson.
„ nitrate of, anhyd.	2·857	Filhol.
„ „ crys.	2·704	Playfair and Joule.
„ sulphate of	3·588	Karsten.
„ „	3·770	Filhol.
„ „	3·958	Moseley.*
Strontianite	3·713	Thomson.
Strontium, chloride of,	2·960	Filhol.
„ „	2·803	Karsten.
„ „ crys.	2·015	Playfair and Joule.
Struvite	1·700	Ulex.
Styrole	0·924	Blyth & Hofmann.
Succineupion	0·645 at 43°	Elsner.
Succinic acid, hyd.	1·550	Brande.*
Sugar, cane and beet	1·5 to 1·6	„
„ „	1·5629	Thomson.*
„ „	1·6065	Fahrenheit.
„ „	1·5929 at 39°·1	Playfair and Joule.
„ grape	1·500	Proust.
„ „ syrup of,	1·215 (!)	„

Substances.	Density at 60° F.	Authority.
Sugar, milk	1·543	Brande.
„ „	1·5339 at 39°·1	Playfair and Joule.
Sulphur, native	1·97 to 2·08	Brande.*
„ „	1·989 to 2·050	Karsten.
„ „	1·898	Fontenelle.
„ „	2·027	Osann.
„ „	2·072	Moha.
„ „	2·066	Marchand.
„ flowers of,	2·086	Le Royer & Dumas.
„ dichloride of,	1·686	Marchand.
„ „	1·687	Dumas.
„ chloride	1·620	„
„ „	1·625	Marchand.
Sulphuric acid, anhyd	1·97 at 65°	Bussy.
„ „ „	1·946 at 55°	Morveau.
„ Nordhausen	1·896	Thomson.
„ 1-hyd.	1·848	H. Davy.
„ oil of vitriol	1·8455	Brande,* &c.
„ 2-hyd.	1·7800	Chaptal.
„ „	1·7840	Wackenroder.
„ 3-hyd.	1·6321	Ure.
Sulphurous acid, liq.	1·420	Faraday.
„ „	1·450	Bussy.
Sussanite	6·3 to 6·5	Brooke.
Sylvanite	6·343	Cresy.*
Syenite	2·621	Tredgold.*
„	2·5 to 3·0	Portlocke.*
Synovia	1·050	Dupuytren.
Synaptase or emulsine	+1	Dumas.
TACHYLITE	2·7144	Klett.
Talc	2·7 to 2·8	Cresy.*
„	2·697	Thomson.*
Tallow	0·941	Moseley.*
Tankelite	4·5577	Thomson.
Tantalic acid	7·0 to 7·1	H. Rose.
„ ignited	8·2	„
Tantalite	7·236 to 7·655	Berzelius.
„	7·264	Nordenskiöld.
„	7·963	Ekeberg.
Tautolite	3·865	Breithaupt.
Tartar	1·849	Cresy.*
Tartaric acid	1·600	Gray.
Teeth, enamel of,	2·688 to 2·843	Thomson.*
„ ivory of,	1·892 to 2·105	„
Tellurium, graphic ore of,	5·723	Reichenstein.
„ Nagyag ore of,	6·840	Thomson.*
„ native	5·7 to 6·1	W. Phillips.
„ sulpho-plumbifer.	6·840	Berthier.
„ white ore of,	10·673	Reichenstein.
Tennantite	4·375	R. Phillips.
Terpinole	0·852	List.
Tereben, bromide	1·978	Deville.
„ chloride	1·360	„
„ hydriodate	1·084	„
„ hydrobrom.	1·021	„
„ hydrochlor.	0·902 all at 68°	„
Terra-japonica	1·398	Cresy.*
Thenardite	2·730	Thomson.*
Thialdine	1·1910	Wöhler and Liebig.
Thomsonite	2·290 to 2·3696	Thomson.
Thorina	9·402	Berzelius.
Thorite	4·630	Thomson.*
Thujon	—1	Schweizer.
Thulite	3·1055	Thomson.
Tile, common	1·815	Belidor.
„ „	1·853	R. Tredgold.
Tin, chloride	2·759 at 39°·1	Playfair and Joule.
„ peroxide	6·640	W. Herapath.
„ „	6·712	Playfair and Joule.
„ „	6·900	Boullay.
„ „ hyd.	4·933	Berzelius.
„ protoxide	6·666	W. Herapath.
„ pyrites	4·35 to 4·76	Klaproth.

Substances.	Density at 60° F.	Authority.
Tin, sulphide	5·267	Doullay.
Titaniferous iron	4·66 to 4·78	Von Kobell.
„ „	4·545	Klaproth.
„ „	3·153	Möhl.
Titanic acid	4·128	Brande.*
	3·899 to 3·960	Rose.
„ „ ignited	4·206 to 4·254	„
Titanium, native	5·300	Wollaston.
Toad-stone	2·921	Watson.
Tolene	0·858	Kopp.
Topaz	3·499	Haidinger.
„	3·641	Lowry.
„ oriental	4·061	Moseley.*
	3·951	Thomson.
	4·016	Cresy.*
Tourmaline	3·0 to 3·06	Thomson.*
„	3·076	Haidinger.
„	3·14	Leplay.
„	3·059 to 3·246	Gmelin.
Tourquoise	2·6296 to 3.2500.	Thomson.*
„	3·000	Moseley.*
Treacle	1·400	Ure.*
Tremolite	2·9257 to 3·200	Hauy.
Triphyline	3·600	Fuchs.
Tripoli	1·9 to 2·2	Ure.*
Troostite	4·0 to 4·1	Thomson.
Truesite	2·558 to 2·628	Thomson.*
Tschewkinite	4·500	Hermann.
Tufa, Roman	1·217	Rondelet.
Tungstic oxide	12·110	Karsten.
„ acid	5·274	W. Herapath.
„ „	6·120	De Luyert.
„ „	7·139	Karsten
„ ochre	6·000	Silliman.
ULTRAMARINE	2·360	Moseley.*
Umber	2·200	Ure.*

Substances.	Density at 60° F.	Authority.
Unionite	3·298	Sillimann.
Uralite	3·150	G. Rose.
Uranic oxide, hyd.	5·926 at 59°	Malaguti.
Uranite or Uran-mica	2·990	Moseley.*
	3·120	Champeaux.
	3·198	Th. Herapath.
Uranium, native protoxide	6·468	Thomson.*
„ „	6·940	Richter.
Uranoso-uranic oxide	7·193	Karsten.
Urea	1·350	Prout.
	mean.	
Urine, healthy human	1·017 1·0183	Becq. Thomson.
„ „	1·020	Prout. Bird.
„ „	1·0204	Chassat.
„ „	1·030	Brande.*
„ „	1·0016 to 1·0388.	Chassat.
„ „	1·0050 to 1·033	Cruickshank.
„ „	1·010 to 1·038	Lecanu.
	mean.	
„ of man	1·0189	Becquerel.
„ of woman	1·0151	„
„ of cow	1·035	Boussingault.
„ of horse	1·03 to 1·05	Fourcroy & Vauquel.
„ „	1·0293	Prout.
„ „	1·064	Boussingault.
Uwarowite	3·41	Komonen.
VALERIANIC acid	0·944	Trommsdorff.
Valero, aceto-nitril	0·790	Gluckenberger
„ „	0·810	Schlieper.
„ „	0·813	Gluckenberger.
Vanadite	6·99 to 7·23	Johnston.
Valyl	0·894 at 65°	Kolbe.
Varvicite	4·283 to 4·531	Phillips.
Vauquelinite	6·8 to 7·2	Berzelius.
Vermiculite	2·5252	Thomson.

Substances.	Density at 60° F.	Authority.
Vesuvian	3·420 to 3·575	Moseley.*
„	3·339 to 3·407	Cresy.*
Vesuvian-idocrase	3·349 to 3·990	Thomson. [zeaux.
Villenite	4·154	Delesse & Descloi-
Vivianite	2·661	Thomson.*
Vinegar, Revenue Pr.	1·0085	Moseley.*
„ Malt	1·014	Ure.
Volborthite	3·550	Phillips.
Voltzine	3·660	Thomson.
Vulpinite	2·8787	Hauy.
Wacke	2·55 to 2·90	Ure.*
Wad	2·314 to 3·700	Phillips.
Wagnerite	3·068 to 3·130	Fuchs.
Warwickite	3·00 to 3·14	Shepard.
Water	1·000	
„ sea	1·026 to 1·028	Brande.*
„ Dead sea	1·153 to 1·245	
Wavellite	2·337	Haidinger.
„	2·253	Gregor.
„	2·361	Richardson.
Wax, bees'	0·960	Brande.*
„	0·964	Moseley.*
„ Brazilian	0·980	Brande.*
„ Japan vegetable	0·970	Thomson.
„ white	0·968	Moseley.*
„ „	0·8203 to 0·9666	Fabroni.
„ shoemakers'	0·897	Moseley.*
Weisserite	2·808	Thomson.*
Wermelandite	3·383 to 3·538	Berzelius.
Whet-slate	2·722	Ure.*
Whey, cows'	1·019	Moseley.*
Wichtyn	3·030	Laurent.
Williamsite	3·935	Thomson.*
Wine, Bordeaux	0·993	Moseley.*
„ Bucellas	0·979	Ure.*
„ Burgundy	0·983 to 0·9854	„
„ „	0·991	Moseley.*
Wine, Calcavella	0·979	Ure.*
„ Cape, Madeira	0·979	„
„ Champagne, rose	0·986	„
„ „ white	0·9845 to 0·9970	„
„ Constantia	1·081	Moseley.*
„ „	0·977	Ure.*
„ Cote-roti	0·9849	„
„ Currant	0·9769	„
„ Elder	0·9876	„
„ Frontignac	0·9845	„
„ Gooseberry	0·9855	„
„ Hermitage, red	0·9849	„
„ „ white	0·9799	„
„ Hock	0·9829 to 0·9887	„
„ Lisbon	0·9784	„
„ Madeira	0·9733 to 0·9781	„
„ Malaga	0·980	Ure.*
„ „	1·022	Moseley.*
„ Muscat.	0·979	Ure.*
„ Port.	0·972 to 0·976	„
„ Raisin	0·972	„
„ Roussillon	0·980	„
„ Schiraz	0·982	„
„ Sherry	0·977 to 0·9791	„
„ Syracuse	0·982	„
„ "Vin de Grave"	0·9845	„
Withamite	2·857	Thomson.
„	3·137	Turner.
Witherite	4·292	Hauy.
„	4·298	Thomson.
„	4·301	Mohs.
Wohlerite	3·410	Shepard.
Wolfram	7·191 to 7·544	Schaffgotsch.
„	7·002	Richardson.
Wollastonite	2·850 to 2·876	Thomson.
Wood-ashes, with interstices	0·933	Tredgold.
Wood, petrified	2·341	„

Substances.	Density at 60° F.	Authority.
Wood, petrified,	2·011 to 2·531	Th. Herapath.
Woods:—Ash	0·845	
„ Alder	0·800	
„ Beech	0·852	
„ Cedar, American	0·561	
„ Chestnut	0·610	
„ Cork	0·240	
„ Deal	0·590	
„ Elm	0·673	
„ Fir	0·546 to 0·753	
„ Larch	0·530	
„ Mahog., Spanish	0·800	
„ „ Honduras	0·637	
„ Oak, African	0·944	Templeton.*
„ „ Dantzic	0·756	
„ „ American	0·672	
„ „ English	0·934	
„ „ Plane	0·640	
„ Pine, pitch	0·660	
„ „ yellow	0·461	
„ Poplar	0·383	
„ Poonah	0·640	
„ Spruce	0·551	
„ Sycamore	0·604	
„ Teak	0·750	
„ Walnut	0·671	
Xanthite	3·201 to 3·221	Thomson.
Xanthophyllite	3·0 to 3·1	Gmelin.
„	3·044	G. Rose.
Xylite	0·816	Gmelin.
Yttria	4·842	Ekeberg.
„ native phosphate	4·557	Thomson.
Yttrocerite	3·447	„
Yttroilmenite	5·398 to 5·45	Hermann.
„	5·6142	Wornam.
„	5·6170	H. Rose.
Yttrotantalite	5·130	Cresy.*
Yttrotantalite, black	5·395	Thomson.*
„ „	5·670	Peretz.
„ yellow	5·882	Ekeberg.
„ ignited	6·400	Peretz.
Zeilanite	3·6 to 3·8	Abich.
Zeolite	2·718	Moseley.*
Zeuxite	3·051	Thomson.
Zinc, chloride of,	1·577	Gray.*
„ blende	4·049	Thomson.*
„ „	3·90 to 4.07	Karsten.
„ ferruginous blende	3·2 to 3·6	Bouis.
„ „	3·850	„
„ oxyde of,	5·430	Mohs.
„ „	5·600	Boullay.
„ „	5·612	Filhol.
„ „	5·734	Karsten.
„ „ crys.	5·5298	Messrs. Herapath.
„ „ „	6·000	Vernon.
„ „ hydr.	3·053	Filhol.
„ sulphate	1·930	Gray.
„ „	2·036	Filhol.
„ „ anhyd.	3·400	„
„ sulphide	3·923	Karsten.
„ red	5·432	Thomson.*
„ selenide	5·560	„
Zinkinite	5·303	G. Rose.
Zircon	4·681	Thomson.
„	4·721	Lowry.
„	4·505	Haidinger.
„	4·615	Henneberg.
„ ignited	4·710	„
Zirconia	4·300	Klaproth.
„	4·850	Vauquelin.
Zoisite	3·320 to 3·3207	Klaproth.
„	3·170 to 3·430	Hermann.
Zurlite	3·274	Ramondini.

On Percolation.* By Mr. Collins.

Gutta cavat lapidem; non vi, sed sæpe cadenda.

This truth was uttered by the great Latin poet two thousand years ago; and whether this thought, so well expressed by Horace, or accident, that mother of inventions, first gave birth to the idea, that Percolation might be brought to bear upon the natural kingdom, to wrest from it its latent treasures, the application will not, perhaps, be considered far fetched; for if the stubborn rock would yield to this unpretending influence, how much rather the solid wood, the corrugated bark, the leafy twig, the tiny seed, the attenuated flower, the ductile gum, and pulpy fruit. Sources from which, taught by science, we draw the remedies to cure or alleviate "the various ills which flesh is heir to." We know not the birth and parentage of Percolation; but as a means of obtaining in solution the active properties of the various Therapeutic agents contained in the vegetable kingdom, it has long been known, and by some persons extensively employed, but it may be doubted if it obtain anything like general acceptance, considerable prejudice existing in favor of the old processes; and some chemists quite refuse to admit the possibility of a tincture made in a few hours, being at all comparable to one macerated for the magic 14 days.

Having myself at one time entertained these conservative views, but now seeing the error of my way, I am desirous of making amends by endeavoring to dispel any misapprehension that may exist on the subject. But before proceeding any further, it will be well to say that a Percolator is in itself very simple: any elongated vessel, open at each end, would serve. Very convenient ones are made of tin. An inverted cone as Percolator, fitting by means of a bung into a jar-shaped receiver, having a tap at the bottom. I have taken a photographic view of one, which will give a better idea than any description.

There are three pharmaceutical operations to which the above cold process is applicable, and with but few exceptions preferable, combining efficiency, economy and celerity, viz., cold maceration, infusion and decoction.

* Read before the "Chemical Discussion Society," February 23rd, 1854.

The first mentioned process may be said to be specially applicable to what are called tinctures. In making tinctures, the following will be found a good mode of procedure:—reduce the ingredients employed to a coarse powder, taking care that there be no great inequalities, for which purpose it would generally be expedient to pass the materials through a proper sieve, then let an equal bulk of silver sand (carefully washed and dried) be mixed with them, place some tow at the bottom of the Percolator, upon which place some of the plain sand to the depth of from one to two inches; let this be well packed, then put in the ingredients, mixed with the sand, packing lightly or closely, according to the character of the ingredients. Substances, that would swell when wetted, should be lightly packed, such as orange peel, gentian, columba, &c.; whilst resinous substances, diminishing in bulk, should be pressed more closely together, such as myrrh, guaiacum, &c. When the whole of the ingredients are in the Percolator, place some of the sand to the depth of two or three inches on the top; this will prevent the ingredients being disturbed and floating about; upon this, pour the necessary quantity of rectified or proof spirit, which must be allowed to permeate slowly through, drop by drop. When the liquid has ceased dropping for some time, measure the product, the deficiency to be made up by water, which should be carefully poured into the Percolator, so as not to disturb the sand, and thus mix with the spirit. The spirit left in the Percolator having to be displaced by the water, great care must be taken at this stage of the proceedings. A convenient way of breaking the force of the water in falling is to to stick a bung on a fork, and bringing the bung within half an inch of the sand, let the water be poured gradually on it, by which means it will flow on to the sand, without causing much inconvenience.

Should any one from these remarks be inclined to test the power of Percolation, I would advise the making an essence of ginger by the above process in the following proportions:—ginger root, bruised (Jamaica), 1lb. avoird., rectified spirit 3 pints imperial; and I venture to say satisfaction will ensue, and a stinging testimony be afforded in favor of the process.

Whilst on this first portion of the subject, I would here state that the alcoholic extracts are readily and efficiently prepared by displacement. I have prepared the alcoholic extract of Nux Vomica thus most successfully and productively. The extract is prepared in the usual way, substituting Percolation for Maceration. With regard to vegetable infusions little need be said, the ready mode of keeping these preparations in a fresh state by bottling them, and exhausting the air by heat, precludes the necessity for concentration; but where concentrated infusions are desired, a good way of proceeding, if a concentration of one to eight be required, is to place eight times the quantity of the ingredients for the ordinary infusion, and pass the one quantity of liquid through; but, instead of plain water, let one-fifth part of rectified spirit be added. If the product thus obtained be diluted, one part to seven of water, a good substitute will be found for the fresh infusion. I have thus treated rhubarb and columba, and with

success, though always preferring the fresh infusion, and merely mentioning this incidentally.

The third portion of the subject will now engage our attention; viz., Percolation as a substitute for decoction, a process we all know to be extremely tedious; and could its inutility be shewn, it would doubtless be speedily abrogated. I need not tell you what a troublesome preparation syrup of poppies is to make in the ordinary way. To avoid so much stewing, boiling, and pressing, I determined on making trial of Percolation, and the result exceeded my most sanguine expectations.

Take the quantity of poppy heads (free from seeds) ordered in the Pharmacopœa, of 1851, viz., lbiij; let them be well broken up in a mortar, so as to pass through a coarse cane sieve, pack rather closely in the Percolator, without the intervention of sand; the quantity of water not being limited, sand is not necessary; then pass cold water through till the liquid passes devoid of taste, color, or smell.—Let the liquid be evaporated to Oij imperial; I did so over gas in an iron pan, lined with porcelain; then pour into some elongated vessel, and allow to stand for some hours, when it may be poured off quite bright, the albuminous particles having subsided. The liquid thus prepared will keep for any time if treated in the same way as infusions, viz., bottling and exhausting the air by boiling and corking whilst hot. I used 16-oz. bottles, as the quantity was convenient for use; 2 lb. avoirdupoise of sugar being added to the 16 oz. of liquor; the syrup should be boiled for about two minutes, and well skimmed; about ʒij of water may be added at first, to make up for loss by evaporation. I never found that the syrup, thus prepared, either fermented or crystallised, and as a sedative it is far more efficacious than that prepared in the ordinary way; insomuch so, that some care must be exercised in its administration to infants.

The foregoing successful issue induces me to think that many extracts may be prepared in this manner; but as most of the materials used are bulky, I would suggest the use of a small cask for a Percolator, having a false bottom perforated, and open at the top; the bottom to have a tap in it; a 9-gallon beer cask would do well.

One great requisite would be to reduce all substances employed to an uniform state of subdivision; and as the quantity of water would not be limited, no sand need be employed. It would be well, perhaps, to have a perforated piece of tin, to lie loosely on the top, as by this means the ingredients would be kept from floating. The plan of operation would be to pass water continuously through, till neither taste, color, nor smell remained. The former portions of the percolated liquid might be evaporating whilst the latter were percolating, so that no risk of decomposition need be encountered by the protraction of the process. One great advantage of this plan is, that only the soluble medicinal properties of the agent employed would be in the extract, the woody fibre and dirty attachments being left behind; these useless encumbrances not being able to escape from the fierce embrace of the stolid Percolator.

I would here mention that it would, perhaps, be as well to lay tow and sand on the false bottom in the same manner recommended for tinctures.

Where it would be desirous to get rid of albumen, the liquid, after undergoing evaporation to a given point (before thickening), might be laid aside to cool, and then decanted, by which means the objectionable admixture might be got rid of.

The following extracts might be made by Percolation, viz., Extract Aloes Aquos (using sand); Extract Colchici; Extract Colchici Acet. (using, of course, vinegar instead of water); Extract Gentian; Extract Glycyrrh; Extract Opii; Extract Papav. Alb.; Extract Sarzæ; Extract Taraxaci; and others of the same character.

I cannot say that I have tried Percolation in all the above instances; but by the induction of analogy I should deem them fair game for experiment. Upon the economical portion of the subject I need scarcely enter, as most gentlemen present are well able to test that point for themselves. I am convinced that preparations of superior efficacy may be obtained by the cold process of displacement at no advance in the cost of production; the cost of apparatus would be but a few shillings, and the saving of time most considerable, as the operation requires but little attention.

If these suggestions have the effect of rendering any of our brethren less dependent upon others for the medicinal agents in daily use, it will be a source of some satisfaction to me, and, perhaps, may compensate my audience for engaging their attention upon a somewhat dry and tedious subject.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

On the CHEMICAL COMPOSITION *of* POLLEN.

BY MM. E. FREMY & CLOEZ.

THE careful microscopical examinations of MM. Mohl, Brown, Payen, Decaisne, and especially of M. Fritzsche, shew that pollen is not an immediate principle, for it is essentially formed of external membranes enveloping an internal substance, to which has been given the name of *Fovilla*.

The composition of fovilla appears to be very complex; there are found in it a thick liquid, some oily drops, small granular corpuscules, and sometimes starch. The grains of pollen of some plants present besides on their exterior a fatty, viscid matter, which incompletely unites them to each other.

We have thought that, to complete this study of pollen, it would be important to examine chemically the immediate principles which constitute it, to determine the composition of the membranes which cover the grain, likewise to analyse the fatty bodies which are enclosed in

this membrane, or are found on its surface; to make, in one word, a complete analysis of pollen. This analysis would enable us to compare the chemical composition of pollen with that of other vegetable substances. Such is the object of our present publication.

External Fatty Matter of Pollen.—We have mentioned that the grains of pollen have frequently on their surface, a fatty, glutinous substance, which causes them to adhere to each other.

The chemical study of this substance was of some importance. Some botanists have imagined that it was of the same nature as the oil existing in the interior of the grains of pollen; others have advanced that it proceeded from an incomplete decomposition of the cellule, in which the pollen was in some sort developed. It is evident that the chemical analysis of this substance ought to throw some light on its real nature.

Our observations were made on the pollen of the orange lily (*Lilium croceum*), which contains a great quantity of this fatty external substance, the presence of which was proved by the microscope.

We easily extracted this fatty substance by means of ether, which dissolved it immediately without altering the grain of pollen.

It is of a deep yellow color, unctuous and sticky, insoluble in water, little soluble in alcohol, even when boiling; ether dissolves it rapidly. It is impossible to obtain a crystalline matter from it. The alcoholic or ethereal solutions always deposit it in a viscid state. It is neutral to colored reagents, and often possesses a powerful waxy odor. The alkalies saponify it with difficulty, and always incompletely. This soap was decomposed by an acid, and gave rise to an acid fatty liquor resembling oleïc acid.

This substance shewed us the following composition :—

	I.	II.
Matter . . .	0·431	0·267
Water . . .	0·467	0·291
Carbonic Acid .	1·257	0·780
	Per Cent.	Per Cent.
Carbon . .	79·53	79·65
Hydrogen . .	12·00	12·09
Oxygen . .	8·47	8·26
	100·00	100·00

It is thus seen that this fatty body resembles the composition of some species of vegetable wax analysed by M. Lewy.

The yellow substance which colors the external fatty body of pollen is rapidly destroyed in contact with the air, under the influence of even diffused light.

The experiments which we have just described prove that the external fatty substance of pollen presents a certain analogy to yellow wax. It ought not to be considered as a pure immediate principle, for it is evidently formed of a yellow coloring matter, of a fatty body

saponified by alkalies, and of another fatty body which resists the action of concentrated potassa.

We think that this substance exists in certain species of yellow wax, and that it gives them their color and plasticity.

It does not appear to us possible that this external fatty body should be the remains of the cellule which first contained the grain of pollen, for there is no connexion between the composition of this fatty substance and that of the nitrogenous and non-nitrogenous membranes which constitute the cellules.

M. Decaisne, in a work on vegetable physiology, had already established, by anatomical considerations, that the external fatty body of pollen was entirely independent of the substance which constitutes the cellule.

Examination of the immediate principles which constitute the Pollen of the Lily.—Our observations were made chiefly on the pollen of the white and orange lilies, of which we could procure a sufficient quantity in the gardens of the museum.

We do not now insist on the form of this pollen and the nature of its external membrane and of its fovilla: we shall only describe the modifications which the pollen of the lily undergoes by the action of the principal reagents, and we shall make known the composition of the immediate principles which may be taken from them.

Cold water had no perceptible action on the pollen of the lily; but when the water boiled the liquor became of a yellow color and charged with a gummy substance, which was nothing but dextrine; by prolonging the action of heat this dextrine was transformed into glucose.

This experiment leaves no doubt on the existence of starch in the interior of the pollen, as had been, moreover, proved by M. Payen. When the pollen of the lily was put in contact with iodised water, the blue coloration was not immediate; but the iodised solution soon introduced itself into the grain by endosmose, traversed the external membranes which it did not color, and then acted on the starch contained in the membranes.

The transformation of the starch of the pollen into dextrine and glucose leads to the presumption that there exists in the interior of the grain an active substance, a ferment, which acts on the starch in the same manner as diastase.

Moreover, the presence of a soluble nitrogenous matter in pollen is not doubtful; we have ascertained it in the aqueous extract, and we have likewise found that during the evaporation a substance always coagulates which may be compared to albumen or rather to caseine. These bodies may evidently determine the disaggregation of the starch.

Alcohol and ether do not act on the grain o fpollen when entire; but these solvents exercise, on the contrary, a rapid action on the pollen when it has been triturated for some hours.

During this mechanical operation, the pollen becomes of a yellow color, takes a fatty appearance, and will then grease paper in the manner of an oleaginous grain which has been subjected to pressure. If the pollen be thus bruised with alcohol, or better, with ether, this latter

liquid will be observed to remove from the pollen a considerable quantity of oil which is readily obtained by the evaporation of the ether.

The oil of pollen presents all the characters of a fat oil; it saponifies with ease, in which it differs from the external fatty body of the pollen; its color is slightly yellow; it gave us the following composition:—

Oil	0·338
Carbonic acid	0·934
Water	0·392
In hundredths.	
Carbon	75·36
Hydrogen	12·90
Oxygen	11·74
	100·00

By its composition it therefore resembles entirely the fat oils properly so called, extracted from oleaginous grains.

Pollen exhausted by boiling water, by alcohol and ether, leaves a very abundant residue evidently formed of the external membranes of the pollen and perhaps of a part of the fovilla which resisted the action of the solvents. We thought that an elementary analysis might throw some light on the nature of this residue, we give here the result of an analysis made on that part of the pollen which was insoluble in boiling water, alcohol, and ether.

1·063 gr. left by incineration, 2·35 per cent. of ash; this ash was alkaline, and contained phosphate and carbonate of lime.

Matter (the ash abstracted)	0·370
Water	0·246
Carbonic acid	0·709
Estimation of the Nitrogen.	
Matter	0·696
Nitrogen at 0° and at $0^m 76'$ pressure	0·0754
Composition in hundredths.	
Carbon	52·25
Hydrogen	7·38
Nitrogen	10·83
Oxygen	29·54
	100·00

The considerable proportion of nitrogen still existing in this residue, led us to think that it must retain some substance of an albuminous nature; we, therefore, treated it several times with a concentrated solution of potassa, which really removed a nitrogenous substance, soluble by alkalies and precipitable by acids.

The residue left by the alkali was again purified by solvents, and then subjected to analysis: it gave the following numbers:—

0·466 gr. left by incineration, 1·71 per cent. of ash.

Matter (the ash abstracted) . . .	0·288
Water	0·196
Carbonic acid	0·592
Estimation of the Nitrogen.	
Matter	0·359
Nitrogen at 0° and 0^{m} 76′ . . .	0·022
Hundredths.	
Carbon	56·05
Hydrogen	7·55
Nitrogen	6·12
Oxygen	30·28
	100·00

This residue, which, with the microscope, presents the appearance of the external membranes of the pollen, still contains a very considerable quantity of nitrogen, and is far from lignine.

The reagents appear likewise to demonstrate that the external membranes of the pollen are not formed of cellulose; they resist the action even of concentrated sulphuric acid, which colors them black, but does not dissolve them; they are remarkable for their great fixity.

We wished to determine the composition of the nitrogenous substance, which dissolves in alkalies, and which acids precipitate from the solution; we give here the results of our analysis, which apply to a substance which potassa in solution would injure.

Matter	0·305
Carbonic acid	0·560
Water	0·200
Estimation of the Nitrogen.	
Matter	0·305
Nitrogen at 0° and at 0^{m} 76′ . .	0·0384
Hundredths.	
Carbon	50·06
Hydrogen	7·28
Nitrogen	12·46
Oxygen	30·20
	100·00

We cannot look upon this substance as absolutely pure, but still we see that its composition resembles the albuminous bodies.

After having determined the composition of the pollen of lilies, it was interesting to examine other pollens analytically, and to see whether they all presented the same chemical composition.

For this purpose we analysed the pollen of two species of pines (*pinus mughus* and *pinus austriaca*), and two species of *typha latifolia* and *typha angustifolia*. These investigations are not yet complete; but from our first observations it seems that these pollens, which in form are quite different from the pollen of lilies, have the same che-

mical composition. By the methods described in this note we have obtained from them oil, starch, dextrine, glucose, albumen, a nitrogenous substance soluble in potassa, and membranes presenting the chemical character of those covering the pollen of the lily.

We think, therefore, that the immediate principles which compose the greater part of the pollen of the lily may be found in other pollens.

Thus, our first researches on pollen lead us to a result which will probably be interesting to botanists,—that there exists a striking analogy between the composition of pollen and that of an oleaginous seed. In fact, we find in pollen, as in grain, more or less nitrogenous membranes, starch, a species of diastase, a great quantity of a fatty oil, and albuminous substances.

Journal de Pharmacie et de Chimie, March, 1854.

ANALYTICAL CHEMISTRY.

On the ACTION *which* ZINC *and* IRON *exert on the* SALTS *of* SESQUI-OXIDE *of* CHROME. By M. HENRI LOEWEL.

(Concluded from page 420.)

III.—Action of Iron in solutions of Chrome Alum, and on those of Sesquichloride of Chrome.—7.—I formed solutions of alum and of sesquichloride of alum, similar to those of the experiments §1 and §2, in large glass tubes of from 15 to 18 millimetres diameter internally, about 2 or 3 decimetres in length, and closed at one end. I introduced into each a well scoured iron rod, from 5 to 6 millimetres in diameter, and long enough to come a little above the column of liquid contained in the tubes; and I immediately closed the upper openings of them with good corks containing bent glass tubes, so as to collect the gas disengaged.

After a short time, small bubbles of hydrogen gas appeared on the surface of the iron rods; these bubbles afterwards detached themselves and passed into the bell glasses which I had prepared to receive them. This disengagement of hydrogen continued slowly and very regularly; the solutions remained green, and deposited nothing. After a certain time (15 to 30 days, according as the heat of the weather hastened the reaction), they became a green gelatinous mass. The hydrogen gas, which was still formed, raising and swelling this gelatinous mass, it was then necessary to stop the operation and uncork the tubes.

In these experiments the action of the iron on the sulphate of sesquioxide of chrome contained in the alum is confined to removing the acid from it, becoming oxidised at the expense of the water whose hydrogen is disengaged. Iron acts in the same manner on sesquioxide of chrome in solution which, in these circumstances, behaves exactly like hydrochlorate of sesquioxide of chrome. The volume of hydrogen gas disengaged until the solution becomes a gelatinous mass

was always, within a few cubic centimetres, equal to what would be disengaged, while combining with iron by 2 of the 3 equivalents of sulphuric or hydrochloric acid contained in the chromic salts dissolved. The green gelatinous masses are hydrated basic salts of sesquioxide of chrome, which the iron has expelled from the solution, by removing a great part of their acid to form sulphate of protoxide and protochloride of iron.

I have made a series of experiments with more or less concentrated solutions, to see whether during the reaction there were any production of salts with a base of protoxide of chrome. For this purpose I stopped the operations when the reactions were more or less advanced, at the end of two, four, eight, ten, twelve, fifteen days, or more, up to the time when the solution began to turn into gelatinous masses. On uncorking the tubes at these various times, and immediately pouring the liquors which they contained on violet sesquichloride of chrome, their contact never caused the rapid solution of this salt. Now, it is well known that this solution would infallibly have taken place instantaneously if even a trace of sulphate of protoxide or protochloride of chrome remained in the liquors.

These results prove that iron is not sufficiently electro-positive to reduce the salts of sesquioxide of chrome to the state of salts of protoxide, as is done by zinc. Its action is limited, as I have said, to removing from these salts a portion of their acid, and expelling them from solution to a state of gelatinous basic salts, as would be done by an alkali when added by degrees and in small portions.

The results are the same when iron is made to act on solutions of chromic salts previously rendered very acid.

IV.—Action of Tin on Solutions of Sesquichloride of Chrome.—8. I said §3, that the blue solution of protochloride of chrome poured into a solution of protochloride of tin, immediately produces a precipitate of pulverulent metallic tin, by removing the chlorine in passing to the state of sesquichloride of chrome. It is, nevertheless, not exactly so, at least in the circumstances which I am about to describe.

I put some tin in grains into a phial, on to which I poured a solution of green crystallised sesquichloride of chrome which did not contain an excess of acid. After closing the phial with a cork furnished with a bent glass tube to collect the gas, I heated it and boiled the solution for ten or twelve minutes. No hydrogen was disengaged, and the solution remained green. Thinking that no reaction had taken place between the tin and the chromic salt, I removed the phial from the lamp, uncorked it, and poured the liquor into a capsule. I was then astonished to see that the liquor was rendered sparkling by a multitude of small metallic plates which had formed in it. This was metallic tin that then deposited; now, this tin could only have been dissolved by the removal of the chlorine from the sesquichloride of chrome.

For the purpose of studying this reaction better, and avoiding all contact with the air, I poured into glass tubes, closed at one end, more or less dilute solutions of pure sesquichloride of chrome; I sealed these

tubes hermetically, after having placed a piece of tin in each, and I plunged them into boiling water for fifteen or twenty minutes. After having removed them, I saw that on cooling, the liquors deposited metallic tin in powder, which was in a great measure redissolved when I plunged them into boiling water and was precipitated again on cooling.

I left some of these tubes for some hours in boiling water, the solutions never assumed the blue color of protochloride of chrome, and it did not appear to me that the quantity of tin in powder which they deposited on cooling was much more considerable than when the tubes were only exposed for twenty minutes to a temperature of 212° F.

It results from the experiments which I have made that in proportion to the augmentation of the temperature, and especially when it rises to 212° F., tin removes the chlorine from the sesquichloride of chrome; but it appears that, when a certain quantity of protochloride of chrome and protochloride of tin are found in the solution, the reaction ceases, although the tin is still in the presence of a great quantity of undecomposed sesquichloride of chrome. Then, when the temperature is lowered, the affinities change; the protochloride of chrome formed removes the chrome again from the protochloride of tin, passing again into the state of sesquichloride of chrome, and metallic tin is precipitated in a pulverulent form. This fact appeared to me of sufficient interest to deserve mention.*

ACETATE OF PROTOXIDE OF CHROME.

9.—Having spoken to M. Fremy of the facts mentioned in this note, that skilful chemist advised me to try whether, by means of the blue solution §1 and §2, acetate of protoxide of chrome might not be prepared. Hitherto that salt has been obtained by mixing solutions of acetate of soda and protochloride of chrome, prepared by passing a current of very dry hydrogen gas over violet sesquichloride of chrome at a temperature of nearly 572° F. The preparation of any considerable quantity of those two anhydrous chlorides of chrome, is, as is well known, tedious, expensive, and requires great care, whereas that of the blue liquors, by means of zinc, would be very simple, easy, and inexpensive, especially by employing the solutions of the chromic salts of which I shall speak §10.

To carry out M. Fremy's idea I made a very dilute boiling solution of acetate of soda; I poured it into two phials with ground stoppers, which I nearly filled; I added to the one some of the blue liquor §1, and to the other some of the blue liquor §2, and corked them immediately. The liquors in the two phials became red and opaque. The next day there was at the bottom of each bottle a pulverulent deposit covered with small granular crystals, the whole of a pomegranate red; the supernatant liquors, which were very limpid, were likewise slightly red. On uncorking the bottles and pouring these liquors into glasses, they became violet from the contact with the air.

* This fact is analogous to the solution of silver by the water of boiling sulphate of peroxide of iron and the precipitation of metallic silver by cooling.

The small red crystals possessed all the properties which M. Peligot has mentioned as appertaining to the acetate of protoxide of chrome. They were but little soluble in unaërated water, but on contact with the air, they very quickly assumed a violet color externally, by absorbing oxygen, and passing to the state of acetate of sesquioxide, which is very soluble in water.

The sulphate and chloride of zinc, the same as the salts of potassa, which are found in the blue liquors employed, having no action on acetate of soda, remain in solution, whereas the red acetate of chrome is deposited; the latter may be cleared from it by washing.

From this experiment, although imperfect and incomplete, I am convinced that these blue liquors obtained by means of zinc might serve to prepare the salt in question, taking, however, all the precautions to avoid the contact of air which are recommended by M. Peligot in his beautiful investigations on chrome.

10.—In the experiments related §1 and §2, I used chrome alum and green sesquioxide ($Cr^2Cl^3 + 12HO$), because, with these pure crystallised salts I could better study and appreciate the phenomena produced in the action of zinc on their solutions. However, these phenomena are the same, when we use instead of alum slightly acid simple sulphate of sesquioxide of chrome, obtained by dissolving, for example, 5 parts of dry greyish-green hydrate of sesquioxide in about 7 or 8 parts of monohydrated sulphuric acid diluted with four or five times its weight of water.

They are likewise the same if, instead of green crystallised sesquichloride of chrome, whose preparation is lengthy and requires great care, we use a solution of greyish-green hydrate of sesquioxide of chrome in about three times its weight of fuming hydrochloric acid, diluted with three or four times its weight of water.

To avoid even the preparation of hydrate of chrome, 50 grammes of bichromate of potassa may be treated with 150 or 160 grammes of fuming hydrochloric acid, diluted with 500 grammes of water, to which has been added 20 to 25 grammes of alcohol; when the reaction is finished, the liquor is evaporated to a syrupy consistence, to remove the excess of alcohol, acetic acid, and aldehyde which are formed. This liquor, when very concentrated, deposits in the state of chloride, the greater part of the potassa contained in the bichromate. Then diluting the green syrupy mother liquor with a suitable quantity of water, a slightly acid solution of sesquichloride of chrome is produced, retaining a certain quantity of chloride of potassium, which will answer perfectly for making the experiment §2.

Annales de Chimie et de Physique, January, 1854.

On the direct Analysis *of* Hydraulic Limestone *and* Cements.

By M. H. Sainte-Claire Deville.

I have had the honor of laying before the Academy some reforms, which I have endeavored to introduce into the general methods of che-

mical analysis, by applying to it some fundamental principles, which appeared to me to have been hitherto misunderstood. In an already published memoir I shewed the advantages of processes of calcination at low temperatures, to avoid the necessity of precipitation as a means of separation, and to allow of the exclusive employment of gaseous or volatile reagents. Nitric acid and nitrate of ammonia are the principal agents used in these new methods, to which I have given the name of method *by the mean way*. I will shew further on, that they are applicable in the analysis of metallic matters, properly so called; just now I only wish to make known to the Academy an application of nitrate of ammonia, in the search for the immediate principles constituting hydraulic limestone and cements.

The problem which I gave myself to work is composed of two distinct parts; it was necessary to extract the carbonates of lime and magnesia, without altering in any way the argil and other matters which accompany them; to extract from a cement the free calcium contained in it, and to isolate the hydraulising matter, the alumino-calcareous silicate, which acids and a large quantity of water alter so quickly, as is the case with all silicates with an excess of base. All these analyses may be performed with nitrate of ammonia alone. This salt, with the intermediation of water, dissolves the carbonate of lime at a boiling temperature; carbonate of ammonia is then disengaged, and the other matters remaining intact, they may be weighed. These argilaceous matters are to be examined with care and the composition determined by following the processes which I have already given. I would merely recommend that the iron should be removed first, by heating the matter in a current of gaseous hydrochloric acid.

The search for free calcium in cements is executed by putting them in contact with nitrate of ammonia without heat, in an apparatus of which I gave a description in the *Annales de Chimie et de Physique*, and which, by rendering useful a very ingenious idea of M. Schlœsing and M. Peligot's processes, enables us here to substitute a simple estimation of sulphuric acid for the direct determination of the free calcium in cements.

I shall conclude with an observation which has been of the first consequence in the application of my methods. The greyish blue calcareous stone employed in the manufacture of cement at Vassy, contains, besides bitumen, a proportion of pyrites, which exceeds 6 per cent. of the weight of the stone. This may be proved by heating to redness in the air a fragment of the stone. The bitumen soon burns, a very powerful odor of sulphurous acid is developed; the sulphuret of carbon removes no part of the sulphur. M. Ebelmen proved the presence of pyrites in the calcareous stones of the jura: he even generalised this fact, and thought that all greyish-blue calcareous stones contained pyrites. Now this is generally the color of the stones used for hydraulic cement, we must consequently admit that they generally contain pyrites.

I should naturally have supposed that the cement would contain plaster; I proved this with the cement of Vassy, which contains more

than 5 per cent., and with the Roman cement from Pouilly, which contains 3 per cent.

It is certain that there is much interest, in a practical point of view, in the search for the immediate principles which compose calcareous stones and cements, the importance of such accidental matters as pyrites and plaster, and the influence which they may exert on the preservation or alteration of the cements in various liquids. But this subject is one of those which M. Paul Michelot and myself are treating of in full in a work on this subject, the principal results of which we shall shortly lay before the Academy.

Comptes Rendus, No. 26, December 26th, 1853.

PHYSICS.

THERMO-CHEMICAL *Investigations Concerning* COMBINATIONS *Formed in* MULTIPLE PROPORTIONS. By M. P. A. FAVRE.

(*Continued from page* 426.)

OXYGENOUS COMPOUNDS OF SULPHUR.

THE combustion of sulphur to the state of sulphurous acid has already been the subject of numerous experiments published by M. Silbermann and myself. We have, moreover, determined the heat of solution of sulphurous acid in water.

I relied on these numbers for estimating the heat disengaged by the combustion of sulphur to the state of sulphurous acid; for that purpose I determined, as will be seen further on, the heat of oxidation which accompanied the passage of sulphurous acid to the state of sulphuric acid. To control the results of these two successive combustions, I burned sulphur directly to the state of sulphuric acid with the aid of hypochlorous acid.

The employment of hypochlorous acid in the transformation of the alkaline hyposulphites into sulphates, led me to the calorific equivalent of hyposulphurous acid.

I will also give an account of the remarkable peculiarities which I observed in the decomposition of the hyposulphites by the acid, an experiment undertaken with the view of determining the heat which accompanies the combination of sulphur with sulphurous acid to constitute a hyposulphite. The variable phenomena observed in this case are owing to the various states which the sulphur separated in this reaction may affect.

The mean of the numbers obtained by the combustion of native sulphur to the state of gaseous sulphurous acid (in the water calorimeter) is

2220 units for 1 grm.

35520 units for 16 grms. or 1 equivalent.

Now, 1 equivalent of sulphurous acid in dissolving in water disengages

3853 units of heat;

we have, therefore, for 1 calorific equivalent of sulphurous acid formed in the state of solution,

39373 units of heat.

Transformation of Sulphurous Acid in solution into Sulphuric Acid.—I relied for this on the reaction of chlorine on sulphurous acid in solution.

$$SO^2 + x\,Aq + HO + Cl = ClH + SO^2 + x\,Aq.$$

The chlorine from the gasometer arrived dried in the calorimetric eprouvette by a tube drawn out dipping to the bottom of the liquid. The eprouvette containing the solution of sulphurous acid was weighed before and after the reaction. The increase of weight indicated the weight of chlorine which had reacted for converting a portion only of the sulphurous acid into sulphuric acid.

A gramme of chlorine, in reacting on a solution of sulphurous acid, gave

I.	942·4
II.	948·8
Mean.	945·6 units of heat.

It follows from this, that the heat *disengaged* by the transformation of 1 equivalent of sulphurous acid into sulphuric acid is 27839 units, since each equivalent of chlorine corresponds to an equivalent of sulphurous acid.

The above value is deduced from the equation,—

$$R = x + A - D,$$

in which A is the calorific equivalent of dissolved hydrochloric acid (40192), and D the calorific equivalent of water (34462).

Now, 39373 being the calorific equivalent of dissolved sulphurous acid, the sum

39373 + 27839

will be equal to 67212 units of heat,

which are *disengaged* by the transformation of natural sulphur into dilute sulphuric acid.

Oxidation of Sulphur with Hypochlorous Acid.—I next proposed to oxidise natural sulphur directly with hypochlorous acid, in order to control the foregoing number; unfortunately the attack was made too slowly (even when the sulphur was pulverised) for the calorimetric experiment to give satisfactory results.

I succeeded better with flower of sulphur. But we know, from M. Ch. Deville's experiments, that this substance is not homogeneous; consequently, I have thought proper only to operate on that portion of flower of sulphur which is insoluble in sulphuret of carbon. I employed a sample sent to me by M. Ch. Deville, and prepared by himself.

The following are the results deduced from experiments made on about 0·1 grm.

I.	5041·6
II.	5375·9
Mean	5388·7 units of heat.

We have the equation

$$S + 3ClO - x\ Aq. = SO^3 + x\ Aq + 3Cl.$$

and

$$R = x - D,$$

in which D represents the heat disengaged by the decomposition of 3 equivalents of hypochlorous acid.

In making the calculation, we find that 16 grms. of sulphur, or 1 equivalent, thus transformed into dilute sulphuric acid, disengage

64110 units of heat.

Now, from the successive combustions starting from the native sulphur, we have

67212 units.

It is impossible to attribute this difference solely to errors of experiment, although the resistance *a se mouiller*, which M. Ch. Deville's sulphur presents, renders possible the expulsion of a little chloride of sulphur from the calorimeter.

Decomposition of Hyposulphurous Acid into Sulphur and Sulphurous Acid.—Relying on the considerations already expressed with regard to nitrous acid, I will admit that hyposulphurous and sulphuric acids disengage the same quantity of heat when they combine with the same base in order to give rise to a soluble salt. This being done, when hyposulphurous acid is displaced from its combination with soda, for example, by means of dilute sulphuric acid, the calorific effect observed will be attributed to the single phenomenon of decomposition of hyposulphurous acid into sulphurous acid which remains in solution, and into sulphur which separates sometimes in the *oily state*, and sometimes in the pulverulent state, according to the degree of concentration of the solution.*

1 grm. of crystallised hyposulphite of soda ($S^2O^2NaO, 5HO$) previously dissolved, then decomposed with dilute sulphuric acid, and giving rise to *oily sulphur*, absorbs,—

I.	5·5	units of heat.
II.	6·63	
III.	8·84	

and for 1 equivalent of crystallised hyposulphite of soda, or 124 grms., we have

I.	688	units of heat *absorbed*.
II.	882	
III.	1097	

It must be remarked that when the solution of hyposulphite is very

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* This peculiar state of sulphur had already been noticed several years ago by M. Stas. This sulphur does not very soon resume the solid state; it is probable, therefore, that the numbers obtained afterwards may differ from those which would correspond to a separation of the sulphur in the native state, but of unknown quantity; for we cannot be sure of making this correction exactly, even by causing the latent heat of fusion of sulphur to intervene.

I may here mention that M. Schrœlter has noticed a similar peculiarity as regards phosphorus, which, in certain conditions, may remain liquid for a very long time, at a low temperature, notwithstanding agitation.

dilute, the sulphur is precipitated in the *pulverulent state* of a yellowish white, which at the end of some hours acquires a slightly reddish tint. In this case, the reaction instead of being accompanied by an *absorption*, gives rise to a *disengagement* of heat.*

1 grm. of crystallised hyposulphite of soda, dissolved, then decomposed by sulphuric acid in conditions in which the sulphur is precipitated in the *pulverulent state*, disengages :—

$$\left.\begin{matrix}17{\cdot}44\\21{\cdot}83\end{matrix}\right\}\text{ units of heat.}$$

And for 1 equivalent we have

$$\left.\begin{matrix}21{\cdot}62\\27{\cdot}07\end{matrix}\right\}\text{ units of heat.}$$

It may be admitted that the sum of the maximum numbers

$$2707 + 1097 = 3804$$

represents the *heat disengaged by the passage of an equivalent of liquid sulphur into the state of pulverulent sulphur*. This number should, indeed, be regarded as a minimum, seeing that we cannot affirm that there is not precipitated in all cases a mixture of two varieties of sulphur, one being in great excess with regard to the other.

Admitting that pulverulent sulphur which is precipitated in this reaction is assimilable to ordinary sulphur, it would follow that the combination of sulphur with 1 equivalent of sulphurous acid in solution to constitute hyposulphurous acid in solution would absorb, at least, 2707 units of heat, whilst oily sulphur would disengage, in the same circumstances, at least 1097 units of heat.

Peroxidation of Hyposulphurous Acid by Hypochlorous Acid.—By making hypochlorous acid in solution act on a solution of hyposulphite of soda, I obtained, for 1 grm. of salt

I.	5401·6
II.	5375·9
Mean	5388·7 units of heat disengaged.

The reaction is as follows :—

$$S^2O^2 + x\ Aq + 4\,ClO = 2SO^3 + x\ Aq + 4Cl.$$

I call to mind, indeed, that I thought I could abstract the difference existing between the heat disengaged by the union of sulphuric acid with soda and that which hyposulphurous acid disengages in combining with the same base ; moreover, I will call to mind, that in the researches which I made conjointly with M. Silbermann, it was proved that there was no heat disengaged in presence of dilute sulphuric acid and of a solution of neutral sulphate of soda, *when nothing is precipitated.*

* I employed from 50 to 60 cubic centimetres of distilled water for 1 grm. of crystallised hyposulphite. In the preceding case, that is to say, for the precipitating of sulphur in the oily state, we employed only the quantity of water strictly necessary for maintaining in solution the sulphurous acid liberated.

Now, the reaction $R = x - D$,

R, representing the heat disengaged by the decomposition of 4 equivalents of hypochlorous acid.

x, expressing, moreover, the heat disengaged by the transformation of 1 equivalent of hyposulphurous acid into 2 equivalents of sulphuric acid.

In making the calculation, we find :—

$x = 93953$ units of heat,

which represent the heat disengaged by the transformation of an equivalent of hyposulphurous acid into 2 equivalents of sulphuric acid.

By subtracting this number from the double calorific equivalent of sulphuric acid 134420, we obtain 40467 units of heat, which represent the calorific effect due to the combination of 2 equivalents of native sulphur with 2 equivalents of oxygen, to give rise to 1 equivalent of hyposulphurous acid in solution. Half an equivalent, or (S + O), would give 20233 units of heat *disengaged*.

If we regard S^2O^2 as $= S + SO^2$,

and if we subtract from 93953, the heat disengaged by the peroxidation of S^2O^2, 27839, the number which corresponds to the transformation of SO^2 into SO^3 ; the difference

66114 units,

expresses the quantity of heat which the second equivalent of sulphur contained in S^2O^2 would disengage in being converted into sulphuric acid.

It must be borne in mind that the reaction which divides hyposulphurous acid into sulphurous acid and *oily sulphur*, gives rise to an *absorption* of 1097 units per equivalent of sulphur isolated, or

—1097 units of heat.

When sulphur is precipitated in the *pulverulent state*, the reaction gives rise to + 2707 units of heat.

If, then, we wish to deduce from the experiments relative to the oxidation of hyposulphurous acid, the number to be applied to the calorific equivalent of sulphurous acid formed by oily sulphur, or by the pulverulent sulphur of the hyposulphites to the fictitious number, 66114, applied to the sulphur (and which results from the calculation effected above) must be added 1097, or 2707 must be subtracted. We shall thus have :—

66114 + 1097 = 67211 units, heat of formation of sulphuric acid in solution setting out from the oily sulphur of the hyposulphites.*

66114 + 32707 = 63407 units, heat of formation of sulphuric acid in solution, setting out from the *pulverulent sulphur* of the hyposulphites.

The result 63407 units for the calorific equivalent of sulphuric acid setting out from the pulverulent sulphur of the hyposulphites, is in-

* It is certainly very singular to see the oily sulphur disengage by its direct combustion 67211 units of heat, that is to say, a number identical with that which results from the transformation of native sulphur into sulphuric acid deduced from two successive combustions, $S + O^2$, $SO^2 + O$.

This fortuitous coincidence is an additional reason for admitting that oily sulphur must contain pulverulent sulphur. The number 67211 is certainly a minimum.

teresting; for, according to MM. Fordos and Gelis, this sulphur, like that of the chlorides of sulphur, possesses, like M. Ch. Deville's sulphur, the property of being insoluble in sulphuret of carbon.

Now, M. Deville's sulphur, arising from flowers of sulphur exhausted with sulphuret of carbon, disengaged

64110 units of heat.

There is therefore a great analogy, if not identity, of molecular constitution between these two varieties of sulphur.

In conclusion, the following are the results which apply to the oxidation of sulphur :—

Sulphur.	One equivalent in being converted into SO^2 disengages	Observations.
Native sulphur . . .	67212 units.	Number deduced from $S + O^2$ and from SO^2 in solution oxidised by chlorine.
Sulphur insoluble in sulphuret of carbon.	64110 „	Direct combination by ClO.
Pulverulent sulphur of the hyposulphites .	63497 „	Number deduced from the combustion of S^2O^2 by ClO and from the reaction of the acids on S^2O^2, NaO, 5HO.
Oily sulphur of the hyposulphites	67211 „	*id.* *id.* *id.*

The following is a table of the heats disengaged by the combination of 1 equivalent of sulphur or 16 grammes with 1, 2, or 3 equivalents of oxygen, (the combination remaining dissolved) :—

Oxygenous acid of sulphur.	Number of units of heat disengaged by the combination containing 1 equivalent of sulphur.	Observations.
S + O	20233	Number deduced from the combustion of S^2O^2 by ClO $= \frac{40467}{2}$
$S + O^2$	39373	Direct experiment (Favre and Silbermann).
$S + O^3$	67212	Successive combustions of native sulphur.
id.	64110	Direct combustion of sulphur insoluble in CS^2 by ClO.

Thus the quantities of heat disengaged in order to form successively $S + O$, $S + O^2$, $S + O^3$, (the compound remaining dissolved) are respectively,—

:: 1 : 1·95 : 332

when native sulphur is made to intervene.

It is evident that these numbers are among themselves nearly as the equivalents of oxygen, contained respectively in the 3 acids, that is to say,—

:: 1 : 2 : 3.

If we take as unity the heat disengaged by the formation of SO^2 in solution, that which corresponds to the formation of sulphuric acid will be 1·71, always setting out from native sulphur.

If we designate by E and E' the calorific equivalents of SO^2 and SO^3, we shall have,—

E : E' :: 1 : 1·71.

OXYGENOUS COMPOUNDS OF SELENIUM.

Selenic Acid.—1 grm. of selenium treated with hypochlorous acid in solution, disengages in passing to the state of selenic acid,—

I.	1470·4
II.	1453·2
Mean	1461·8 units of heat.

1 equivalent of selenium would give,—

58470 units of heat.

The reaction is formulised thus,—

$$Se + ClO + x\,Aq = SeO^3 + 3Cl + x\,Aq.$$

In the equation,—

$$R = x - D,$$

D = —22110 three times the calorific equivalent of hypochlorous acid,

Whence $x = 58470 - 22110 = 36360$.

The calorific equivalent of selenic acid is then represented by 36360 units of heat.

Selenious Acid.—1 grm. of selenious acid in solution treated with hypochlorous acid disengages, in being converted into selenic acid,—

I.	372·9
II.	366·7
Mean	369·8 units of heat.

Then 1 equivalent of selenious acid or 55·5 gr. disengages,—

20524 units of heat.

The reaction is thus formulised :—

$$SeO^2 + ClO + x\,Aq = SeO^3 + Cl + x\,Aq.$$

In the equation,—

$$R = x - D,$$

D = —7370 the calorific equivalent of ClO dissolved.

Whence

$$x = 20524 - 7370 = 13154 \text{ units of heat,}$$

which expres the heat disengaged by the peroxidation of selenious acid.

By subtracting this latter quantity 13154 from 36360, the calorific equivalent of selenic acid, we have,—

23206 units of heat disengaged,

to express the calorific equivalent of selenious acid in solution.

These results are, in my opinion, interesting. Indeed, the quantity of heat disengaged by selenium in being oxidised is much less than that which accompanies the formation of the corresponding oxygenous compounds of sulphur, which is in accordance with the lesser affinity of selenium for oxygen. It is known, indeed, that selenious acid readily yields its oxygen to sulphurous acid, abandoning its selenium.

The relation between the calorific equivalents of selenious and selenic acids differs little from the relation found for the calorific equivalents of sulphurous and sulphuric acids.

Indeed

	Calorific equiv.		Ratio.		Calorific equiv.		Ratio.
SO^2	dissolved	39374	1	SeO^2	dissolved	23206	1
SO^2 (native sulp.)	„	67212	1·71	SeO^2	„	36360	1·56
SO^2 (S in sol. in CS^2)	„	64110	1·63				

OXYGENOUS COMPOUNDS OF CARBON.

Combustion of Oxalic Acid with Hypochlorous Acid.—I thought I might turn to account the energetic oxidising action which hypochlorous acid exerts at the ordinary temperature, in order to determine the heat which results from the transformation of oxalic acid into carbonic acid. Considering oxalic acid as an intermediate term between oxide of carbon and carbonic acid, it might be interesting to know the calorific equivalent of the compound CO_2^2 or C^2O^3 in order to compare the result with the calorific equivalents of oxide of carbon and carbonic acid.

1 grm. of oxalic acid in solution treated with hypochlorous acid in solution, disengages,—

I.	596·24
II.	594·56
Mean	595·40 units.

Consequently, 1 equivalent of this body, or 63 grms., would have disengaged 37510 units of heat.

This number expresses the heat disengaged by the peroxidation of oxalic acid in solution, under the influence of hypochlorous acid, in the reaction which is thus formulised :—

$$C^2O^3, 3HO + ClO + x\,Aq = 2CO^2 + Cl + (x+3)\,Aq.$$

In the equation $R = x—D$

We have $D = 7370$ the calorific equivalent of hypochlorous acid.

Whence $x = 37510—7370 = 30140$ units of heat,

which express the calorific effect produced by the transformation of 1 equivalent of oxalic acid into carbonic acid.

The number 30140 subtracted from 96960, double the equivalent of carbonic acid, gives 66820 units of heat disengaged, which express the calorific equivalent of dissolved oxalic acid.

Deducting the water of the oxalic acid, and regarding this acid as *carbonous* acid (which I am far from admitting), we should have for the series of oxidation of carbon the following calorific numbers:—

			Units of heat.	Ratio.
Heat disengaged by 1 equivalent of carbon with	O	to form CO	14838	1·00
	$O^{\frac{1}{2}}$	„ $CO^{\frac{1}{2}}$	33410	
	O^2	„ CO^2	48480	

(To be concluded in our next.)

INVESTIGATIONS *as to the* QUANTITY *of* AMMONIA *contained in* RAIN WATER, *collected far from* CITIES, *and in* PARIS. By M. BOUSSINGAULT.

IN the course of his analyses of rain water, for the purpose of estimating the ammonia contained in it, M. Boussingault observed that the proportion of this alkali is far from being the same at the beginning and end of each fall of rain. Thus, during a storm on the 5th July, the water which was first collected contained per litre 0·5 milligramme of ammonia, whilst that which he examined afterwards gave only 0·06 mil. This fact gave rise to a new series of experiments for the purpose of ascertaining the progress of the diminution of the ammonia in the water of a fall of rain during different periods of its duration.

To obtain a sufficient quantity with ease, M. Boussingault used a very fine cloth, fixed on stakes driven into the ground. The cloth, slightly depressed in the middle, was about 1·5 metre above some grass land; under the lowest part was a tin funnel 80 centimetres in diameter, which was placed in a bottle. The cloth presented a horizontal surface of 4·922 metres.

From the 26th May to the 16th November, M. Boussingault measured at Liebfrauenberg, seventy-five falls of rain, on which he executed a hundred and thirty-seven determinations of ammonia. We shall here only indicate some of the results mentioned in his memoir.

On the 26th August the rain began at half-past four in the afternoon; it had not rained for two days; at six o'clock there was thunder; at a quarter past six the rain ceased; 6·85 litres of rain water had been collected.

Rain in millimetres.	Water received.	Ammonia. in a litre of water.	Ammonia. in the water collected.	
	litres.	millig.	millig.	
0·25	1·25	3·75	4·69	1st time.
0·20	1·00	1·91	1·91	2nd do.
0·20	1·00	1·33	1·33	3rd do.
0·20	1·00	0·61	0·61	4th do.
0·20	1·00	0·33	0·33	5th do.
0·33	1·60	0·64	1·02	6th do.
1·38	6·85		9·89	

Ammonia in a litre of water : mean 1·47 milligramme.

In other circumstances, the mean of ammonia was from 0·30 mil. to 0·17. But the proportion has always diminished rapidly from the beginning of each fall of rain until the end ; has been less when rain has been abundant ; and greater, on the contrary, when the rains have succeeded a long drought.

M. Boussingault has also sought for ammonia in dew and fogs. The following are his results as regards dew :—

	Water in millim.	Water collected, lit.	Ammonia per lit. millig.
Night of the 18th to 19th Aug.	0·25	1·25	3·14
„ 9th to 10th Sept.	0·16	0· 8	6·20
„ 11th to 12th Sept.	0·18	0· 9	6·20
„ 21st to 22nd Sept.	0.20	1· 0	6·20
After a rainy day from the 24th to 25th Sept.	0·33	1· 6	1·02
Night of the 27th to 28th Sept.	0·18	0· 9	6·20

In fogs he found :—

	Water in millimetres.	Water collected.	Ammonia per litre millig.
On the 26th to 27th Oct.	0·35	1·7	5·28 thick fog.
„ 27th to 28th Oct.	0·07	0·4	7·21 fog at night.
„ 4th Nov.	0·26	1·3	5·13 fog in the day.
„ 6th to 7th	0·33	1·6	2·56 fog at night.
„ 7th	0·33	1·6	3·00 „ „
„ 8th	0·24	1·2	4·56 fog in morning.
„ 14th to 16th Nov.	0·50	2·55	49·71 from morning of 14th to night of 16th.

The water given by the fog of the 14th to 16th instantly restored the blue color to reddened litmus paper.

On grouping these rains in series corresponding to the measures of the udometer, in other words, to the quantity of water furnished by each fall of rain, we can form an opinion of the proportion in which the ammonia augments as the rain is less abundant ; thus we have,—

	Millig.
From 20 to 31 millimetres ammonia per litre	0·41
„ 15 „ 20	0·40
„ 10 „ 15	0·45
„ 5 „ 10	0·45
„ 1 „ 5	0·70
„ 0·5 „ 1	1·21
„ 0 „ 0·5	3·11

These facts are explained by the properties of volatility and solubility possessed by carbonate of ammonia, which certainly furnishes the greater part of the ammonia contained in rain. The air contains it in the state of vapor emitted by the ground when sufficiently moist ; the rain dissolves it in passing through the air, and is more charged with it at first than at last ; and as soon as the rain ceases, the tendency of this salt is to pass again into the atmosphere, to be again dissolved by the first rain that falls.

As for the nitrate of ammonia, it is also found in meteoric waters; but it is necessary to establish a distinction as to its origin.

The beautiful experiments of Cavendish taught us that, under the influence of electricity, nitric acid and ammonia are produced in storm clouds, and this circumstance explains the presence of this salt in storm waters; but, as it has been found in rain collected at all seasons of the year, it is necessary to attribute another origin to it; and, as it is fixed, it is evident that it must, like common salt, the iodides, and generally speaking all the soluble and not volatile substances found in meteoric waters, form a part of the dust which the air always holds in suspension, and which is perceptible to the eye when a ray of light enters an otherwise obscure place, and which Bergman has perfectly characterised when he called it the dirt of the atmosphere. Meteoric waters remove this dust, dissolving the soluble matters, amongst which are some fixed ammoniacal salts, especially the nitrate, which they dissolve at the same time as the vapors of carbonate of ammonia and the carbonic acid gas which are diffused through the air.

The nitrate of ammonia is then constantly poured upon the soil by the meteoric waters, and is soon transformed into carbonate of ammonia by the action of calcareous rocks or their detritus; it thus becomes one of the most efficient agents in vegetation, aiding in the elaboration of the nitrogenous principles of plants.

I took two opportunities which presented themselves, to repeat last month, at Paris, the observations which I had made in the country on the ammonia contained in rain water and fogs.

On a terrace between two gardens, near the Place Royale, I placed a metal funnel, of $\frac{1}{2}$ a square metre in diameter, at its opening.

On the 3rd January, about 9 at night, a very heavy rain fell. I collected successively 5 volumes of water, and estimated the ammonia in them.

Water collected.	Rain in millimetres.	Ammonia in the water collected.	Amonia per litre.
litres.	millig.	millig.	millig.
0·67	1·34	3·37	5·03 1st vol.
0·85	1·70	3·73	4·39 2nd vol.
1·10	2·20	3·30	3·00 3rd vol.
6·69	1·38	1·52	2·20 4th vol.
0·76	1·52	0·60	0·79 5th vol.
4·07	8·14	12·52	

Mean, in 1 litre of rain, 3·08 millig. of ammonia.

This proportion approaches that proved by M. Barral in the rain waters collected in the udometers at the Observatory, but in rains as abundant as that which fell in Paris, the quantity of ammonia appears to be much less in the waters collected far from the great centres of population. Thus, in amounts of water comparable to that measured

on the 3rd of January, I found, in my observations at Liebfrauenberg, the following numbers :—

Dates.	Water collected in the udometer.	Rain in millimeters.	Ammonia in the water collected.
	litres	milligramme.	milligramme.
1853, 2nd June	44·3	9·00	11·08
„ 5th June	34·4	7·00	17·06
„ 30th June	42·3	8·60	18·19
„ 2nd Sept.	50·6	10·27	21·71
„ 25th Sept.	40·1	8·14	9·63
„ 9th Oct.	38·5	7·81	20·40
„ 14th Oct.	46·65	9·47	11·66
	296·85		109·73
Mean, in a litre of water, 0·34 milligrammes of ammonia.			

One difference in this respect is due, probably, to the matters which float constantly in the air of a great city. The aspect of the water received during the different phases of the rain on the 3rd of January evidently indicate the presence of these matters. In fact, the water first taken held a blackish substance in suspension, in which, independently of some flakes of soot, was found a very fine powder of a siliceous nature. The second and third were likewise turbid. The fourth and fifth gave no deposit.

WATER DEPOSITED BY FOGS.

I obtained some water deposited by the fogs which appeared in Paris in the month of January. The fog on the 23rd was so thick that candles were required at 10 o'clock in the morning.

The water collected was limpid, but it had an amber color proceeding most probably from the smoke diffused through the atmosphere of Paris ; at least, I can state positively that the waters of fogs collected at Liebfrauenberg were always limpid and colorless.

The water from the fogs of Paris, obtained in the course of the month of January to the 23rd inclusive, was especially remarkable for the quantity of alkali which it contained. In one litre 136 milligrammes of ammonia were found ; now the most ammoniacal meteoric water which I had examined in the country only contained 50 milligrammes per litre ; it was deposited by a fog which continued without interruption in the valley of the Rhine from the 14th to the 16th of November. 138 milligrammes of ammonia are equivalent to 64 centigrammes of bicarbonate, the state in which it is reasonable to suppose that it exists in the atmosphere, since it is found in the presence of an excess of carbonic acid. The fact of so great a proportion of a volatile ammoniacal salt existing in 1 litre of the water which

constitutes the vesicular vapor, will, perhaps, explain why, in certain circumstances, the fog of cities possesses a sufficiently penetrating odor to affect the organs of respiration to a painful degree.

Comptes Rendus, Nov. 23rd, 1853, and 6th of February, 1854.

On the Passiveness *of* Nickel *and* Cobalt.

By M. J. Nickles.

The curious property possessed by iron of becoming less oxidisable after having been in contact with fuming nitric acid, has exercised the sagacity of many chemists and physicists. Having been discovered by Keir, it has been examined in various ways by MM. Herschel, Faraday, Schœnbein, Becquerel, Buff, De la Rive, Andrews, Mousson, Millon, Beetz, and Rollmann. From the works of these learned men, it appears that iron may become passive, not only by contact with fuming nitric acid, but that it acquires this property when rendered blue in the lamp; and when, having been plunged into not fuming nitric acid, it is touched, whilst in that liquid, with a platinum wire.

The same effect is obtained when the iron is placed in communication with the positive pole of a voltaic battery.

In consequence of the modifications which it undergoes in these circumstances, iron does not precipitate sulphate of copper when it serves as the anode of a voltaic battery; the oxygen is disengaged round it without attacking it; it remains in contact with aqueous nitric acid without undergoing any alteration, but it resumes its activity when, having been removed from the acid, it is plunged into water.

These properties likewise belong, in a greater or less degree, to nickel and cobalt when drawn out into wire. These two metals are liable to become passive in the same manner as iron. A lamina of nickel formed by means of galvanism behaved in every respect like wire, which tends to prove that the pressure endured by the wire in being drawn out, and the greater density which it thus acquires have nothing to do with the negative properties which the metal acquires under the influence of fuming nitric acid.

The process by means of which this plate was formed is at once simple and expeditious; as it may be useful to persons who work in nickelisation, I will describe it in a few words.

The first matter which I employed was the double sulphate $SO^3NO + SO^3KO + 6HO$, so well known by its beautiful oblique rhomboïdal prisms, and which is formed in the treatment of the ore of nickel. This double sulphate is to be put into a capsule with water; placed on a fire, and without waiting until the whole is dissolved, the two poles of a battery are plunged into it; the negative pole, intended to receive the deposit, is terminated by a plate of platinum, very slightly greased, so as to facilitate the separation of the layer of nickel to be produced. The deposition takes place quickly when the solution

is kept upon the fire: the plate deposited is very consistent, elastic and perfectly homogeneous.

The wires of nickel and cobalt, which served for the experiments of which I speak, were chemically pure; they had been kindly sent to me by M. H. Sainte-Claire Deville, who had himself prepared and analysed them.

The following are the analytical results which M. Deville obtained with the nickel wire. 100 parts of the metal gave:—

Silicium	0·3
Copper	0·1
Nickel	99·6
	100·0

The nickel was, therefore, very pure; the equivalent which results from this analysis is 369·9, which differs very little from the number 369·03, adopted by Berzelius.

The passive state acquired by nickel and cobalt in the presence of fuming nitric acid is only of a short duration, but becomes permanent when, having been rendered blue either in the lamp or on a charcoal fire, they are plunged while still hot into this acid; from this time they behave in every respect like passive iron, except that they are less negative with ordinary nitric acid. However, they will communicate their passive condition to active iron when plunged into not fuming acid, and will stop the powerful action, of which it is the object, on the part of that acid.

Platinum is always negative with regard to these three metals thus rendered passive, and any one of these last will be negative with regard to the three active metals.

The inocuousness of passive iron in sulphate of copper, mentioned by M. Schœnbein, cannot be produced outside the circuit; in all my experiments the iron was quickly surrounded by metallic copper.

I at the same time examined the electro-chemical relations of iron, nickel, and cobalt in the active and passive states in contact with various acids, as well as with aqueous potassa. The negative character of passive iron is only really apparent in nitric acid; in the other liquids which I used in my experiments, positive electricity was carried to the iron instead of emanating from it. In aqueous potassa the relations were the same for the active and the passive metals, which would appear to indicate that the passiveness of these metals is destroyed by this alkali; it is, however, nothing. Indeed, when after immersion in potassa, they are replaced in contact with nitric acid of a density of 1·34, they resume the electro-chemical action suitable to their passive state and are not attacked by the acid.

The following series shew the respective relations of iron, nickel, and cobalt in these two states, beginning with the positive and ending with the negative metal; the experiments were made with the various liquids mentioned in the table:—

Liquid employed.	Active metals. +	Active metals. —	Passive metals. +	Passive metals. —
Fuming nitric acid . .			Co, Ni,	Fe
„ „ of 1·34 of density	Fe,	Co, Ni . .	Co, Ni,	Fe
SO^3 + HO	Co,	Fe, Ni . .	Ni, Co,	Fe
SO^3 + HO diluted with 9 parts water	Fe,	Ni, Co . .	Fe, Co.	Ni
Aqueous potassa . . .	Fe,	Ni, Co . .	Fe, Ni,	Co

Journal de Pharmacie et de Chimie, March, 1854.

INDUSTRIAL CHEMISTRY.

On some NEW *and* SIMPLE METHODS *for detecting* MANGANESE *in* NATURAL *and* ARTIFICIAL COMPOUNDS, *and obtaining its* COMBINATIONS *for economical or other uses.*

By EDMUND DAVY, Esq., F.R.S., M.R.I.A., &c.

IN this paper the growing importance of Manganese since its discovery, and its extensive distribution in nature are noticed. Manganese is chiefly found combined with oxygen, but its oxides are commonly mixed with those of iron, and though different methods of separating them have been recommended, yet no very simple or unobjectionable test for Manganese seems to be known. Two methods for detecting Manganese are recommended, viz.,—

1. The pure hydrated fixed alkalies, potassa, and soda, and especially potassa. 2. Sulphur.

With regard to the first method. Though the compound *Chameleon mineral*, made by strongly heating nitre or potassa and peroxide of Manganese together, has long been known, yet it appears hitherto to have escaped observation, that potassa seems to be a more delicate test for Manganese than any other known substance. The use of potassa in this way is simple and easy; it is employed in solution; equal weights of the alkali and water form a fluid well adapted for the purpose; different metals may be used in the form of slips on which to make the experiments, but a preference is given to silver foil, as it is less acted on by alkalies than platinum, and is more readily cleaned. A slip of such foil, about two or three inches in length and half an inch wide, answers well. Solids, to be examined for Manganese, are finely pulverised; fluids require no preparation; the smallest portion of ether is mixed with a drop or part of a drop of the alkali on the foil, and heated by a spirit-lamp (for many experiments a candle affords sufficient heat), when, on boiling the alkali to dryness, and raising the heat, the characteristic green manganate of potassa will appear on the foil. The delicacy of the alkali, as a test thus applied, will be obvious on using the most minute portions of Manganese ores in fine powder; and the author's son, Dr. E. W. Davy, readily detected Manganese in a single drop of a solution containing one grain of solid sulphate in ten thousand grains of water. The pre-

sence of other oxides do not appear to impair the efficacy of this test. A strong solution of hydrate of soda in water, used in a similar manner, affords an excellent test for Manganese, little inferior in delicacy to potassa, but the latter is shewn to be preferable.

Carbonate of soda has long been regarded as one of the most delicate tests for Manganese, especially if aided by a little nitrate or chlorate of potassa, but that carbonate is much inferior as a test for Manganese to potassa or soda, requiring a far higher temperature to form the manganate of soda, and the aid of oxidising substances, as nitre and chlorate of potassa, which are quite unnecessary with those alkalies. Borax, too, in point of delicacy, is not to be compared with the fixed alkalies as a test for Manganese.

The author is of opinion that the fixed alkalies in solution and silver foil will form a valuable addition to the agents employed by the mineralogist and chemist in the examination of minerals, oars, &c.

2. *Sulphur.*—If a little flowers of sulphur be mixed with about its own bulk of the common peroxide of Manganese, and exposed on a slip of platinum foil to a red heat, sesquioxide, sulphuret and sulphate of Manganese will be formed, and by continuing the heat for a short time, an additional quantity of the sulphate will be produced from the sulphuret. On treating the mass with water, and filtering the fluid, a solution of sulphate of Manganese will be obtained, which will yield a white precipitate with the ferrocyanide of potassium, without a trace of iron.

Similar experiments may be made with any Manganese ores, or with substances known or suspected to contain Manganese. The quantity of materials operated on may be increased or diminished at pleasure; but if increased, the heat should be continued a little longer, to decompose any remaining sulphuret, and thus add to the quantity of sulphate formed. In the same way Manganese was detected in some minerals in which it was known to exist, and in others in which it had not been previously found; likewise in soils and sub-soils, in the ashes of coal and peat, in a number of pigment, and also in the ashes of different fabrics partially dyed brown by Manganese.

Sulphate of Manganese is formed, with sulphuret, when sulphurous acid gas is made by heating a mixture of peroxide of Manganese and flowers of sulphur, even in close vessels. The sulphate may also be more readily obtained, in quantity, by simply boiling a solution of common green vitriol in water for about a quarter of an hour or upwards, in contact with an excess of sesquioxide of Manganese in fine powder, till the solution affords a white precipitate with ferrocyanide of potassium.

Chloride of Manganese may also be formed in a similar manner by boiling an aqueous solution of protochloride of iron with an excess of sesquioxide of Manganese, or it may be made with greater facility by dissolving this oxide in common commercial hydrochloric acid, taking care that the oxide be present in excess.

The brown sesquioxide of Manganese may be made, not only by

means of sulphur, but more readily and better by mixing the common peroxide with about one-third of its weight of peat mould, sawdust, or starch, and exposing it to a red heat in an open crucible with occasional stirring for about a quarter of an hour, or until the oxide acquires a uniform brown color.

The sulphate and chloride of Manganese being extensively used in dying, calico-printing, and other arts, and in making the compounds of Manganese, the simple means stated of forming those salts, free from iron (it is presumed), are material improvements on the circuitous methods hitherto adopted.

Chem. Gaz. March 15th, 1854.

AGRICULTURAL CHEMISTRY.

EXPERIMENTAL RESEARCHES *relative to the* ACTION *on* VEGETATION *of* SALTS, *&c., employed in* CHEMICAL EQUIVALENTS.

By M. AD. CHATIN.

A.—The results which I am now about to submit to the judgment of the Academy of Sciences, belongs to the second portion of the researches which I have pursued for some years on the action exercised on vegetation, by a great number of matters both mineral and organic. The first part related to the effects of the salts of potassa, soda, ammonia, lime, baryta, magnesia, zinc, iron, manganese, copper, and lead on the potato (*Solanum tuberosum* L.). By introducing into these experiments some compounds which do not form, in an appreciable degree, any part in vegetation, my aim was to cause some phenomena which might be interesting in vegetable physiology; my hope was to discover among them some remedy for the malady which attacks this valuable tubercle. Added to the soil in which vines are planted, these same salts have had no action on the special disease of that plant; employed as a lotion to the upper parts attacked by the *Oïdium* (*Erysiphe*, according to M. Tulasne); several of them (the salts of zinc, iron, manganese and copper) have, on the contrary, caused the death of this parasite.

B.—For the purpose of verifying the effects on the vegetation of the potato, produced by the alkaline, earthy and metallic salts, I subjected the haricot (*Phaseolus vulgaris*) to a parallel series of observations.* The results complete and confirm each other in general, notwithstanding the difference between the botanical species and the soil. The following is a summary of the effects observed on the *Phaseolus*.

The salts of ammonia produced disastrous effects; but in smaller quantities they had a decidedly favorable action.

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* The experiments on the Solanum took place in the argiliferous and sandy soil of Brie, those on the haricot (as well as the other experiments which I am about to describe) were made in the calco-silicious soil of the neighbourhood of Paris.

The salts of potassa, lime, and baryta, had a decidedly good action.

The salts of iron and lead had scarcely any action.

The salts of magnesia, zinc, manganese, and copper, were injurious.

Tennant (according to M. Chevreul's account) and M. Boussingault had already observed the injurious influence of the magnesian earths.

The comparison of the salts of the same base, but of different acids, shewed that the phosphates hold the first rank, then the sulphates, nitrates, carbonates, and, finally, the chlorides.

Experiments tried on the *Solanum* and *Phaseolus* particularly shewed the *opposite action of the salts of potassa and soda*, the latter salts being in general injurious to vegetation. This fact, equally interesting to vegetable physiology and practical agriculture (in which the salts of soda are still admired), required fresh observations to prove it, and, if necessary, to generalise it. The following are the results of those which I undertook on haricots, which had already germinated at the time of the addition of the salts, on spinach, barley, and garden cress, the comparisons are made exclusively between the salts of potassa and those of soda.

I.—Action of the salts of potassa and soda on the haricot (Phaseolus vulgaris L.), the two primordial leaves of which were developed when the salt was added.—Phosphate of potassa was favorable; phosphate of soda did no harm. The nitrates were unfavorable in the proportions tried. Carbonate of potassa had no marked effect; carbonate of soda was very injurious. Chloride of potassium had very little influence; chloride of sodium was injurious. The tartrates and acetates had but little effect; the acid tartrate of potassa and tartaric acid rather retarded vegetation.

II.—Action of Spinach (Spinacia inermis, Moench).—Sulphate of potassa was favorable; sulphate of soda injurious. Carbonate of potassa had no marked effect; carbonate of soda did harm. Nitrate of potassa favored the vegetation, which nitrate of soda had not modified. Chloride of potassium had no apparent effect; chloride of sodium was very injurious. The acetates had but little influence.

III.—Action on barley (Hordeum vulgaris, L.).—Phosphate of potassa was more favorable than phosphate of soda. Sulphate of potassa permitted vegetation to go on as if the earth were without the addition of any salts; sulphate of soda was injurious. Chloride of potassium made no difference to the vegetation; chloride of sodium did harm.

IV.—Action on oats (Avena sativa, L.).—Sulphate of potassa was rather favorable; sulphate of soda perceptibly injurious. Carbonate of potassa had no marked effect; that of carbonate of soda was injurious.

V.—Action on cress (Lepidum sativum, L.).—Carbonate of potassa was decidedly favorable; carbonate of soda was very injurious. Chloride of potassium was injurious; chloride of sodium was extremely injurious.

C.—All the preceding facts agree in establishing that the salts of soda, added to soil, exert a bad influence on vegetation, and that the salts of potassa have, in general, a favorable action. It is well known

that the salts of soda are, on the contrary, useful to animals, to the exclusion of the salts of potassa. It is likewise remarkable that chloride of potassium, the only one of the salts of the potassa series which appears indispensable to animals, in whom it is localised in the muscles, should be at the same time that salt of potassa which was the least favorable to vegetation; we may, perhaps, find in these contrary influences of the salts of potassa and soda, a means of determining the real nature of those beings which are at present placed, according to the pleasure of classifiers, in one or other of the two organic kingdoms.

With the exception of marine or maritime plants, we may admit the results which prove that the salts of soda are injurious to plants as the indication of a general fact. The ancients knew of this action in chloride of sodium, and it has been confirmed by the observations of M. Becquerel, who at the same time shewed that the humidity of the soil modified it; this observation of M. Becquerel appears to me to extend to all the salts of soda.

Analyses shew that the salts of potassa are, in proportion to the salts of soda, more abundant in the ashes of vegetables than in the waters whence these plants draw their mineral elements; a similar comparison, although in not so great a degree, exists between magnesia and lime. Are these facts connected with the choice which plants make in *absorption*, or with a *special property of elimination*? We shall submit to the Academy our experiments on this yet uncertain point of vegetable physiology.

Comptes Rendus, No. 6, February 6th, 1854.

Researches *on the* Gluten *of* Corn. By M. Millon.

In 1848 and 1849, the study of several kinds of corn gathered in the Arrondissement of Lille, enabled me to prove great variations in the proportion of gluten furnished by their flour. These specimens of corn were all of the best appearance; they had been gathered and kept by careful farmers, and I possessed complete information of the different places from which they had been procured. On my part I took the pains to grind them in my own laboratory; the flour was analysed almost immediately, and no uncertainty could remain as to the product.

Among these grains, poor in gluten, I shall first mention an English red wheat, which only gave six per cent. of dry gluten; this gluten was collected with difficulty, but the estimation always gave numbers between 5·7 and 6·3 per cent. The nitrogen was not small in proportion; its number corresponded to 10·3 per cent. of gluten, or rather of albuminoïd principle.

Another sample of English red wheat, which I analysed in comparison, contained a normal proportion of gluten, and I found it impossible to establish any distinction between the corn which contained 6 per cent. of gluten and that which contained 10.

In 1852, M. Roy, inspector of colonisation, sent me several samples

of corn, which represented the principal types of the cultivation in the neighborhood of Algiers. One of these samples, gathered at Guyotville, was remarkable for the size of the grain; it was a most beautiful soft corn: I could not get any gluten from the flour. The operation, performed again with the greatest precaution, only gave a brittle paste, which soon cracked, and which, moistened with the smallest quantity of water, and on a very fine sieve, left, instead of gluten, a dry and friable matter. After dessication, this matter, which was so different in its appearance, represented 4·8 per cent. of the flour. The proportion of nitrogen contained in the corn was very great, and corresponded to 11·5 per cent. of gluten, or rather of albuminoïd matter.

The estimation of the different principles contained in this corn used the whole sample, and I could not procure any more. But in the harvest of 1853, M. Roy took great pains to procure some of this corn (sample No. 1), which was much prized in the market, and which is still cultivated at Guyotville. I have now nearly 10 kilogrammes of it. The search for gluten gave results analogous to that of 1852. I obtained 3·5 per cent. of the same friable substance.

On a close examination of this corn, I discovered that the grains, which at first sight appeared remarkably uniform, nevertheless contained two quite distinct varieties. There were a few grains (sample No. 2), which, although of the same form as the others, were glossy externally, and had an almost horny fracture. I separated these glossy grains and estimated the gluten in them; it was then collected with the greatest ease, and amounted to 11·8 per cent. of the weight of the flour.

On the other hand, I estimated the whitest grains, those which contained most fecula, and their flour gave not even a trace of gluten.

This corn then consisted of a very few grains, rich in gluten, but chiefly of grains, which contained none at all.

This great dissimilarity in the nature of the nitrogenous matters is quite compatible with such a similarity of form, that all the grains, whether rich in, or entirely without, gluten, appeared as if cast in one mould; it is not even uncommon to find in one grain a portion appearing horny and a portion not so,—that is, one part rich and the other poor in gluten.

An idea struck me that this composition of the corn from Guyotville might be a general fact applicable to all soft corns. I sought the verification of this fact, by examining some tender corns whose natures was opposed to that of the Guyotville corn; I took a soft corn from Algiers (sample No. 3), almost entirely composed of glazed grains; I separated the least glazed grains, less horny than the others and containing the most fecula (sample No. 4), and estimated the gluten in both. The sample No. 3 gave 11·9 of gluten; the sample No. 4 only gave 9·5.

The same experiment was tried on a corn from Aix, whose moss was very glossy; it contained 13·5 per cent. of gluten; those grains which were not horny, but slightly feculent, contained only 10·3.

The English red corn gathered in the Arrondissement of Lille, which

I first mentioned, should contain a very large proportion of feculent grains not containing gluten; it would be the same, doubtless, with regard to the other northern corns which I have examined, and whose proportion of gluten did not exceed 8 per cent.

The corn from Guyotville, collected two years following in a country in which cereals grow splendidly, renders certain the existence of corn entirely without gluten. The diminution of the gluten, to different degrees, in different species of grain, appears to me a necessary consequence of the preceding facts. These corns produce and pour into the market flours of corresponding characters; whence it may happen that the freshest, most beautiful flour may contain, in cases which I have hitherto supposed exceptional, but which may be very frequent, a proportion of gluten amounting only to 7, 8, or 9 per cent. In legal cases, this yield is of the greatest importance. I have seen a very careful report of a seizure of flour; the careful study of the report, and information obtained from other quarters, led me to think that the slight deficiency in gluten, proved by the chemists consulted, belonged to the nature of the corn. This report, nevertheless, caused the confiscation of the flour, a heavy fine, and the imprisonment of the seller.

When the harvest has been deficient, attempts at sophisticating flour are sure to increase in number; if the vigilance of the authorities requires doubling, their conclusions should likewise be arrived at with a careful attention to all the facts acquired by science and experience.

This distinction between the corns, which are rich and poor in gluten, likewise gives an opportunity for adulteration, as the flour of corn rich in gluten will best support the addition of maize or potato flour, and probably of other feculent substances. Bread is made easily with a mixture containing a great proportion of these substances, if the corn flour were rich in gluten. Thus the hard corns, in which the nitrogen is represented chiefly by gluten, had a great advantage for this purpose over the soft grains.

I must, however, mention that gluten is not indispensable to panification. I made bread with flour containing no gluten, from the Guyotville corn; the paste was difficult to work, it was very short, and it rose slowly, but it was made in the regular way. This bread had several peculiarities when eaten; it seemed to stop in the throat, like very stale bread. It is probable that, independently of its other properties, gluten contributes to render the mouthful easy to swallow. I must again observe that this bread required a great deal of saliva; common bread takes less, and bread from hard corn still less.

Répertoire de Pharmacie, February, 1854.

On the DISEASES *of* VEGETABLES. By M. L. F. HOFFMANN.

MANY treatises have been written on the diseases of vegetables. All learned bodies have been occupied with the potato disease which has for some years occasioned so much anxiety. Hitherto the real remedy

has not been discovered, but we believe that this disease arises on one hand from the manures employed, and on the other, from the dampness of the atmosphere, as well as the degeneration of the tubercle due to the necessarily various culture which it has received. Each species of plant has received from Nature a certain point of development which it may attain without becoming diseased.*

The maximum of this development of vegetables is at present unknown to us as regards most cultivated vegetables. As to the height, diameter, and general development which a plant may attain in favorable circumstances, we know scarcely anything.

Our knowledge is not much farther advanced as to the law which regulates the relative quantity of the constituent parts in different vegetables. Let us, then, the first thing, set aside cultivated plants, and let us study only those which grow wild and in their natural localities. We are only likely to find the normal type among wild plants. The cultivated plant is in an abnormal condition and will not enable us to attain this end.

The analysis of wheat gives sometimes 35 per cent. and sometimes 12 per cent. of gluten, and in some cases even only 9 per cent. If we wished to know how much gluten is contained by wheat in its normal state, that is to say, when uncultivated, we should find it difficult to obtain an answer.

Do we imagine that we know the normal state of the fecula of the potato? Analysis demonstrates that in the cultivated plant it may vary from 9 to 24 per cent. In truth, it may be said that these different results do not proceed from the same tubercle, but from different varieties, although they all belong to the *Solanum tuberosum*.

Even if we look at the results of experiments which have been made with one sort only, we shall find that the yield has not been the same each year, as may be known by the result obtained with alcohol in different years from the self-same species of potato.

Cultivated plants then will not help us to the solution of this problem; and to arrive at the diseases of cultivated plants we must first study them in the wild state.

If, for example, we suppose that by means of careful researches we shall confirm that the normal relative proportions of gluten and fecula are 20 to 80, and that independently of that, we may advance with certainty that this proportion cannot be changed in cultivation without the danger of the wheat, from various causes, becoming diseased, we should direct all our attention to maintaining this number unaltered. Science is then still in her infancy with respect to the diseases of vegetables. Supposing that we admit that a grain of corn may attain a weight of 5 grms., provided that its gluten remains in the

* We have seen our opinion in great measure confirmed. In certain localities were they have cut the vines close, and in others where they have covered them with earth until ready to flower, so as to prevent too much growth, the grapes have been free from disease, even in infected districts. It is not surprising that a too great development of the plant, joined to other causes, such as great moisture of the atmosphere, should contribute to the development of this disease.

proportion of 20 to 80, as well as the other principles in suitable proportions, there would then be much less anxiety, provided the other constituent parts gained in proportion. It is known that the different cereals are rich in nitrogenous constituent vegetable proportions. The preceding applies equally to potatoes with relation to a nitrogenous vegetable constituent which they contain. To know on what this opinion is grounded, it is only necessary to analyse the ash of the potato or corn, and we shall recognise the alkaline salts, phosphates and chlorides, as well as the magnesian and ammoniacal salts.

Vegetable substances rich in non-nitrogenous constituents.		Potatoes.	Vegetable substances rich in nitrogenous constituents.	
	Wheat Straw.		Peas.	Wheat.
Alkalies . . 186·2 Silicic acid . . 724·3	910·5	70·1	454·7	284·9 3·5
Lime	35·0	5·8	99·1	14·9
Magnesia	traces	41·7	64·3	121·8
Oxide of iron	3·4	0·9	10·5	1·5
Chlorine	0·4	41·7	22·4	traces
Sulphuric acid	9·4	70·8	49·1	0·4
Phosphoric acid . . .	40·8	138·6	345·0	573·1

What is wanting to the number 1,000 is made up by carbonic acid and water. By analysis the ashes of the potatoes are easily distinguished from that of corn. In the tubercle of the potato, vegetable fibrine and fecula play the chief part, whereas the nitrogenous constituents form but a minor portion. In analysing the ash of the plant we come to a decided conclusion as to its richness in nitrogenous and non-nitrogenous constituents.

In choosing a manure, it is indispensable to know the constituent parts of the vegetable to be cultivated in it, so as to give in the manure the fixed principles of the plant.

Analysis will, therefore, give an exact idea of the chemical differences of those bodies which have hitherto appeared to have the same composition, although under a different form.

According to our opinion, the cause of the potato disease, as well as that of corn, is too small a proportion of the non-nitrogenous constituent principles. In consequence of this, the constituent principles which are in excess act on those which are non-nitrogenous, then on the cellular substances, decompose them, and, finally, become themselves decomposed. Carbonic acid, water, and ammonia, are formed instead of fecula; it is the same with the disease in the corn. The cause of this is likewise a too great abundance of nitrogenous vegetable constituents. The capsule, which envelopes the grain, no longer contains the constituent parts of corn (gluten, fecula). The whole internal cavity is filled with a dark brown pulverulent substance. The specific weight of the diseased grain is very small: 100 grms. of diseased wheat would, in a healthy state, weigh quite double. Here also the normal

proportion of the constituents having been shaken, a morbid condition resulted. The following table shews the difference between the constituent parts of the potato in health and disease; they proceed from the self-same species, gathered from the same piece of land.

Potatoes.	No. 1. Healthy.	No. 1. Diseased.	No. 2. Healthy.	No. 2. Diseased.
Alkalies . .	630·6	75·5	575·5	553·9
Lime . . .	4·8	8·8	6·7	12·4
Magnesia . .	37·4	45·4	41·6	63·6
Oxide of iron . .	0·8	1·0	1·0	2·2
Chlorine . .	37·7	33·3	30·6	24·5
Sulphuric acid .	53·7	50·9	61·8	29·6
Phosphoric acid .	124·7	119·0	113·9	82·8

That which is wanting of the number 1,000 is made up of carbonic acid and charcoal. Admitting that the ash of the healthy potatoes contained the normal proportions of the constituent parts of this tubercle, and comparing the ash with that of the diseased potato, we see a disproportion between some of the numbers. The ash of the diseased potato contained more phosphoric acid and magnesia, and less alkali, sulphuric acid, and chlorine than the ash of the same species of potato when in a healthy state.

As for wheat, the following is the difference between the diseased and the healthy. All the analyses were made by a German chemist, M. Petzhold.

	Healthy Wheat Straw.	Diseased Wheat Straw.	Healthy Grain.	Diseased Grain.
Potassa . .	154·83	150·33	258·10	266·87
Soda . .	31·34	55·12	26·81	71·14
Lime . .	36·00	23·23	14·89	38·30
Magnesia . .	traces.	traces.	121·78	116·45
Oxide of iron and magnesia .	3·79	3·19	1·48	0·51
Chlorine . .	0·37	traces.	traces.	traces.
Sulphuric acid .	9·44	5·01	0·37	3·10
Phosphoric acid .	40·82	103·87	573·14	500·00
Silicic acid . .	724·32	659·22	3·35	2·58

What is wanting to the number 1,000 is formed of charcoal and carbonic acid.

Regarding the proportion of constituent principles in the ash of the healthy wheat as being in the normal state, we find in the diseased straw an excess of phosphoric acid,—and, on the contrary, the alkalies and silicic acid are in insufficient proportion. Thus knowing in which plant a disproportion exists, whether in excess or in deficiency, we are on the road to learn how to stop the progress of the disease. We think also that the organic formation of the constituent organic parts is connected with the existence of certain constituents of the soil in

which the plant grows, and that one or more of these constituents depends on the manure given to the soil.

If the point were now the potato disease, the manure destined for potatoes should be analysed, so as to ascertain that it shall not contain an excess of phosphoric acid. It would be the same with the diseased corn, supposing always that the existence of the malady should be ascertained to depend on a similar disproportion of the nitrogenous and non-nitrogenous constituent principles.

In conclusion, we say:—

1st. That it is more prudent not to sow seeds in a field which has just been covered with fresh manure, but to wait until the manure has become thoroughly decomposed, otherwise it is probable that the organic bodies still existing in it may give rise to certain peculiar cryptogamia.

2nd. That rainy or damp years favor the formation of these cryptogamia, and this abnormal development is then propagated with extraordinary activity.

3rd. That a very wholesome manure is formed of the ashes of the same plant which it is intended to cultivate. In fact, the ash contains and represents all the consistuent parts of the plant itself. The ash is deprived of all organic matter; whereas, in common manure, it is to be feared that some disease may develope itself, of which the undestroyed organic substances are the indubitable origin.

Artificial manures are preferable to ordinary manure, especially when care is taken to form them so that they shall represent exactly the constituent parts of the plant which is to be cultivated in them. These manures in a small volume, are then very advantageous, as they are easy of transport, and keep perfectly, especially when in a pulverulent form.

Répertoire de Pharmacie, March, 1854.

On the DIGESTIVE PRINCIPLE *of* WHEAT BRAN. By M. M. MOURIES.

THE digestive principle of bran may be obtained in three different states:—1st, soluble; 2nd, modified by precipitation; 3rd, by heat.

A.—In the soluble state it exists in bran water, to which it communicates its characteristic properties. To obtain it in a sufficiently pure state to study its properties, bran is macerated for six hours in 10 parts of commercial alcohol, diluted with twice its volume of water; it is pressed, and this operation is performed three times, which removes the dextrine and sugar, without dissolving or coagulating the digestive principle. The pressed bran is mixed with five parts of distilled water, and left in contact with it for half-an-hour; it is again pressed; the liquor is filtered and evaporated at 104° F. Thus prepared, this body is dry, amorphous, analogous to albumen, very soluble in water, insoluble in alcohol, ether, and oils. Its decomposition by heat gives ammoniacal products, its solution is coagulated by a heat of 159° F. An excess of alcohol likewise coagulates it; acetic, tartaric,

hydrochloric, sulphuric, oxalic, and phosphoric acids, at all degrees of hydratation; oxalic acid, &c., diluted with water, precipitate it in the form of cheesy flocks. These precipitates all redissolve with an excess of acid. The concentrated acids do not render the solution turbid; neutral pressure has no action. Five centigrammes transform, in twenty-five minutes, 10 grains of starch, kept at 113° F. The addition of an alkali gives no appreciable precipitate, but it modifies the action on starch in the same manner as if coagulated by an acid, a modification of which we are about to speak.

This new body has, as is evident, great resemblance to amandine, albumen, and legumine, but it differs in its clear reactions, and especially in its digestive action on starch; the addition of alcohol and heat separate it from the diastase, which appears to be only this same principle modified by germination.

B.—When precipitated by an acid, it requires six hours to transform starch instead of twenty-five minutes (using the same quantities). Its solution in an excess of acid or alkali does not change the duration of this reaction. Precipitated albumen, legumine, and amandine, in the same proportion, have no action in starch.

C.—Coagulated by a heat of 159° F., it is insoluble both by acids and alkalies; it has many analogies with modified albumen, but it still retains the property of converting, in six hours, 10 grms. of starch, in the quantity of five centigrammes, a property which is not possessed by albumen.

Comptes Rendus, No. 11, March 13th, 1854.

TOXICOLOGY.

On the Poisonous Properties *of the* Yew Tree.

M. Dujardin, veterinary surgeon at Bayeux, informs us that, being sent for to prove the cause of death of two mares who had perished the day before, whilst harnessed to the same vehicle, he found on examination that they had died from eating the leaves of the yew (*Taxus baccata*).

He then procured a horse whom he caused to eat the leaves of the same tree; the poison acted so rapidly, that he fell as if struck by a thunderbolt, about an hour after beginning to eat them, and when a few symptoms of colic had only just manifested themselves. On examination, the same characters were perceptible in the stomach as had been seen in those of the mares.

He has since heard that sheep, cows, and asses, have died suddenly in pastures where yew-trees exist.

The poisonous properties of the yew-tree have been long known; according to Plutarch its smoke will destroy rats; the juice was used, says Strabonius, to poison the arrows of the Gauls; Theophrastus says that the leaves are poisonous to horses, but not to ruminating animals; Pliny writes that wine in Spain, if put into casks made of yew, caused the death of those who drank of it. If we are to believe Cæsar's Commentaries, Cativulus, King of the Ebronians, killed himself with the juice of the leaves.

Dioscorides says that the berries of the yew poison birds; other authors say that the roots of this vegetable, thrown into water, poison fish. Beaulieu mentions the death of a horse who had eated yew leaves; and this fact has been verified at the school at Alfort.

Journal de Chimie Médicale, February, 1854.

THERAPEUTICS.

On Calcareous Sulphate *of* Quinine *in* Diabetes.

By MM. A. Delondre and O. Henry.

A person affected with diabetes in the highest degree, with progressive emaciation, ardent thirst, and complete depression, after many useless endeavors, received from her medical attendant a prescription for extract of cinchona; but as the composition of this medicament has always appeared uncertain, and as the extract had not succeeded well, we thought that the action would be more sure and efficacious by replacing the extract of cinchona with a calcareous mixture of quinine. Our aim was, according to generally adopted principles, to render the alkaloïd as alkaline as possible, so as to neutralise the acids in the stomach, which transform the fecula of food into glucose, and to obtain, at the same time, the tonic results necessary for stopping the disorganisation and weakness which are the result of this terrible disease. In the dose of 50 centigrammes each day, taken morning and evening, and following the general treatment advised by the physician, we had the pleasure of seeing the sugar in the urine gradually diminish, until, at the end of six months, not a trace remained. The patient had regained her flesh, and is now in a perfect state of health. The interest which we took in the patient, and the repugnance which she felt at taking the extract of cinchona uselessly, led us to endeavor to prepare this medicament in the smallest possible volume, and so as to be taken at one effort. We are of course only anxious that the efficacy of the remedy should be tried in many cases.

We have prepared it as follows:—

Precipitate a solution of sulphate of quinine in boiling water, with an excess of milk of lime; filter, wash and dry at a moderate temperature.

Répertoire de Pharmacie, January, 1854.

On Tartar Emetic *in the* Treatment *of* Phthisis.

We have already mentioned the good effects of stibiated tartar in doses of 2 centigrammes in 20 pills, one of which is to be taken each day, in the treatment of phthisis, and we now find that M. Bricheteau calls attention anew to the administration of stibiated tartar in this terrible affection; only the doses generally recommended by M. Bricheteau are much larger, and he is not afraid of making his patients vomit a great many times, and with but little intermission, and he thinks that the physiological explanation of the manner in which the emetic acts may be found in Carwel's theory of tuberculisation; according to this author, tuberculisation is the result of a very complex secretory

function; the tuberculous matter would be in the first place deposited on the free surface of the mucous membranes, particularly on those which cover the bronchial extremities, then on the pulmonary tissue. Now, nothing could be better adapted to prevent this deposition of tuberculous matter than the reiterated action of emetics capable of augmenting the bronchial secretion and facilitating its expectoration, so as to prevent the malady from reaching the pulmonary organs. This local action must necessarily be powerfully seconded by the other effects of the emetic, such as diaphoresis, stimulation of the biliary apparatus, the general activity which extends to the whole of the secretory organs: such is the explanation proposed by M. Bricheteau. However this may be, it is certain that in many cases of phthisis, the administration of tartar emetic has been followed by the most advantageous effects; for from the time that Langlois, of Montpellier, prescribed emetics to consumptive patients (1818), it has produced, if not cures, at least great amelioration of this dreadful disease. Nevertheless, we cannot admit as sufficiently proved, the success attributed to Giovanni de Vitis, chief physician to the Italian army, who, having the charge of the military hospital at Capua to which are sent almost all the consumptive patients in the army, prescribes emetics in all cases. According to this physician, from the 1st May, 1828, to the 18th January, 1832, there went out of the hospital at Capua, perfectly cured, forty cases of chronic catarrh, forty-seven of phthisis in the first stage, a hundred and two of phthisis in the second stage, and finally twenty-seven which had reached the third stage of the disease, making altogether two hundred and sixteen cures, a hundred and sixty of which were consumption. The mode of treatment consisted of giving, night and morning, a dessert spoonful of the following mixture:—

℞.	Infusion of elder flowers . . .	150 gram.
	Stibiated tartar	15 centig.
	Simple syrup	30 gram.

A second spoonful was administered in a quarter of an hour if the first had not produced vomiting. The patients were at the same time kept upon a light farinaceous diet, composed principally of rice, chocolate, and biscuits. If the stibiated tartar produced a severe purging, it was suspended for a few days and replaced by digitalis and ipecacuanha, to which he attributed powerful effect in the cure of diarrhœa when administered in the dose of 5 centigs. each, repeated every hour and even oftener, until the diarrhœa ceases.

Without admitting all these results, especially those concerning phthisis at the third stage, and supposing that many diagnostic errors may have been made by M. Giovanni Vitis, and that he may have confounded catarrh and other mild complaints of the lungs with confirmed phthisis, it is difficult to deny or doubt the utility of the means employed at the hospital at Capua.

M. Bricheteau follows the same method of administration as Dr Vitis; thus he gives from 5 to 15 centigs. of stibiated tartar in 150 grammes of water of infusion of elder flowers with the addition of 30 grammes of syrup. The patient takes generally a dessert spoonful

night and morning, two hours before and after a meal, he adds a second spoonful where the medicament has produced neither nausea nor vomiting. For fifteen years he has always employed the same method of treatment with marked advantage, and without having met with any great inconveniences. Independently, he says, of a comparatively small number of cures considering the many experiments tried, a great many patients who have not remained long enough in the hospital to have attained positive results would answer to inquiries on the subject; "I have found great relief from the treatment, I have no longer any pain and oppression at my chest, I expectorate less, I have more appetite and strength, the night sweats have disappeared," &c. Nevertheless, he adds, it is difficult to draw decided conclusions as to a plan of treatment which is so repugnant to many patients and with regard to which they will often deceive the doctor.

Stibiated tartar has always been administered in well characterised cases of phthisis, but very frequently in cases much too far advanced for any hope to be entertained of effecting a cure. Many of them remained in a stationary condition even for some years. Many give it up to resume, after some months, a treatment from which they say themselves that they found so much relief; they give it up again, and again are glad to resort to it. One returned in 1850, after a lapse of three years, to M. Bricheteau. From the time of his departure from the hospital he had continued to work, and after a short time he left again in much the same state.

He has never met with any inconvenience except nausea and purging; quickly cured by suspending the medicine, and administering some doses of ipecacuanha and purple digitalis.

Richter, Robinson, and Russ, have employed stibiated tartar in phthisis, and all mention its perfect harmlessness; the last, mentions having caused it to be taken at all periods of phthisis, with the exception of three cases, in which he was forced to stop the administration of the medicament; in all the others, namely, seventeen cases, it produced good effects; almost always the cough became quieter, the respiration freer, the appetite better. We should add, that latterly, finding the repugnance that patients felt to the medicine, M. Bricheteau has given the emetic in much smaller doses—thus, he substitutes ipecacuanha for stibiated tartar, and only gives 10, 15, or 20 grammes of syrup of ipecacuanha in 120 grammes of a gummy vehicle, which is taken by spoonfuls; or else he uses Dr. Vitis's mixture, reducing the quantity of tartar emetic to 3 or 5 centigrammes.

It is, however, right to take into account, in the observations of M. Bricheteau, the influence of repose and a better diet on the health of the patients received into hospitals, and who previously have been exposed to such deplorable hygienic influences. We have ourselves tried the effect of small doses of stibiated tartar in consumptive patients, whose general hygienic influences remained unaltered, and have found the most variable results: sometimes a great amelioration, sometimes no good result; but never any injurious effect. This is then a means which we think should not be neglected by medical men. It

is useless to add that no other means should be neglected either, and that, even without the tuberculous diathesis, there are often other indications which will not allow of being neglected.

Journal de Pharmacie et de Chimie, March, 1854.

PROCEEDINGS OF SOCIETIES.

CHEMICAL DISCUSSION SOCIETY, JAN. 26, 1854.

(*Continued from p.* 448.)

Mr. COLLINS suggested that, perhaps, the Ung. Hyd. Nitratis might contain free acid, which was often left in it, in order to leave it soft and of a good color.

Mr. GREENISH thought the ointment should be prepared at a low temperature, in order to prevent the decomposition of the nitrate of mercury, and thus prevent the existence of free acid in the ointment. He thought it unwise to sacrifice the efficacy of the ointment to its color and consistence.

Mr. HOPKIN begged to differ entirely from Mr. Greenish. He thought the ointment could be prepared of a good color and of a soft consistence perfectly free from acid. The plan he adopts, is to raise the temperature of the melted lard, &c. in a water-bath, and add the solution of mercury in the nitric acid as soon as complete, which increases the temperature also very much; a brisk effervescence ensues, and he keeps it constantly stirred until it no longer tastes acid, and he has always found the result satisfactory.

Mr. SPENCER stated, that in preparing this ointment a reaction took place between the nitric acid and the oleïc acid, which not only got rid of any excess of nitric acid that might be present, but converted the oleïc acid into elaïdic acid. If this process was conducted at a low temperature the elaïdic acid formed would give rise to a solid product; but if the temperature were raised, a further decomposition was effected, the free acid was decomposed, and the resulting ointment was of the color and consistence of butter, and kept good for a length of time.

The experience of several members present was certainly in favor of making the ointment at a raised temperature.

Mr. GREENISH stated, that he had found that after washing the ointment in distilled water the creosote could be mixed with it without decomposition; and he further observed, that the Ung. Hyd. Nitrat. mixed with the Ung. Sulphuris of the Old Pharmacopœia became speedily discolored; but, as in the New Pharmacopœia, the Ol. Bergamot was omitted, this did not now take place, and might lead to some confusion if the cause was not distinctly pointed out.

Mr. HOPKIN mentioned a somewhat similar circumstance which had occurred in his own experience, except that instead of slow decomposition, rapid action and inflammation (!) had arisen from the dropping of creosote on to oxide of silver, ordered in a prescription; he warned members to be careful of an explosion, if they should happen to have such materials to mix together.

Whilst on the subject of decomposition in prescriptions,

Mr. BLYTH gave an example of a mixture, the compound resulting from which he thought would test the skill of any one desirous of making the examination. It was composed of sp. eth. nit., potas., iodid. er., ferri. sesquichlor., et aq. cassiæ, whichever way it was put together violent decomposition and effervescence took place. It was very common to prescribe Sp: Eth: Nit.: with iodide of potassium. Perhaps it was not generally known that however neutral and pure the Sp. of Nitric Ether might be which was employed, immediately on being placed in contact with water decomposition commenced and hyponitrous acid was set free; and unless some alkali was added in excess the iodide of potassium was decomposed and free iodine liberated; but in the mixture under notice not only was there a neutral decomposition of the nitric ether and the iodide of potassium, but as soon as the zinc of the sesquichloride of iron was added a violent effervescence took place. Chlorine and iodine were evidently liberated; the sesquioxide of iron was at first precipitated and then redissolved. He would not undertake to say what was the resulting compound left in the mixture.

The meeting was then adjourned to February 9th.

Chemical Discussion Society, Feb. 9, 1854.

Mr. Tewtrell in the Chair.

Mr. Hopkin laid a fine specimen of "Iron Alum" on the table, and suggested that as it had been stated that the name, Potassio-Sulphate of Peroxide of Iron, was a more appropriate name to be applied to this article, and more in consonance with the nomenclature of the London Pharmacopœia, where alumen is further described as "Aluminæ et Potassæ Sulphas." Taking this as a prototype, he thought it should be named "Ferri et Potassæ Sulphas," and not "Ferri Potassio Sulphas;" which would lead to the idea that it was a salt similarly constituted to the "Ferri Ammonio Citras" of the Pharmacopœia.

Mr. Blyth said that he had explained the composition and mode of preparing the salt elsewhere*, and he now begged to remark, that although a difference of opinion seemed to be entertained as to what it should be called, he considered that no argument had yet been adduced in favor of the disuse of the generic term "alum." The names Aluminum and Alumina had been applied to the metal, and its oxide discovered to be the base of the common alum of commerce; whereas, the term "Alumin" had been employed by Pliny as early as the commencement of the christian era; and its Greek analogue, by Herodotus, 500 years before that, to signify what it really meant, viz., an *astringent salt* or *mineral*. The term has ultimately been applied to one substance,—the alum of commerce; and taking that substance as a type, had been applied by chemists as a generic term to describe the peculiar constitution of a whole series. He had not coined this name for bringing out a new and secret preparation; this substance was described under this title in every standard chemical work; and he thought the name not only simple but very expressive, as distinguishing it from all salts, having similar bases and acids in their composition, and having an equal right to the title of "Potassio-Sulphate of Peroxide of Iron;" but with different proportions of water of crystallisation and degrees of solublility. It was true the "*Alumen*" of the Pharmacopœia was described as "Aluminæ et Potassæ Sulphas" in the Materia Medica; but in all the formulæ in the rest of the Pharmacopœia the name "Alumen" was adhered to. At any rate, the name ought not to be abolished until some other generic term could be suggested, which should embrace the whole series; and he thought that, —using the term "Alumen" as a generic one,—each separate salt of the series could be clearly indicated by the addition of the names of their respective bases; thus, "Alumen Ferri et Potassæ"—"Alumen Ferri et Ammoniæ," &c. &c.

Mr. Williams quite agreed with Mr. Blyth in considering that the best and most scientific name for the compound in question was "Iron Alum;" adding also the name of the protobase employed, if it were thought sufficient to distinguish Potash Iron Alum, Ammonia Iron Alum, &c., from each other. The term alum was objected to, because it was supposed to refer entirely to salts containing Alumina; but such was not the case,—as for many years Chrome Alum and Manganese Alum had been recognised by chemists, and constantly referred to under those titles. And as the science of chemistry advances, we find numerous instances of series of bodies having analogous characters, though differing in elementary constitution, bearing some generic name, frequently derived from the first or best known term of the series; thus he would allude to the Amides, the Ureas, &c., and more especially to the Ammonias. In this instance, the atoms of Hydrogen existing in Ammonia were capable of replacement by various compound molecules; the atomic constitution was completely altered, but the body retained the physical characters of the original type unimpaired. It was still a gas, soluble in water, having a pungent odor, being a powerful base, and often, except by an ultimate analysis, difficult to distinguish from the normal Ammonia. This, and many other instances which might be brought forward, he thought good ground for maintaining the generic term "Alum," simple as it is, more especially as a sulphate of peroxide of iron and potassa, or ammonia, could be readily produced, but having none of the distinctive and well marked characters of an alum. He thought that it should be our endeavor to raise the chemist and druggist

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* *See* "Pharmaceutical Journal," vol. 13, p. 306.

to the standard of true chemistry and science; not to reduce that standard to our ignorance or convenience.

Mr. LINFORD confirmed the statement that sulphate of peroxide of iron and potassa could be obtained, not having any of the characters of an alum; he had found it in needles, containing six equivalents of water, and possessing the ordinary characters of a salt of the peroxide of iron.

Mr. WILLIAMS then read a paper on the production of bromine, in which, after describing the usual process by which bromine is produced from the supernatant liquor of sea water, or some other water containing bromides, by passing a current of chlorine through it with proper precaution, and shaking with ether, which separates the bromine from the solution. The bromine is afterwards separated from the ether by means of caustic potassa, evaporated to dryness, fused, and the mass distilled with peroxide of manganese and sulphuric acid, by which means the bromine is obtained nearly pure. The author recommended the use of benzole in place of the far more costly ether, and stated that he found it answer the purpose much better from its smaller degree of volatility, and the great power it possesses of dissolving the bromine. He was at present engaged in a series of experiments with the object of more readily separating chlorine from bromine than was at present possible; he could not on the present occasion lay the results before the meeting, but hoped that the time would soon come when bromine might be added to the list of British manufactured articles, and that we should hereafter be independent of foreign sources for our supply of an element which we have in such abundance around our own coasts.

Mr. LINDSEY BLYTH described the process of M. Henry, of Paris, for separating chlorine, bromine, and iodine; but thought, that although the process was admirably adapted for analytical purposes, it would not answer as a manufacturing process.

The meeting then adjourned to February 23rd.

QUEVENNE'S IRON.

TO THE EDITOR OF THE PHARMACEUTICAL JOURNAL.

SIR,—In your number for January last, Mr. Morson, in a paper entitled "*On the Substitution of the Magnetic Oxide of Iron for Iron in the Metallic state reduced by Hydrogen*," makes a complaint against me, and I ask the favor of sufficient space in your present number to reply to the charge.

The complaint in question represents me as having done a double wrong. I have, in the first place, given Mr. Robertson an analysis, which, as it is at variance with the results of other chemists, must be inaccurate; and in the second, I have not pointed out that Mr. Heathfield's "Quevenne's iron" was magnetic oxide of iron. I shall discuss these complaints in turn; and first of the analysis, which I reprint here in full, as in Mr. Morson's paper two of the figures referring to Mr. Heathfield's specimen are given wrong, and the numbers make up only 97·01 instead of 100. I do not doubt that this mistake has been made inadvertently, but it has unavoidably misled readers of the paper, for the percentage of iron in Heathfield's preparation is represented as 75·55 instead of 78·55, and my statement that it contained seven per cent. more of the metal than Morson's preparation, is necessarily unintelligible. The percentage of water is also made 1·43 instead of 1·42.

"*Analysis of two samples of Quevenne's Iron received from J. Robertson, Esq., Druggist, George Street, Edinburgh.*

	No. 1, Morson's.	No. 2, Heathfield's.
Metallic iron	71·35	78·55
Sulphur (as sulphuret of iron)	0·50	0·41
Water	2·01	1·42
Insoluble in acids	2·52	1·24
Oxygen and loss in analysis	23·61	18·38
	100·00	100·00"

I have no alteration to make in the analysis printed above, which I feel assured is quite accurate. Had I anticipated the controversy which has arisen, I should have preserved a portion of the specimen of Morson's preparation, so that the analysis might be repeated, but not looking forward to any such discussion, I did

not keep that specimen apart. However, it was not thrown away. I received from Mr. Robertson originally a small sample of Morson's "Quevenne's iron," with which some preliminary trials were made, and afterwards a larger one, which was employed for the analysis given above.

When the report was furnished to Mr. Robertson, the samples were put together into the same tube, as substantially identical, with a view to their preservation as a specimen for exhibition to my class. This mixed sample I have taken as the nearest approximation which can now be obtained to the original specimen, and have subjected it to analysis. In making this analysis, as well as the published one, and those referred to in the sequel, my assistant, Dr. S. Macadam, and I have worked together, with a view to guard more certainly against mistake.

This analysis was made as the first had been, by digesting the powder in aqua regia, filtering from insoluble matter, and precipitating by ammonia. The precipitate was carefully washed and dried, and after the filter had been incinerated, the oxide of iron was moistened with nitric acid and raised to a low red heat. The quantity of peroxide of iron thus procured corresponded to 75·03 per cent. of metallic iron, or the mixed specimen contained nearly four per cent. more of iron than the original sample which formed a part of it—3½ per cent. less than Heathfield's preparation—and 25 per cent. less than pure *Pulvis Ferri* should yield.

As for the second complaint, namely, that I did not call Mr. Heathfield's preparation "Magnetic Oxide of Iron," I might reply that, had I done so, I must, and with greater emphasis, have applied that name to Mr. Morson's preparation. I applied it to neither, because, in the first place, the question referred to me by Mr. Robertson was, "Which of these preparations is the purer Pulvis Ferri?" and, secondly, I did not, and do not regard either as having been the definite magnetic oxide of iron, $Fe^3 O^4$. This will sufficiently appear if the amount of metal and oxygen be compared. The proportion of oxygen in Heathfield's preparation, namely, 18·38 per cent., would unite to form magnetic oxide (Fe^3O^4) with 48·24 of iron, leaving 30·31 of metal over. The proportion of oxygen in Morson's preparation, namely, 23·61, would unite to form the same oxide with 61·97 of iron, leaving 9·38 of metal over.

As for the exact condition in which the metal was in these specimens, I did not, and do not now, offer any opinion, further than that I believe a portion to have been free, and a portion oxidised. But oxide of iron, partially reduced by hydrogen (which was the chief component of both preparations) will contain iron in the metallic form, and also complex combinations of all its known oxides. A mere calculation from the percentage of oxygen will not determine the condition of the metal, neither will the action of such reagents as potassa and ammonia on the dissolved compound. Only a protracted and difficult analysis will supply the requisite information, and even then the result will be in part conjectural. I neither offered, nor was asked to make such an analysis, which was quite uncalled for; but in the report on Messrs. Morson and Heathfield's preparations, I applied impartially the same standard, and that a fair one, to the determination of their respective claims, to be called metallic iron.

I have only to add, that whilst Mr. Robertson, who has long been known to me as a very conscientious and scrupulously careful Pharmaceutical Chemist, had my full permission to make known to others my analysis and report, had I foreseen that I should be involved in the controversy which has arisen, I should have requested from Messrs. Morson and Heathfield several samples of their "Quevenne's Iron," before allowing myself to be referred to publicly as an arbiter between them.

It may contribute to put an end to a dispute, which the exercise of a little charity might have prevented ever arising, if I state that I have no doubt, from the quality of the sample of Quevenne's iron analysed by Dr. Gregory, that Mr. Morson can prepare *Pulvis Ferri*, making as near an approach as is needed to perfect purity; and that Mr. Heathfield can prepare it equally pure, I have ascertained for myself by the analysis of two specimens sent me by Mr. Robertson, the one of which contained 91·51, the other 98·59 per cent. of metallic iron.

I remain, yours truly,

GEORGE WILSON, M.D.

Analytical Laboratory, Surgeons' Hall, Nicolson Street, Edinburgh, Feb. 18th, 1854.

Pharm. Journal.

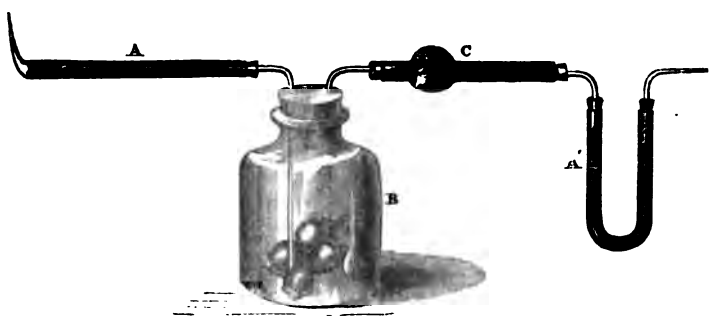
THE CHEMIST.

ORIGINAL COMMUNICATIONS.

Chemical *and* Microscopical Examination *of the* Fruit *of the* Medlar *(Mespilus germanica); with some* Observations *on the* Phenomena *of* Bletting *or* Rotting. By Thornton J. Herapath.

The fruit of the medlar, as is well-known, when fresh-plucked from the tree, is firm and hard in the flesh, and possesses a peculiar rough astringent taste; but after it has undergone the process of *bletting* or *rotting*, it becomes soft and sweet-tasted, and is then considered fit for the table. Although the medlar is the only fruit of our own, or perhaps of any other country, which exhibits this remarkable peculiarity, it would not appear that any chemist up to the present time has considered the subject of sufficient interest to merit an investigation, or has attempted to ascertain the nature of the changes which the fruit undergoes during the operation. However this may be, the following experiments, which were commenced in the latter part of the year 1846, will perhaps supply a desideratum.

I.—On the Action of the Air upon the Unripe Fruit.—Six medlars, weighing altogether 1480·8 grains, were taken and introduced into an apparatus, of which a diagram is given below, where they were kept at an uniform temperature of about 75° F., until they had become perfectly ripe and mellow. In this state they arrived in about seven or eight days, during the whole of which time a continual, but slow, current of dry atmospheric air was kept passing through the apparatus by means of an aspirator, the gaseous products being absorbed by passing them through two tubes, one filled with fused chloride of calcium, and the other with moistened sticks of caustic potash.



In the above diagram *A* and *A'* represent the two tubes containing sticks of *potassa-fusa*, slightly moistened with a strong solution of

caustic potassa. The first of these was about half-filled with lumps of fused chloride of calcium, and served to remove all carbonic acid and aqueous vapor from the air before it entered the bottle *B* containing the medlars; the second (*A'*), which was connected by a bent tube with an aspirator, was employed to absorb the carbonic acid produced by the action of the air on the fruit, the aqueous vapor having been already condensed by the fused chloride of calcium in *C*.

On disconnecting the several pieces of the apparatus at the conclusion of the experiment, and weighing them, it was found, that whilst the medlars had lost 9·6 grs., the chloride of calcium and potassa tubes had increased in weight respectively 7·01 and 5·21 grains. Hence, it is clear that a portion of the carbon and hydrogen of the vegetable matters, must have entered into combination with some of the oxygen of the air, and thus been converted into carbonic acid and water. The truth of this conjecture was subsequently proved by confining some unripe medlars in an atmosphere of dry oxygen over mercury, and then subjecting the gas to analysis, when it was found that a portion of the oxygen had disappeared, and had been converted into water and carbonic acid.

II.—Quantitative Analysis of the Fruit.—A. The first step in the analysis consisted in separating by pressure and washing the epicarp and stones from the fleshy and edible sarcocarp. The two former were then dried between bibulous paper and weighed; afterwards exposed for some hours to a temperature of about 220° or 230° F., and again weighed. 1000 grains of the unripe and ripe fruit gave respectively the following numbers:

	Unripe Fruit.	Ripe Fruit.
Epicarp . . .	50·589	48·69
Stones . . .	115·310	110·98
Sarcocarp* . . .	834·101	840·33
	1000.	1000.
When dried :—		
Epicarp . . .	17·90	17·27
Stones . . .	58·71	56·51

B. The sarcocarp from the above-mentioned quantity of the ripe and unripe fruit was then separately taken, and repeatedly treated with large quantities, first of cold, and afterwards of hot, water, until everything soluble in those menstrua were extracted. When completely cold the solutions were mixed and allowed to rest undisturbed for some hours, in order to allow a little flocculent starchy matter to subside. This occurred in by far the largest quantity in the solutions from the unripe fruit: the sediment from the extract of the ripe fruit was so small as hardly to admit of being estimated, and contained no starch. The solutions having been decanted from the sedimentary matters, as before described, was concentrated by evaporation and

* Estimated by loss.

then heated to the boiling-point, when some albumen separated in light flocculi of a brownish color; these were collected on tared filters, washed, dried, and weighed. The solutions and the washings of the filters were next reduced by evaporation to a syrupy consistence, and treated with twice their bulk of anhydrous alcohol; the whole was then allowed to remain undisturbed in closed vessels for about five or six hours, in order to allow the gum and pectine, &c., to entirely separate, and the mucilaginous precipitates were collected with the necessary precautions, washed with cold alcohol, dried in a steam-bath, and their weight estimated in the usual manner. They were afterwards redissolved in a very dilute solution of caustic potassa, and the pectic acid precipitated by an acid; the loss shewed the proportion of gum and dextrine. The solutions from which the gum and pectine had been precipitated by alcohol were then each transferred into a tubulated retort, and the greater portion of the alcohol recovered by distillation. The alcoholic distillate from the extract of the ripe fruit, it should be observed, when tested with blue litmus paper, was found to possess a very distinct acid reaction, and when neutralised with alkali and concentrated by evaporation, gave a crystalline precipitate on the addition of nitrate of silver. These crystals when separated, were found to possess all the characters of acetate of silver; but the small quantity obtained did not admit of my subjecting them to an ultimate analysis. This acetic acid was doubtless produced by the sugar having partly undergone the acetous fermentation, as no trace of the acid could be detected in the unripe fruit.

The syrupy mass which remained behind in the retorts was then diluted with water, and precipitated by a solution of the basic acetate of lead. The greyish-white precipitates produced were collected on filters, and well washed with cold water. They were then diffused through a small quantity of water, and decomposed by hydrosulphuric acid gas. The solutions were next boiled to expel the excess of sulphuretted hydrogen, and filtered, in order to separate the sulphuret of lead produced. Upon concentrating the liquids which passed through the filters, they were found to possess a very sour, acid taste, and strongly reddened vegetable blues; and in time deposited a number of very minute acicular crystals, which, when examined under the microscope, were found to be small rhombic prisms. These crystals dissolved easily, both in water and alcohol, and the solutions afforded crystalline precipitates with nitrate and acetate of lime, which were redissolved upon adding an excess of the acid solution. They were not precipitated by potassa or soda, thus proving that they contained neither tartaric nor oxalic acid. They were evidently crystals of citric acid, as was proved by subjecting the silver salt to analysis.

I. 7·53 grains of the silver salt, previously dried at 212° F., when burnt with oxide of copper, gave 3·866 grs. of carbonic acid, and 0·659 grs. of water.

II. 5·11 grs. gave 2·627 grs. of carbonic acid, and 0·450 grs. of water.

III. 11·014 grs. gave 6·964 grs. of metallic silver.

	I.	II.	III.	Mean.
Carbon	14·001	14·02		14·015
Hydrogen	0·973	0·978		0·975
Oxygen				21·782
Silver			63·228	63·228

Calculation requires C—14·035, H—0·974, Ag—63·157, O—21·834.

The acid uncrystallisable liquid, from which the crystals of citric acid had been deposited, appeared to possess all the properties of malic acid. The analysis of the lead-salt, however, I should observe, did not afford satisfactory results; doubtless, from its being contaminated with a little citrate of lead.

The solutions, after the separation of the lead precipitates, were saturated with hydrosulphuric acid gas; they were then boiled to expel the excess of the precipitant, and filtered to remove the insoluble sulphuret of lead produced. They were afterwards concentrated by evaporation to the consistence of a syrup, and mixed with about twice their bulk of alcohol sp. gr. 0·835, by which a small quantity of gum was separated. They were finally evaporated to dryness, and the weight of the fixed matters estimated. These consisted of sugar, extractive matter, acetate of potassa and soda, and a little acetate of lime. The extractive matter was separated by redissolving the other substances in water; and the earthy and alkaline ingredients were estimated by again evaporating the solutions to dryness, and incinerating the residues in a small platinum capsule, with the necessary precautions, and analysing the ashes by the ordinary process. These being calculated as acetates, and added to the weight of the extractive matter, when deducted from that of the dried matters operated upon, gave the proportion of saccharine matter. The amount of sugar in the fruit, was, however, more accurately estimated by a subsequent operation, which consisted in fermenting the juice of a fresh quantity of the medlars, after carefully purifying it from dextrine and then determining the proportion and density of the spirit produced.

The pulpy mass of the fruit, after being treated with cold and hot water, as before described, was next acted upon with boiling alcohol and ether. These solvents removed only an exceedingly small quantity of fatty or waxy matter, which was not further examined. The insoluble residue were found to be but slightly acted upon by acetic acid, but when digested in hot diluted hydrochloric acid, that from the unripe fruit became soft and pulpy, probably in consequence of one of its constituents being converted into pectic acid. Ammonia and caustic potassa changed its color to a deep brownish-yellow, and gradually dissolved a portion of it, furnishing solutions of the same hue, from which acetic acid and the diluted mineral acids threw down dark brownish-black precipitates. The latter were found on examination to possess many of the characters of humic or ulmic acid, but they were not subjected to analysis, as there was every reason to believe

that they consisted of a mixture of two or more substances, which had been formed by the action of the alkaline solutions on the incrusting matters of the cells. The proportion of gallic acid in the fruit was estimated by a subsequent operation, which consisted in treating the expressed juice of a given weight of medlars with a per-salt of iron, introducing it into a colorimeter, and contrasting the tint produced with that of a standard solution of gallic acid, to which some of the same reagent had been added, with the necessary precautions. Before adopting this process, however, I should observe, I convinced myself that the juice contained but very slight traces of tannic acid.

The numerical results of my analyses of the unripe and rotten fruit, calculated to the 1000 parts, are subjoined.

	Unripe Fruit.	Ripe or Rotten Fruit.
Epicarp (dried)	17·900	17·200
Stones (dried)	58·710	56·510
Albumen	1·912	1·701
Flocculent matters, containing, in the case of the unripe fruit, a little starch	9·016	6·254
Dextrine and gum	50·659	10·125
Extractive matters	3·930	2·799
Pectine	0·095	21·064
Pectose—*i.e.*, matter convertible into pectine or pectic acid by the action of acids and alkalies, &c.	undetermined; included in the "vegetable fibre."	
Acetic acid	.	a little.
Sugar	12·061	81·899
Citrates of potassa and soda	3·057	3·714
Bimalates of potassa and soda, with a little malate of lime	7·914	8·909
Tannic acid	traces	traces.
Gallic acid	0·213	0·099
Fatty matter and wax(?)	0·354	0·340
Vegetable fibre, cellular tissue, &c.	109·255	52·150
Water and loss	724·924	737·236

Upon comparing the results of these two analyses, it will be seen that the greatest change which takes place in the composition of the fruit during ripening, is in the proportion of the sugar, gum (or more probably dextrine), pectine, and vegetable fibre. In fact, it would appear as if the dextrine and a portion of the cellular tissue were converted by the action of the air into sugar, and pectine or pectic acid. How far this conclusion is borne out by the microscopical examination will be hereafter shewn.

The proportion of water, again, it will be remarked, is also somewhat greater in the rotten fruit. This result differs materially from those obtained by M. Berard, in his examination of the pear, apple,

plum, gooseberry, &c. That chemist found that, in all the fruits which he analysed, the watery portion invariably decreased in quantity during the act of ripening. Thus, whilst unripe cherries contained 88·24 to 90.31% of water, the ripe fruit contained only 74·85 to 80·24%; unripe apricots, peaches, and gooseberries, 90·31, 89·39, and 86·41%; ripe apricots, &c., respectively, 80·24, 74·87, and 81·10% of water.

In order to confirm my result, therefore, I determined to ascertain, by direct experiment, the relative proportion of watery matter contained in the ripe and unripe medlars. Accordingly, a certain number of medlars in both states were separately taken, cut into small pieces, and dried, first in a steam-bath at 212°, then powdered, and finally introduced into a Liebig's drying-tubè, and exposed to a temperature of about 220°, or 230° F., and the loss of weight noted :—

I. 512 grs. of unripe medlars gave 138·695 grs. of dried vegetable matter = water 373·305 grs., or 72·501 per cent.

II. 603·5 grs. of unripe medlars gave 164·889 grs. of dried vegetable matter = 438·611 grs. of water, or 72·678 per cent.

III. 701·2 grs. of ripe or rotten medlars gave 219·16 grs. of dried matters = 514·29 grs. of water, or 73·345 per cent.

ORGANOGRAPHIC EXAMINATION.

A. Of the Unripe Fruit.—When a very thin section of the unripe fruit was treated with strong nitric acid, and then examined under the microscope, it was found that the cells, which in their natural state are pellucid and nearly colorless, became opaque and presented a peculiar yellow granular appearance, as if the contents of the cells had become coagulated by the acid. These granules in a short time disappeared, and the whole of the interior surfaces of the cell-walls were colored of an uniform yellowish-brown, which, by the subsequent addition of an excess of ammonia, was changed to a bright bistre-brown. A moderately strong solution of caustic potassa caused the cells to become loose and transparent, and very similar in appearance to those of the ripe fruit; the interior portion assumed a rich madder-red color, and the intercellular matter became swollen and gelatinous. When the alkaline solution was heated gently over a spirit-lamp, the cellular contents gradually deepened in color to a dark umber-brown, and then dissolved, as did also the gelatinous outside covering, leaving the cell-walls as a thin transparent insoluble film, enclosing a few oily globules.

Concentrated hydrochloric acid behaved towards the cell contents like nitric acid; but the color was much deeper, and the granular character of the coagulum was altogether more distinct.

A strong aqueous solution of iodine had very little action on the cells themselves or their contents, but colored a few starch granules in the interior of a deep blue. When acted upon by iodine and sulphuric acid (3 parts of vitriol to 1 part of water) the cell-walls became of a fine purplish-blue color, which gradually shaded off towards the

intercellular matter; the latter becoming gelatinous as with caustic potassa; whilst the cell-contents were changed to a delicate rosy-brown, but still remained transparent, and did not become opaque and granular as with hydrochloric acid.

B. Of the Ripe Fruit.—On examining the pulpy matter of the ripe fruit, it was found that the cells of which it was composed were loose and unconnected, being readily separated one from another by the slightest pressure. Each of the cells appeared to be surrounded with a thick coating of some gelatinous substance, which was perfectly transparent and colorless, and could be thus easily distinguished from the cells themselves, as the latter had become of a fine rosy-fawn color. A few circular and ovoïd cells were observed, the walls of which were apparently perforated in different places. These were evidently more firm and solid in their structure than the ordinary cells. When treated with nitric acid, the cell contents immediately became of a deep madder-brown color, but still remained translucent, and in this respect differed materially from the cells of the unripe fruit when similarly treated. In a short time a few bubbles of gas were evolved, and the color rapidly changed to a fine orange-yellow, which became deeper on the subsequent addition of ammonia. In the case of the apparently perforated cells before described, however, the reaction was somewhat different; the interior part of each cell, it is true, was turned brown, but the exterior portion still remained colorless and transparent; but in all the cells the outside gelatinous coating continued unaltered in appearance. Hydrochloric acid acted towards the cell-contents like nitric acid, and colored them of a fine reddish-brown, but did not render them granular as it did those of the unripe fruit. Potassa acted towards them as it did on the unripe cells. Iodine had little or no effect upon them, the contents being merely rendered somewhat darker in color. No starch granules whatever could be detected in the interior of the cells. On treating them with concentrated sulphuric acid, they burst, and after a time partly dissolved; but however long the action was allowed to continue, the thin transparent cell-film remained undissolved. The action of the solution of iodine and sulphuric acid produced results similar to those obtained with the unripe fruit.

From these results I have arrived at the following general conclusions:—

I. That the primary-cell walls of the sarcocarp of the medlar are composed of pure cellulose, which, by the action of sulphuric acid, may be converted into starch, or a variety of dextrine, which is colored blue by iodine.

II. That the cells in the unripe fruit are connected together by a peculiar substance, the exact nature of which we are unacquainted with, but which, by the combined action of heat and atmospheric air, is gradually converted into the soluble gelatinous substance, pectine, and pectic acid (?).

III. That this peculiar compound is very similar in character to, and is probably identical with, the substance met with in the turnip and other roots, and which has been termed by Chodnew and others, *pectose*.

IV. That the starch granules which are present in the interior of the cells of the unripe medlar disappear during the process of rotting, and are probably converted either into dextrine or sugar.

V. That the incrusting matters of the cells contain a peculiar nitrogenous substance, which, by the action of nitric acid, is first rendered granular, and is afterwards dissolved and converted into the so-called xanthoproteïc acid.

VI. That this nitrogenous compound, in rotting, is converted into another substance, of an apparently different composition, which is not rendered granular by nitric acid.

VII. That some of the constituents of these incrusting matters are converted by caustic potassa and other alkaline solutions into a peculiar brown substance, evidently analogous in character to ulmine or ulmic acid.

VIII., and lastly. That the principal changes which take place in the medlar during rotting consist in the starch, dextrine, and a portion of the cellular fibre, or intercellular matters, being converted into sugar, whilst the pectose is changed into pectine and pectic acid; these changes being accompanied by the absorption of oxygen from the air, and the formation of carbonic acid and water.

On the Constitution *of* Alums. By Lindsey Blyth.*

The derivation of the term "alumen" is involved in great obscurity; either from the Greek ἁλς *(sal)*, a salt, or from *lumen*, light, on account of its giving a brightness to dye colors; or perhaps it may have had an Egyptian origin, signifying *the earth*, or mineral, *par excellence*, as the best sort came out of Egypt.

Pliny† is one of the oldest writers who uses the term *alumen*. He says, "There are many kinds of alum, the uses of which are very opposite;" and that they were all obtained as natural exudations from the earth. He describes several sorts; one of these as schistos or chalkitis, a mineral, *rock alum*, and observes, that the different kinds of alum are all possessed of an astringent property, whence they derived their Greek appellation στυπτηρία. Beckman‡ maintains, in his history of inventions, that alumen of the ancients signified any vitrolic salt, and says—"The kinds of alum known to the ancients, which were real vitriols, maintained a preference in medicine, and for dyeing black." But the ancients were certainly acquainted with our alum, for we find they used it not only for dyeing, but also for securing wooden buildings and warlike machines against fire.§ Herodotus also

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* Read before the "Chemical Discussion Society," April 13th, 1854.

† Pliny, lib. 35, cap. 15—"De sulphure *alumine* et generibus corum, etc." "In Cypro candidum, et nigrum, exigua coloris differentia, quum sit *usus magna;* quoniam inficiendis *claro colore* lanis candidum liquidumque utilissimum est, contraque fuscis aut obscuris nigrum."

‡ Beckman's "History of Inventions," translated by Johnston, vol. i. page 288.

§ "Gellius," lib. xv., cap. 1.

relates that when the people of Delphos solicited a contribution for rebuilding their temple, which had been burnt, that "Amasis, king of Egypt, sent them a thousand talents worth of alum."*

The Greek word *στυπτηρία*, the equivalent to the Latin word *alumen*, was not only applied to the salt now known as alum, but to any astringent salt, and signifies, *in general*, astringent, thickening, and *especially* of an astringent *taste;* hence our word "styptic."

From this it would appear that these terms denote a quality rather than any particular substance.

At a later period the name "alumen" came to be applied alone to the mineral substance rock-alum, the *alumen rupeum*, or *romanum*, and at last to the article now known in commerce as common alum. That alum was principally used for dyeing purposes, there can be but little doubt, as most early writers mention it in connexion with that subject; but when it was first discovered to be useful in fixing colors does not appear. Although Beckman maintains that the alum of the ancients was a vitriol or a sulphate of iron, copper, or zinc, yet there is no reason for believing that Pliny did not also include the sulphate of alumina under the same general title. That some of the substances mentioned by him as alum consisted of iron, must be admitted, because he says two of them produced a *black* color, with galls; but he also states that another produced *clear* and *lively* colors. Being a natural exudation or mineral, it was not perhaps crystallised like our alum. "According to Berthollet, there is a mine of alum at Solfatara, near Naples, which, in the form of a white earth, contains alum, formed by the action of the sulphurous acid, disengaged by the heat of the volcano upon the argillaceous matter evolved by it. There, the alum requires only to be dissolved out and crystallised. From a mine like this, or its earth, the good effects which Pliny ascribes to the white alum of Cyprus might be readily obtained. The famous alum mines of Tolfa, near Civita-Vecchia, are still purer, according to the accounts of Monnet, Bergman, and Vauquelin, by each of whom they were analysed; but *there* the mineral requires torrefaction."†

In nature crystallised alum is rare; it occurs most frequently as an efflorescence or in silky fibrous masses. It is procured in different ways according the sources whence it is derived. In the Solfatara mines it occurs in a native state, and has but to be dissolved out and crystallised. Other rocks from which it is prepared contain the necessary elements for its formation, and are either roasted, or exposed to the action of the air, when, by lixiviation, the elements of which alum is composed are separated and dissolved out, and it crystallises with great facility from the solution. In other instances argillaceous rocks furnish the sulphate of alumina, and potassa or ammonia is added from other sources, such as the waste liquor of soap or gas works. Besides the native potash alum of Italy, soda alum has been observed in the Isle of

* Herodotus, book ii. (Euterpe) chap. 180—"Αμασὶς μὲν γάρ σφί ἔδωχε χίλια στυπτηρίης ταλαντα."

† "Bancroft on Permanent Colors,"—Introduction.

Milo (called Melos, by Pliny, and one of the sources of his alum), and in South America; and ammonia alum is found in Bohemia.

The common or potash alum of commerce, is a double sulphate of alumina and potassa. It is composed of one equivalent of the sulphate of alumina, with one equivalent of sulphate of potassa, and 24 equivalents of water, and *crystallises in octohedrons*. This salt, to which alone the term alum had come to be applied, was found to be a definite salt having a peculiar constitution; and a number of salts of a similar composition having been observed, it was on this series that Mitscherlich founded his theory of Isomorphism.

A compound may consist of the union of two simple substances as $H + O$ to form water, and $S + O^3$ to form sulphuric acid; or of the union of two compounds of the first order with each other, in which the new combination acts as an elementary substance; the simple salts belong to this order, and amongst them we find the two classes of neutral sulphates, of which KO, SO^3, and $Al^2 O^3$, $3\ SO^3$, may be taken as examples. The term neutral here signifies a *saturated* salt, in contra-distinction to a *basic* or *acid* salt, in which every atom of oxygen in the base requires one atom of a monobasic acid to saturate or neutralise it, and not because they are neutral to the taste or to vegetable colors. The acid and base may be *saturated*, and yet the resulting compound give an acid or alkaline reaction, according as the base or acid may be the most powerful; or neutral, in the ordinary acceptation of the term, when they have about equal power. Other compounds may be formed by the combination of two simple salts to produce double salts, as anhydrous alum or the double sulphate of alumina and potassa $Al^2 O^3$, $3\ SO^3 + KO$, SO^3; and still another order of compound may be formed, in which a double salt is combined with water as an essential part of its composition, as in crystallised alum, $Al^2 O^3$, $3\ SO^3 + 24$ aq. In this formula we may replace the KO, with NH^4O, and we then obtain the ammonia alum instead of potash alum; and again we can substitute NaO, for the ammonia, and we obtain a soda alum, without affecting the form of the crystal or many of its physical properties. All substances which will replace each other in this manner are said to be isomorphous.

The two bases of a real double salt are never isomorphous, or to state the same proposition inversely, two isomorphous substances can never unite to form a true compound. As no other sulphate is isomorphous with those of potassa and ammonia, we cannot replace either of these with the sulphate of any other protoxide, without changing the crystalline form of the compound, although its composition may be similar—unless perhaps by the sulphates of some of the Alkaloïds, of which I shall have occasion to make further mention. On the other hand, we may replace Al^2O^3, with Fe^2O^3, Cr^2O^3, and Mn^2O^3, which are all isomorphous, and with these we obtain salts having the form and the physical properties of alum. These salts may be made by mixing solutions of the sulphate of the sesquioxide of iron, chrome, or manganese, with sulphate of potassa or ammonia in equivalent proportions, taking care to have an excess of acid in the solution, and crystallising

out by spontaneous evaporation—they have the form, composition and taste of common alum. Iron alum has frequently a pale violet tint; chrome alum has a deep ruby tint; and manganese alum a brownish violet color. The soda alums are more difficult of preparation, but as the potassa and ammonia may be replaced by soda in common alum, no doubt, by exercising great care, soda alums may be made with the other bases.

Other salts, having a similar composition, have been described by various authors as alums; but they do not crystallise in the octohedral form, and consequently are not legitimately included in that class. The type of this series may be taken in the double sulphate of alumina and the protoxide of iron, which has the formula Al^2O^3, $3\,SO^3 + Fe\,O$, $SO^3 + 24$ Aq, and crystallises in the prismatic form. Here, as in common alum, either of the terms in the formula may be replaced by an isomorphous substance, the Al^2O^3, by Fe^2O^3, Cr^2O^3, or Mn^2O^3, and the FeO, by MgO, MnO, ZnO, CuO, NiO, CoO, and probably CaO, without changing the crystalline form of the salt; but this series must not be confounded with the alums, which may be determined according to Berzelius—the *genus* by the formula and *crystalline form*; the *species* by the oxidised bases of which they are composed. Thus we have the

General formula . .	M^2O^3, $3\,SO^3 + MO$, $SO^3 + 24$ Aq.
COMMON ALUM.	
With Potassa .	Al^2O^3, $3\,SO^3 + KO$, $SO^3 + 24$ Aq.
„ Soda . .	Al^2O^3, $3\,SO^3 + NaO$, $SO^3 + 24$ Aq.
„ Ammonia .	Al^2O^3, $3\,SO^3 + NH^4O$, $SO^3 + 24$ Aq.
IRON ALUM.	
With Potassa .	Fe^2O^3, $3\,SO^3 + KO$, $SO^3 + 24$ Aq.
„ Ammonia .	Fe^2O^3, $3\,SO^3 + NH^4O$, $SO^3 + 24$ Aq.
CHROME ALUM.	
With Potassa .	Cr^2O^3, $3\,SO^3 + KO$, $SO^3 + 24$ Aq.
„ Ammonia .	Cr^2O^3, $3\,SO + NH^4O$, $SO^3 + 24$ Aq.
MANGANESE ALUM.	
With Potassa .	Mn^2O^3, $3\,SO^3 + KO$, $SO^3 + 24$ Aq.
„ Ammonia .	Mn^2O^3, $3\,SO^3 + NH^4O$, $SO^3 + 24$ Aq.

With regard to the nomenclature of these salts, they derive the generic term "alum" not from their containing any alumina, but because they agree in constitution and form with the salt to which alone the name had come to be latterly applied. This term had originally a general signification, and was applied to many different substances having a common property;—it was adopted by science to designate a series of salts differing in their elementary composition, but possessed of similar properties. Berzelius says,* "The nomenclature of double oxysalts may be described similarly with that of double haloïd salts. Thus alum may be called *double sulphate of alumina and*

* Berzelius, "Traité de Chimie."—Article, *Généralités sur des sels.*

potassa, or *potassio sulphate of alumina*; we may say *double sulphate of protoxide of iron* and *potassa*, or *potassio sulphate of peroxide of iron*. However, when a double salt has a 'trivial' name, generally known, it is employed for brevity's sake. Thus we say *alum* in preference to *potassio sulphate of alumina*." And again,* "The more numerous the elements of a combination become, the more difficult it is to apply the principle of the systematic nomenclature. This difficulty begins to make itself apparent when we come to deal with double salts." He again cites "alum" as an example, and concludes by saying, "After all, in endeavoring to indicate too much by the aid of nomenclature, it is an easy matter to spoil it, because it may be rendered either too complicated or disagreeable to the ear." I make these remarks, because I wish to point out that the systematic names at present in use do not so accurately describe the salts of this series as does the generic term "alum." This latter, a "trivial" name, if adhered to strictly with the above definition, at once defines a salt having a fixed and definite composition; whilst, under the systematic name, salts having the same composition so far as regards their bases and acid, but with different proportions of water and *forms of crystallisation*, may be mistaken for each other. As an example, I may cite *potash iron alum*, of which there can be but one example, and of invariable composition, whereas under the designation of *potassio-sulphate of peroxide of iron*, besides the length of the name, we have not only the alum, but four other combinations of the same elements in different proportions, not two of which have any property in common. These are all described in Gmelin's "Handbook of Chemistry," under the head of "Sulphate of ferric oxide and potassa," or "ferrico-potassic sulphate." Not only, then, is the constitution of an "alum" well defined, but the species are not so numerous as might have been supposed, had not the *form of crystallisation* been admitted into its definition. The so-called alum of iron and quinine, described by Will, is not a true alum. In this salt the quinine replaces the alumina, and combines with the protosulphate of iron, which at once places it among the series that crystallise in the prismatic form. In making experiments on the combinations of the persulphate of iron with several of the organic bases, I think I have obtained some true alums. It is a study that may give us some insight into the constitution of the alkaloïds, and I hope on a future occasion to give you some interesting matter for discussion on that subject.

CHEMICAL TABLES,

Compiled by Thornton J. Herapath.

Notes *on the* Table *of* Densities.

ACETIC ACID.—Specific gravity is not a certain criterion of the strength of acetic acid, when the latter contains more than about 34½ per cent. of water.—*Mollerat.*

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* "Traité de Chimie,"—Article, *Nomenclature.*

The acetometer of the Messrs. Taylor has for its basis the strength of proof acid (Pr. v.,) called by the manufacturer No. 24.

Table, by the Messrs. Taylor, shewing the proportion of real acid per cent. at different densities :—

Pr. V.—1·0085, 5%; 1·017, 10%; 1·0257, 15%; 1·032, 20%; 1·047, 30%; 1·058, 40%.

When acetic acid is converted into acetate of lime, the decimal fraction of the density is very nearly doubled;—thus, 1·009 in pure acetic acid becomes 1·018 in acetate of lime. *(See Vinegar.)*

ÆTHER.—*Table, shewing the specific gravity of mixtures of alcohol and æther; by Dalton.*—Pure ether, 0·72(?); 90 + 10, 0·732; 80 + 20, 0·744; 70 + 30, 0·756; 60 + 40, 0·768; 50 + 50, 0·780; 40 + 60, 0·792; 30 + 70, 0·804; 20 + 80, 0·816; 10 + 90, 0·828; 0 + 100, 0·83 (Spirit).

ALCOHOL.—For tables of the density of various mixtures of alcohol and water, *see* "Brande's Manual," "Ure's Dictionary," and other works.

I am disposed to give the preference in point of accuracy to Drinkwater's results.

ALLOYS.—Alloys which possess a density *greater* than the mean of their constituents :—

Gold with antimony, bismuth, cobalt, tin, or zinc; silver with antimony, bismuth, lead, tin, or zinc; lead with antimony; platinum with molybdenum; palladnum with bismuth; copper with bismuth, palladium, tin, or zinc.

Alloys which possess a density *less* than the mean of their constituents :—

Gold with copper, iron, lead, iridium, nickel, or silver; silver with copper or lead; iron with antimony, bismuth, or lead; tin and antimony, lead, or palladium, nickel, and arsenic; zinc and antimony.—*Thénard.*

AMMONIA.—*See* "Tables," by Ure, Dalton, Davy, &c.

"Ure's Table" is generally considered to be the most correct.

BARLEY.—If not more than five "*pickles*" (or grains) per hundred float, when unmalted barley is thrown into water, it is considered by tradesmen to be a good sample of grain; if more, the contrary. After malting, the grain becomes specifically lighter, and when thrown into water, will float in a longitudinal position; in good malt, only about five pickles per hundred will sink; grain that has been partially malted will remain perpendicularly suspended in the water.

BEER.—The rule employed by Mr. Warington for converting the saccharometric indication (by Richardson's instrument) of the wort into real specific gravity, is as follows :—Divide the saccharometer indication by 360, and then add one. Example—

$$36 \div 360 + 1{\cdot}00 = 1{\cdot}1 \text{ sp. gr.}$$

Richardson's saccharometer indicates by how many pounds a barrel of wort of a certain density is heavier than a barrel of

pure water (viz., 360lbs). To convert saccharometric indication into real gravity, add the number indicated (say x) to 360, and make the following simple calculation :—

$$360 : 360 + x :: 1 : \text{true density.}$$

The scale of Dicas's instrument is nearly as 5 to 2 of Richardson's; *i. e.* 80 by the latter are about equal to 200 by the former.

Allan's saccharometer indicates the true specific gravity.

BLOOD.—The density of the blood, and of the serum of the blood, is increased in plethora, cholera, &c., and diminished during pregnancy, and in pneumonia, bronchitis, pleuritis, tubercular pthisis, fever(?), and chlorosis.

THE EARTH.—The density of the earth is supposed to increase in an arithmetical progression at the rate of about ·38 for every 100 miles, as we penetrate through the exterior crust towards the centre.

GAS.—A diffusion experiment affords the elements for calculating the specific gravity of a gas.

$$\text{The specific gravity} = \left(\frac{A}{G}\right)^2$$

Where G is the measure of gas submitted to diffusion, and A the measure of return air.—*Edinb. Phil. Trans.*, XII., 222.

One hundred cubic inches of dry air, at 29·92 inches Bar., weigh 32·58864 grs. at 32° F., and 30·82926 grs. at 60°F.—*Regnault.*

GAS (COAL AND OIL).—The illuminating power of different specimens of these gases, burned under the same circumstances, is proportional, generally speaking, to their specific gravity, as this is to the quantity of carbon they hold in combination.—*Ure.*

GLASS.—The density of annealed glass is greater than that of the unannealed. The phenomena of devitrification are always accompanied by an increase in density; thus, soda glass being = 2·485, the devetrified glass is = 2·503.—*Splittgerber.*

HYDROCHLORIC ACID.—*See* tables by Davy, Thomson, Ure, Kirwan, and Dalton. Ure's results are, I believe, the most accurate.

HYDROCYANIC ACID.—0·957, 16⅞; 0·9768, 10·6⅞; 0·9815, 9·1⅞; 0·984, 8⅞; 0·987, 7·3⅞; 0·989, 6·4⅞; 0·99, 5·8⅞; 0·9923, 5·0⅞; 0·994, 4⅞; 0·9958, 3⅞; 0·9974, 2⅞; 0·9979, 1·6⅞.—*Dr. Ure.*

MAGNESIA (SULPHATE OF).—For a table, by Anthon, *see* Gmelin's Handbook (English trans.)

NITRIC ACID.—Tables by Ure, Thomson, Kirwan, and Dalton, are contained in most of the manuals. Ure's table appears to be the most correct.

OILS (FIXED).—The zero of the scale in the so-called Elaëometer is placed where the instrument would float in pure oil of poppy seeds; the space between this point and that which marks the

density of pure olive oil is generally divided into 50 degrees. The density of pure almond oil is about equal to 38 or 38½ degrees by this scale. The Oleometer of M. Laurot is so graduated as to sink to zero in pure colza oil (heated to 212° F.); to 210° in rapeseed oil; to 124° in poppy oil; to 83° in fish oil; and to 136° in oil of hempseed.

PHOSPHORIC ACID.—Sp. gr. 1·85, 50% anhyd. acid; 1·6, 40%; 1·39, 30%; 1·23, 20%; 1·1, 10%.—*Dalton.*

POTASH.—For tables shewing the proportion of caustic potash and hydrate of potash contained in leys of different strength, *see* the works of Ure, Brande, Gmelin, &c. Ure's table appears to be the most accurate.

POTASH (CARBONATE OF).—For table, by Tünnermann, *see* works of Brande, Gmelin, &c.

POTASH (CHROMATE OF).—S.G. 1·28, 50%; 1·21, 33%; 1·18, 25%; 1·15, 20%; 1·12, 16%; 1·11, 14%: 1·1, 12%.—*Dr. Ure.*

PYROXYLIC SPIRIT.—For table, *see* Ure's Dictionary of Arts, &c.

ROOT-CROPS.—The relative densities of different roots or tubers, are stated by Mr. Hyett to be as follows:—

Turnips:—Pomeranian globe, 889; Green round, 905; Border imperial, 932;—average, 908·57.

Swedes:—Purple-topped, 949; Green-topped, 952; Skirving's, 972;—average, 957·67.

Mangold-wurzel:—Long red, 995·25; Red-globe, 1005·7; Yellow globe, 1014·6;—average, 1005·18.

Potatoes.—Rohan, 1081 to 1089·95; Purples, 1091 to 1102; Welsh-kidney, 1107 to 1108;—average, 1096·49.

The specific gravity of small roots appears generally to exceed that of large roots. Mr. Hyett says, "That the determination of the densities of root-crops affords an easy though rough method of estimating their relative powers of nourishment." (?)

SODA.—For tables, by Richter and Tünnermann, of the proportion of hydrate of soda and anhydrous soda contained in leys of different densities, *see* the works of Gmelin, Brande, Ure, &c.

The following table is by Dalton, and shews the per centage of NaO.

Sp. G. 1·85, 63·6%; 1·72, 53·8%; 1·63, 46·6%; 1·56, 41·2%; 1·5, 36·8%; 1·47, 34%; 1·44, 31%; 1·40, 29%; 1·36, 26%; 1·32, 23%; 1·29, 19%; 1·23, 16%; 1·18, 13%; 1·12, 9%; 1·06, 4·7%.

SODA (CARBONATE OF).—For table, *see* Gmelin's Handbook.

SODA (NITRATE OF.)—*See* Richter's tables; Stöichiometrie 3,164.

SODA (SULPHATE OF).—*See* Brandes and Gruner. Br. Arch. 22·148.

SODIUM, (CHLORIDE OF.)—The proportion of salt in solutions of different densities, at 62° F., is shewn below.

S. G. 1·0283, 1–24th; 1·0275, 1–25th; 1·027, 1·26th; 1·0267, 1–27th; 1·025, 1–28th; 1·0233, 1–30th; 1·0185, 1-39th;

1·0133, 1–44th; 1·0105, 1–56th; 1·004, 1–108th; 1·0023, 1-162nd.

[Calculated by Mr. Kirwan, from Watson's results.]

SUGAR.—According to Dr. Ure, if the decimal part of the number representing the specific gravity of syrup, be multiplied by 26, the product will denote very nearly the quantity of sugar per gallon in pounds weight, at the given sp. gr.

Table, by Dr. Ure, shewing the per centage of sugar in saccharine solutions of different densities :—

Sp. Gr. 1·326, 66·666⅔ sugar; 1·231, 50⅞; 1·1777, 40⅞; 1·144, 33·333⅓; 1·134, 31·25°; 1·125, 29·412⅞; 1·111, 26,316⅞; 1·1045, 25⅞; 1·0905, 21·74⅛; 1·082, 20⅞; 1·0635, 16,666⅔; 1·05, 12·5⅞; 1·0395, 10⅞.

For more extensive tables, *see* the different chemical manuals; also, Evan's, Porter's, and Dutrone's works on the Sugar Manufacture.

At 1·342, syrup of the cane contains 70⅞ of sugar; at the same density syrup of starch sugar contains 75½ per cent.—*Ure.*

For a table of the densities of alcoholic solutions of sugar, *see* Ure's Dictionary of Arts, Manufactures, &c.

SULPHURIC ACID.—For tables, by Parkes, Ure, Bineau, &c., *see* Ure's Dictionary, Gmelin's Handbook, Parnell's Applied Chemistry, and other manuals.

Bineau's table is said to be the most correct, but as the results are calculated to 32° F., Ure's table is the one most commonly resorted to.

URINE.—To determine the proportion of solid matters in urine, multiply the difference between the density of the urine and that of water by 2·58 (Dr. Henry), or by 2·33 (according to Christison), the product shews the quantity of solid matters in 1000 grs. of urine. Thus, given a urine of 1·004 sp. gr.: 1004—1000 = 4, which, multiplied by 2·58, gives 10·32 as the weight in grains of solids in 1000 grs. of urine. According to Becquerel, the proportion of sugar in a specimen of diabetic urine may be ascertained by using the factor 1·65 as above. Millon says, that "the second and third figures after the point (in the density) express with tolerable exactness the quantity of urea in 1000 parts of the urine. Thus, urine of 1·011 contains about 11 parts in 1000 of urea. In the urine of animals and pathological urines, however, M. Millon admits that this correspondence is no longer observed. The density of the urine is generally below the normal standard in *Diabetes insipidus* (1·003 to 1·005—Percy), hysteria, Bright's disease (?), chlorosis, and jaundice (?); and above, in *Diabetes mellitus* (1·032 to 1·048), erysipelas, fever, bronchytis, hæmoptysis, and other inflammatory diseases. It is generally higher in the afternoon (1·023 to 1·028) than in the forenoon (1·017 to 1·022), evening (1·019 to 1·028), or night (1·012 to 1·025)

VINEGAR.—Vinegar of malt, of a density of 1·014, becomes only 1·023 when converted into acetate of lime; ·005 of its density is due to mucilaginous matter.—*Ure.*

WATER.—The temperature at which pure water attains its greatest density, according to different observers, is as follows :—

38° F., *Dalton;* 38·75°, *Stampfer;* 38·8°, *Rumford;* 38,804°, *Muncke;* 39°, *Sir C. Blagden—Gilpin;* 39·101, *Playfair* and *Joule;* 39·176, *Despretz;* 39·38, *Hällstrom;* 39·5, *Hope;* 40°, *Lefebvre Gineau;* 41°, *Deluc.*

Despretz's determination is generally believed to be the most correct.

Sea-water (from the Southern Ocean), according to Bladh, attains its maximum density at 36·59 F.

WOODS.—The densities given in the table have reference to the dry and well-seasoned woods, in their natural condition.

The true specific gravities of these woods (*i. e.* when the air in the interstices of the wood has been expelled) are, according to Count Rumford, as follows :—

Birch, 1·4848; beech, 1·5284; elm, 1·5186; fir, 1·4621; lime, 1·4846; maple, 1·4599; oak, 1·5344; poplar, 1,4854.

On the Production *of* Waxed Paper Negatives.*

By James How.

Almost every one interesting himself as an amateur in the delightful art of photography, complains of the want of simple and practical directions in the works published for his guidance.

The complaint is not more universal than just. The books published are generally defective in the clearness of detail so essential to their utility. I have often found the most precise observance of the directions given fail to give the intended result; so many different methods are jumbled together, the reader being left to choose the best for himself; the consequence is, that many become discouraged at the outset, not having the opportunity of taking practical instructions, nor time nor inclination to wade through such a sea of experiments, before arriving at results sufficiently satisfactory to reward them for their pains, and to stimulate them to further application. It is in consequence of the above complaint having been so frequently made to me, and some of my friends wishing me to endeavor to remove its cause, that I have taken the opportunity, which this valuable Society affords of laying before the lovers of this fascinating art, a few practical hints, which, while of the greatest possible simplicity, will, I hope, add some important facts to our present stock of photographic knowledge.

In the present paper, I propose to confine myself to the waxed paper

* Read before the Chemical Discussion Society, May 11th, 1854.

process; which, although not *at present* in general use, will, I feel confident, eventually, when brought to perfection, and properly appreciated, be universally used for views, while collodion will be as universally employed for portraits. Of the latter, I shall not upon the present occasion speak, but hope at some future time to lay before you the results of some very interesting experiments, which I think promise great things upon this head.

I hold this opinion of the great value of the waxed paper process for the two following reasons:—First, it is obvious, that the thinner the paper used for negatives, the sharper the picture will be in the positive when printed from. In the talbotype process very thin paper cannot be used, since it would be too weak to allow the many and frequent washings necessary, but this process of waxing imparts a horny character to the paper, making the thinnest very strong, without increasing its thickness; it may then be washed, soaked, and re-washed for even several days if required, without the slightest fear of injury. And secondly, the paper so prepared can be kept after it is made sensitive and ready for the camera, without deterioration for from ten to fourteen days; while that prepared by the talbotype process can only be kept for about as many hours. This property in a climate so varying as ours, must be of infinite importance, when considered in point of convenience as well as of economy. One great objection which has been urged against the use of waxed paper up to the present time, has been the fact of its requiring so long an exposure in the camera. After repeated experiments, I have found that when treated in the way I shall presently describe, this objection is entirely removed. Twenty minutes and even half an hour have generally been required for obtaining the impressions, but I have lately succeeded with a 3-in. Voigtlander lens of 16-inch focus, in obtaining first rate negatives with perfectly black skies, which require no painting out, and the whites quite clear, the half tones also quite perfect, with five minutes exposure in the camera. It therefore appears to me quite certain that no process will be able to obtain precedence of this, which holds out such claims to superiority, from the beauty and sharpness of its pictures, the very valuable non-deteriorating quality of the excited paper; considered in connexion with the fact of so short a time comparatively of exposure in the camera, being required to complete the impression.

I have already had the honor of reading a paper before this Society on the preparation of the waxed paper, and must refer for a report of it to Notes and Queries for January 22nd, 1853; but as I have since then made several important modifications in my process, I must beg to recapitulate some of my previous details.

The wax generally sold as white wax in small round cakes, should not be used, as it is found to contain a large proportion of stearine and tallow, both of which are injurious in the preparation of the paper, but pure wax, to be had only in blocks, which is really pure and not white, but a brownish yellow, an article well known in the drug trade. To wax the paper, I directed to take an iron (I prefer a box iron) moderately hot, in the one hand, and to pass

it over the paper from side to side, following closely after it with a piece of white wax, held in the other hand, until the whole surface has been covered. By thus heating the paper, it readily imbibes the wax, and becomes rapidly saturated with it. The first sheet being finished, I place two more sheets of plain paper upon it, and repeat the operation upon the upper one (the intermediate piece serving to absorb any excess of wax that may remain), and so on, sheet after sheet, until the number required is waxed.

The sheets, which now form a compact mass, are separated by passing the iron, moderately heated, over them, then placed between folds of bibulous paper, and submitted to a further application of heat by the means just described, so as to remove all the superfluous wax from the surface, and render them perfectly transparent—most essential points to be attended to in order to obtain fine negative proofs.

I will now endeavor to describe the method of preparing the iodising solution.

Instead of being at the trouble of boiling rice, preparing isinglass, adding sugar of milk, and the whites of eggs, &c., I simply take some milk quite fresh (say that milked the same day), and add to it, drop by drop, glacial acetic acid, in about the proportion of one, or one and a half drachm, fluid measure to the quart, which will separate the caseïne, keeping the mixture well stirred with a glass rod all the time; I then boil it in a porcelain vessel, to throw down the remaining caseïne not previously coagulated, and also to drive off as much as possible of the superfluous acid it may contain. Of course any other acid would precipitate the caseïne; still I give the preference to the acetic from the fact that it does not affect the after process of rendering the paper sensitive, that acid entering into the composition of the sensitive solution.

After boiling for five or ten minutes, the liquid should be allowed to cool, and then be strained through a hair sieve or a piece of muslin, to collect the caseïne: when quite cold, the chemicals are to be added.

I would, however, make a remark before proceeding further, that some difficulty is sometimes found in making this iodising solution from the milk, in consequence of its not filtering perfectly clear, which I believe is owing to the difference of quality found in London milk; however, this difficulty may be removed by adding to the strained liquid, as soon as the caseïne is separated, the whites of two or three eggs to the quart; again boil, and on cooling the liquid will be perfectly clear.

The proportions of chemicals I now use are as follows:—

385 grs. iodide potassium.
60 grs. bromide „
30 grs. cyanide „
20 grs. fluoride „
10 grs. chloride of sodium in crystals.
1½ grs. iodine.
2 oz. alcohol sp. gr. 836.

The above are dissolved in thirty-three fluid ounces of the strained liquid, and after filtration through white bibulous paper, should be perfectly clear, and of a bright lemon color.

The iodising solution is now ready for use, and must be preserved in well stoppered bottles, when it will keep for any length of time.

The waxed paper is laid, sheet by sheet, in the solution, in a flat porcelain or gutta percha dish, and allowed to remain there for the space of from half an hour to an hour, according to the thickness of the paper. Several sheets may be immersed at once—say a dozen or eighteen; the whole should then be turned over, so that the first sheet immersed may be the first taken out and hung up to dry; thus far the whole may be carried on by day-light. A small piece of bibulous paper, about one or two inches square, should be attached to the lower corner to facilitate the drying. The paper in this state may be kept for any length of time without injury, if preserved from the action of the air in a portfolio.

To excite the paper, or render it sensitive, take:—

Crystallised nitrate silver, 35 grs.
Acetic acid* (glacial), 40 min.
Alcohol, sp. gr. 836, 1 dr.
Distilled water, 1 oz. Mix.

As soon as dissolved, put this solution of aceto-nitrate of silver into a glass or porcelain dish (glass should be preferred), then take up one of the sheets of iodised paper by the two ends, and bend it into the shape of a horse shoe, gently drop the centre on the surface of the solution, and gradually lower both ends. By this means all air-bubbles will be excluded; if any should, however, remain, they will be seen through the transparent paper, and may be easily expelled by passing the finger-nail, or any convenient instrument, over them, after having carefully raised up the corner of the paper nearest to the bubble.

The dish should now be gently tilted, to allow the greater portion of the liquid to run to one side, and by means of a pair of horn tongs, the edge of the paper nearest to the deep fluid should be pushed under it; by treating each edge in this way as quickly as possible, and having got all the edges under, then by gently shaking the whole, the entire surface will become immersed, and by constantly agitating the dish containing the solution and the paper, a very nice even coating may be obtained. This method is found far preferable to merely laying the sheet upon the surface, since it is made sensitive in much less time. The paper should be left in this dish for about five minutes, or, in other words, for two minutes after the brown, iodised paper appears by the light of a candle to have been turned white, though by day-light it would be found to be a very beautiful canary-yellow.

To economise the solution, the sheet may now be held up by one or

* It is of the greatest importance that the glacial acetic acid should be perfectly pure, otherwise the whites cannot be obtained perfect, and the whole result will be unsatisfactory; the cheap acid frequently sold under this name is nothing of the kind, and is constantly the cause of disappointment.

two of the corners to drain into the dish for a few seconds, and then plunged into a dish (of dimensions corresponding to the size of the sheet) of distilled water; a second sheet may now be taken, and the whole process of immersion in the exciting liquid commenced with it; as soon as this is thoroughly covered with the solution, and has been agitated as before described, it may remain while the first sheet is being turned over and over in the water, and then transferred to a second dish containing distilled water, in which it is again well washed and afterwards hung up to drain. It should then be placed on a clean white sheet of bibulous paper of a corresponding size, to absorb the moisture, and another sheet of the same bibulous paper laid upon it; by this time the second sheet of excited paper will be ready for treatment in the distilled water in the same manner as the first. Thus any desired number of sheets may be excited.

They should now be placed between fresh pieces of clean white bibulous paper, and kept under pressure, to exclude light and air until required for use.

Paper prepared in this manner will keep perfectly good and equally sensitive for a very long time,* a desideratum of, I think, no small value to the tourist, who knows not how often, nor under what circumstances, an opportunity may occur to him for a sketch; of course, the exciting part of the process must be performed only by the light of a candle, or in a room with a yellow blind drawn over the window, which, while travelling in the country, he should always be provided with, and which will also serve for a focusing cloth,—common yellow callico doubled answering perfectly well for the purpose.

Some may consider the use of so much bibulous paper superfluous or even extravagant, but absolute cleanliness is essential to success, and it would be far better to waste even a dozen pieces of bibulous paper than run the risk of spoiling a single negative picture, to obtain which perhaps many times the cost of the paper has been expended in travelling, or otherwise.

To place the paper in the camera slide, the double paper-frames should be laid open on a table, the glasses in each having been well cleaned; upon one side of one of the glasses a sheet of the sensitive paper, placed with its marked side downwards, or towards the glass, for the purpose of receiving the image; upon the back of this lay a clean piece of dry bibulous paper, previously cut to the size of the frame, then a piece of yellow paper or opaque millboard, sufficiently thick to prevent the light passing through. Again, lay a piece of bibulous paper upon this, and, lastly, a piece of the sensitive paper with its marked sensitive surface upwards. The whole of this will, of course, also be performed in a dark room, or by the light of a candle. After the frame is closed the whole is ready for exposure in the camera. To regulate the time of exposure in the camera, depending

* I have kept this paper for as long as six weeks in this country, and then obtained good results; of course, in India, or other warm climates, it would not keep quite so long.

as it does on the focal length of the lens employed, and the amount of light admitted through the diaphragm, must necessarily be left chiefly to the discretion of the operator—a hint or two, however, may not be unacceptable. The size of the diaphragm should vary with the size and distance of the object to be taken. If the object be a single building, and at such a distance as to fill the field of view of the camera, when a 3-inch or No. 2 Voigtlander lens is employed, and its largest opening of $\frac{7}{8}$-inch diameter be used, which is sufficiently small, three to five minutes' exposure will be found sufficient. For more distant objects, it is advisable to use the smaller diaphragm or stop, which is about $\frac{1}{4}$-inch less in diameter, and to increase the length of time of exposure by two or three minutes, as the same surface has to be impressed and less light to do it with. A few experiments will give a much better notion about the working time of different lenses than could possibly be imparted by many pages of written instructions, so much depending upon the color of the glass employed in the manufacture of the lenses, which should be perfectly white. This may be ascertained by laying the lense upon a sheet of white paper, and judging from the difference of color between the paper through the glass and that part not covered.

The picture may be developed with gallic acid immediately after its removal from the camera; or, if more convenient, that part of the process may be delayed for several days. Whilst at this section of my paper, I may, perhaps, be allowed to describe a method of preparing the solution of gallic acid, whereby it may be kept in a good state of preservation, for several months. I have kept it myself for four months, and have found it, after the lapse of that period, infinitely superior to the newly made solution. This process has, I am informed, been alluded to in photographic circles; but not having seen it in print, and presuming the fact to be one of great practical importance, I trust I shall be excused for introducing it here should it not possess that degree of novelty I attribute to it.

What is generally termed a saturated solution of gallic acid is, I am led to believe, nothing of the kind. In all the works on photography, the directions given run generally as follow:—'Put an excess of gallic acid into distilled water, shake the mixture for about five minutes, allow it to deposit, and then pour off the supernatant fluid, which is found to be a saturated solution of the acid.'

Now I have found by constant experiment, that by keeping an excess of acid in water for several days, the strength of the solution is greatly increased, and its action as a developing agent materially improved. The method I have adopted is to put half an ounce of crystallised gallic acid into a stoppered quart bottle, and then to fill it up with water so that, when the stopper is inserted, a little of the water is displaced, and, consequently, every particle of air excluded.

The solution thus prepared will keep for several months. When a portion of it is required, the bottle should be refilled with fresh distilled water, the same care being taken to exclude every portion of

atmospheric air,—to the presence of which, I am led to believe, is due the decomposition of the ordinary solution of gallic acid.

Of this saturated solution of gallic acid, put into a dish about sufficient to cover the sheet, lay the face of the undeveloped picture downwards in the solution, taking care that no air bubbles are allowed to be present, completely cover this sheet with the liquid, by agitating the dish, &c., and then lay another sheet requiring development with its back on this, or face upwards; again agitate the dish with its contents, so as to entirely cover the whole of the upper surface, and allow the sheets to remain until the pictures partially shew themselves. Now pour the solution of gallic acid from the dish into a glass measure, or other convenient vessel, taking care that it is quite clean, and free from any hyposulphite of soda, and add of the aceto-nitrate of silver (the solution left from exciting the paper) in the proportion of from 5 to 10 drops to the ounce, add also one or two drachms of alcohol; stir these with a glass rod and return them to the dish containing the negatives for their complete development. They should now and then be turned over, that is, the picture which is now at the bottom should be at the top, and so changed two or three times. It is advisable to adopt this means of adding the aceto-nitrate of silver, for if poured directly into the dish in which the negatives are, before being mixed with the gallic acid solution, a black deposit would be formed upon that part of the dish where it was poured, the picture would also be probably spoiled by its having a marbled appearance in some parts. The solution may sometimes have a tendency to turn cloudy or black, in which case it should be changed for fresh, should the development not be completed; the negative should be allowed to remain in this solution, till, by holding it by the corners to the light of a candle, the sky appears perfectly black, when it should be taken out and rinsed in a little clean common water, to wash off the excess of acid. The picture is now fixed by plunging it into a solution of hyposulphite of soda in common water, in the proportion of 1 oz. of the former to 8 ozs. of the latter. It should be allowed to remain in this solution for from thirty minutes to one hour, or until the yellow parts appear perfectly white and transparent, but certainly not so long as to injure the blacks, which would be the consequence of leaving it too long.

Should some parts of the negative appear of a red color instead of white, and this is sometimes the case from over exposure in the camera, they may be recovered by immersing the whole in a weak solution of cyanide of potassium of 5 or 6 grs. to the ounce of water; by over immersion in this solution the blacks are liable to injury, in which case it is necessary that they be replaced with Indian ink or lamp-black, and prepared ox-gall sold for the purpose (the ox-gall serves only as a vehicle for the ink or lamp-black, and will cause either of the above substances readily to adhere to the waxed paper).

After the fixing is satisfactorily performed, each negative should be allowed to soak for several hours in common water, which should be changed several times, in order thoroughly to dissolve out the hypo-

sulphite of soda; unless this be done the picture will eventually be entirely destroyed by the action of this salt.

When sufficiently soaked, the negative should be attached by means of a pin by one of its corners to a shelf or other convenient place; its drying being facilitated by attaching a small strip of bibulous paper to its other extremity.

When perfectly dry, the negative should be held before the fire to remelt the wax it contains, or (which is preferable) placed between two sheets of blotting-paper, and a hot iron passed over it, to diffuse and equalise the wax.

The negative thus finished is ready for printing from; a process to which I hope to draw your attention on a future occasion.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

On the COMBINATIONS *of* GLYCERINE *with the* ACIDS.

By M. BERTHELOT.

THE results which I have obtained relate:—

I. To the study of various glycerine compounds of the highest order;

II. To various hydrochlorates of glycerine;

III. To the reaction of this body on oxalic acid;

IV. To a peculiar combination of glycerine and alcohol.

I shall conclude the statement of these results with some considerations on the constitution of the compounds of glycerine.

I.—I have prepared the following bodies:—

1st.—*Trioleïne*, $C^{114}\ H^{104}\ O^{12} = 3C^{36}\ H^{34}\ O^4 + C^6\ H^8\ O^6$—6HO.—This body is liquid and neutral. Treated with oxide of lead at 212° F., it slowly and difficultly decomposes into oleïc acid and glycerine. Trioleïne is identical with natural oleïne. It possesses the composition and properties of oleïne, as analysed by M. Chevreul.

2nd.—*Trivalerine*, $C^{36}\ H^{32}\ O^{12} = 3C^{10}\ H^{10}\ O^4 + C^6\ H^8\ O^6$—6HO.—This is an oily neutral liquid, with a disagreeable odor, insoluble in water, soluble in alcohol and ether, resolvable into glycerine and valerianic acid.

3rd.—*Tributyrine*, $C^{30}\ H^{28}\ O^{12} = 3C^8\ H^8\ O^4 + C^6\ H^8\ O^6$—6HO—This is an oily, odorous, neutral liquid, of a density equal to 1·056, decomposable into glycerine and butyric acid.

4th.—*Tribenzoïcine*, $C^{48}\ H^{20}\ O^{12} = 3C^{14}\ H^6\ O^4 + C^6\ H^8\ O^6$—6HO.—This body is neutral; when purified it crystallises in beautiful white needles, more voluminous than those of any other compound of glycerine.

5th.—*Triacetine*, $C^{18}\ H^{14}\ O^{12} = 3C^4\ H^4\ O^4 + C^6\ H^8\ O^6$—6HO.—This is an odorous, neutral liquid, of a density equal to 1·174, insoluble in water, but very soluble in dilute alcohol. It contains: C = 49·9;

H = 6·8. The formula indicates C = 49·6 ; H = 6·4. Triacetine is resolved by saponification into acetic acid and glycerine. It gave :—

Acetic acid	80·6
Glycerine	43·1
	123·7

The formula indicates :—

Acetic acid	82·6
Glycerine	42·2
	124·8

The analytical results of the study of these bodies, and especially the analysis and saponification of triacetine, whose equivalent is the lowest, have led me to modify the formula usually admitted for natural stearine.*

According to these results, if we admit that the glyceric compounds of the highest order all possess a similar formula, natural stearine, as well as the artificial compound which is identical with it, should be regarded as a *tristearine* :—

$$C^{114}\ H^{110}\ O^{12} = 3C^{36}\ H^{36}\ O^4C^6 + H^8\ O^6 - 6HO.$$

This formula agrees with the analysis and facts already known. The same observation applies, with the same reservation, to natural margarine and palmitine. These substances, as well as the artificial bodies which are identical with them, appear to me to be *trimargarine* and *tripalmitine*.

II.—1st.—Besides the bodies of which I have just spoken, I have prepared a new neutral combination formed by hydrochloric acid and glycerine.

Dichlorhydrine, $C^6\ H^6\ Cl^2\ O^2 = 2HCl + C^6\ H^8\ O^6 - 4HO$.—Dichlorhydrine is a limpid, neutral oil, insoluble in water, with a powerful ethereal odor ; its density is equal to 1·37 ; potassa slowly decomposes it, reproducing glycerine and hydrochloric acid.†

2nd. I subjected to a special examination the combinations formed by glycerine and the acids with the addition of hydrochloric acid. According to the analysis, properties, and the relatively low temperature at which they distil, these combinations appear to be constituted not by a mixture of simple compounds, but by definite complex bodies, into which hydrochloric acid enters with the other acid, neutralised by the glycerine. Supposing this to be the case, a glyceric compound may contain several different acids, in the same manner as several equivalents of the same acid. One of these combinations,

* This is the formula of M. Lecanu and of Berzelius, calculated with the actual equivalent of stearic acid. The formula of MM. Pelonze and Liebig only differs in 2 equivalents of water.

† The existence and nature of this compound have led me to make fresh researches on acetidine and butyridine. According to my new experiments and saponifications, acetidine is a *diacetine*, $C^{14}\ H^{12}\ O^{10} = 2C^4\ H^4\ O^4 + C^6\ H^8\ O^6 - 4HO$, and butyridine, a *dibutyrine*.

benzochlorhydrine, answers, according to analysis, to the definite formula, $C^{26}H^{11}ClO^6 = C^{14}H^8O^4 + HCl + C^6H^3O^6$—4HO.

III.—Oxalic acid, heated to 212° F., with glycerine, is resolved into pure carbonic acid, which is disengaged, and into formic acid, which remains with the glycerine, without however forming a neutral combination. In the presence of an excess of glycerine, the decomposition is complete at the end of twenty-seven hours. This curious reaction agrees with the production, so frequently observed, of formic acid at the expense of oxalic acid; but I do not believe that this phenomenon has ever so clearly shewn the pure and simple characters of a decomposition:—

$$C^4H^2O^8 = C^2O^4 + C^2H^2O^4.$$

IV.—I have obtained between glycerine and alcohol a combination analogous to the conjugate ethers discovered by Mr. Williamson:—

Diathyline, $C^{12}H^{16}O^6 = 2C^4H^5Br + C^6H^4O^6$—2HBr—

is prepared by heating glycerine, hydrobromic ether and potassa in excess to 212° F. in closed vessels for sixteen hours.* After the experiment, the liquid in the tubes forms two layers. The upper layer is decanted and distilled. At 375° F. diathyline distils over. It is a limpid colorless oil, very moveable, almost if not quite insoluble in water, having a slight ethereal and peppery odor; its density is equal to 0·92. If some drops of diathyline are poured on incandescent chalk, acroleïne appears to be formed; distilled with a mixture of sulphuric and butyric acids, diathyline gives butyric ether.

General Conclusions. 1.—According to the preceding facts, and those which I have before published, the artificial combinations of glycerine studied by me are neutral bodies formed by the union, in equivalent proportions of acids and glycerine.

In this union produced with the elimination of the elements of water, the properties of the acid become latent. These bodies, treated with potassa, reproduce a neutral salt, with an equivalent formation of one body, glycerine in all. At the same time, the elements of water become fixed and the properties of the acid reappear. These same phenomena of decomposition manifest themselves in the most varied circumstances, and often with the most trifling influences.

These conditions, these phenomena, and these products, are precisely the same as those which accompany the decomposition of the natural fatty bodies, as shewn by M. Chevreul's works forty years ago.

2. These facts establish, as he remarked, an approximation between the fatty bodies and the ethers. On the one hand, the ethers, like the fatty bodies, are formed by the direct or indirect union of an acid with alcohol: this union takes place with a separation of the elements of water and the disappearance of the properties of the acid. On the other hand, the neutral compounds thus formed, reproduce, by the most varied processes, the acid and alcohol by fixing the elements of water. The action of the alkalies, that of the concentrated acids, that of water, whether quickly at 428° F. or slowly at the ordinary

* I have prepared ethylmethylic ether in an analogous manner with potassa.

temperature, equally decompose the neutral fatty bodies into acids and glycerine, the ethers into acids and alcohol.

With both, the action of ammonia produces the amides. Moreover, the equivalence of glycerine and alcohol with respect to acids may be demonstrated by direct and reciprocal reactions. We can at will either decompose certain ethers with glycerine and produce a glyceric compound, or decompose a fatty body with alcohol and produce an ether.

These facts concur to prove, independently of hypothesis, the analogy of constitution existing between the glyceric compounds and the ethers.

a.—Nevertheless, if glycerine resembles alcohol in the nature of the compounds to which acids give rise, the formula of these same compounds, the existence of many neutral combinations between glycerine and one acid, establish a great difference between glycerine and alcohol.

In fact, the neutral glyceric compounds belong to three distinct series:—

The first series, analogous to the ethers, even its formulæ, is formed by the union of 1 equivalent of acid and 1 equivalent of glycerine, with the separation of 2 equivalents of water.

Monostearine, $C^{42}H^{42}O^{6} = C^{36}H^{36}O^{4} + C^{6}H^{8}O^{6}$—2HO.
Monochlorhydrine, $C^{6}H^{7}ClO^{4} = HCl + C^{6}H^{8}O^{6}$—2HO.

The second series is formed by the union of 2 equivalents of acid and 1 equivalent of glycerine, with the separation, sometimes of 2, sometimes of 4 equivalents of water.*

Distearine, $C^{78}H^{78}O^{12} = 2C^{36}H^{36}O^{4} + C^{6}H^{8}O^{6}$—2HO.
Dibutyrine, $C^{22}H^{22}O^{12} = 2C^{8}H^{8}O^{4} + C^{6}H^{8}O^{6}$—2HO.
Diacetine, $C^{14}H^{12}O^{10} = 2C^{4}H^{4}O^{4} + C^{6}H^{8}O^{6}$—4HO.
Benzochlorhydrine, $C^{20}H^{11}ClO^{6} = C^{14}H^{6}O^{4} + HCl + C^{6}H^{8}O^{6}$—4HO.
Diathyline, $C^{14}H^{6}O^{6} = 2C^{4}H^{6}O^{3} + C^{6}H^{8}O^{6}$—4HO.
Dichlorhydrine, $C^{6}H^{6}Cl^{2}O^{2} = 2HCl + C^{6}H^{8}O^{6}$—4HO.

The third series results from the union of 3 equivs. of acid and 1 equiv. of glycerine, with the separation of 6 equivs. of water:—

Tristearine . $C^{114}H^{110}O^{12} = 3\ C^{36}H^{36}O^{4} + C^{6}H^{8}O^{6}$—6 HO.
Trioleïne . $C^{114}H^{104}O^{12} = 3\ C^{36}H^{34}O^{4} + C^{6}H^{8}O^{6}$—6HO.
Triacetine . $C^{18}H^{14}O^{12} = 3\ C^{4}H^{4}O^{4} + C^{6}H^{8}O^{6}$—6 HO.

The natural fatty bodies appear to belong to this last series. In the decomposition of the bodies of this series, 3KO replaces, with respect to the anhydrous acid, the remaining $C^{6}H^{5}O^{6}$, in which the oxygen is the third part of the oxygen of the anhydrous acid, which is the same proportion as in the neutral salts.

These facts shew us that glycerine exhibits, with respect to alcohol, precisely the same relation that phosphoric acid exhibits with respect to nitric acid. In fact, nitric acid only produces one series of neutral salts, whereas phosphoric acid gives rise to three distinct series of neutral salts; the common phosphates, the pyrophosphates and the metaphos-

* Perhaps the quantity of water eliminated may be always the same.

phates. These three series of salts, decomposed by a powerful acid in the presence of water, reproduce always the same phosphoric acid. In the same manner, whereas alcohol only forms one series of neutral ethers, glycerine gives rise to three distinct series of neutral compounds; these three series, by their decomposition in the presence of water, reproduce always the same body, glycerine.

Glycerine is not the only body which partakes of the property of alcohol of forming neutral and stable combinations by its union with acids. I have likewise found this property, nearly in the same degree, in mannite; I have already obtained with this body and stearic acid, *stearite;* with palmitic acid, *palmitite;* with butyric acid, *butyrite;* with acetic acid, *acetite;* with hydrochloric acid, *hydrochlorite.* Many of these bodies, decomposed by water at a high temperature, have reproduced the acid which formed them and crystallised mannite.

I intend continuing the study of this property, and from some experiments which I have already made I hope to extend it to several of the chief neutral bodies of the vegetable kingdom.—*Comptes Rendus*, No. 14, April 3rd,, 1854.

On the COMPOSITION *of* EGGS. By MM. FREMY *and* VALENCIENNES.

We have shewn in our Memoir presented to the Academy, the facts proved by our investigations on the eggs of various animals belonging to all the great class of oviparous animals. In this summary we shall endeavor to shew, in a general view, the most important results of this first work.

We have ascertained:—

I.—That there exist fundamental differences between the composition of the eggs of animals, and that, under the collective name of *eggs*, designating the produce of the ovarian apparatus intended to aid in perpetuating different species, are comprehended very complex bodies, differing widely from each other.

II.—That, among vertebrate animals, the eggs of birds, reptiles and fishes, present in their composition differences which are perceptible with the simplest analysis, and yet that the eggs of the saurians and ophidians have a great analogy to those of birds, whereas the eggs of the batraciens resemble more nearly those of cartilaginous fishes.

III.—That the eggs of spiders and insects are completely different in their composition from the eggs of other animals.

IV.—That those of the crustaceæ, being organised so as to hatch in water, in no way resemble those of fishes or other amphibious vertebrata.

V.—That the same observation applies to the eggs of the mollusca.

VI.—That these differences do not belong simply to classes or orders, that they extend to natural families, and do not even stop there, because we have proved that the egg of a cartilaginous fish has not the same composition as that of a bony fish; but, moreover, that the egg of *a*

carp is very different from the egg of a salmon; that the egg of an ophidian, such as the adder, does not contain the same principles as those of the chelonians.

VII.—That, if the composition of the different proximate principles is the same in the nearest species, the form and size of the granules of vitelline vary in a sufficiently appreciable manner to be ascertained and assigned to each species.

VIII.—That the albuminous substances produced by the eggs of birds, reptiles, fish and crustaceæ, present, in their chemical properties and their point of coagulation, such differences as to lead to the supposition that these bodies are constituted of different proximate principles.

IX.—That an egg changes its nature, that its liquids undergo considerable modifications at the different periods of its formation, when becoming detached from the ovary, and while remaining in the oviduct before being laid.

X.—After having ascertained, in the eggs of different animals, the presence of several new proximate principles, ichtine, ichtuline, ichtidine and emidine, and comparing these results with those which MM. Dumas and Cahours obtained in the analysis of hen's eggs, we have no hesitation in proposing to men of science to admit of the existence, in eggs, of a new class of organic bodies comprising proximate principles which we shall henceforward designate under the name of *vitelline substances* or *bodies.*—*Répertoire de Pharmacie*, April, 1854.

On the Coloring Matters *of* Flowers.

By MM. E. Fremy and Cloez.

Chemists possess but incomplete notions respecting the coloring-matters of flowers.

As may be supposed, this study presents great difficulties; the coloring substances of flowers are uncrystallisable; they are frequently altered by the action of the reagents employed to isolate them; and, moreover, flowers of the brightest colors often owe their tint to very small quantities of the coloring-matters.

Various opinions have been held on the nature of the substances which color flowers; many observers have considered that flowers owe their color to two coloring-matters only—one blue, called *cyanic*, and the other, yellow, called *xanthic*. Others have tried to establish a relation between the coloring-matter of the leaves, chlorophylle, and the substances which color the flowers; they rely in general on considerations drawn from the elementary analysis of these different proximate principles. Now, all these chemists know that chlorophylle has never yet been obtained in a state of purity; it probably retains a variable quantity of fatty bodies and albuminous substances; besides the coloring-matters of flowers were scarcely known; it was, therefore, im-

possible to establish any theory based on the necessarily uncertain composition of impure proximate principles.

For some time the blue coloration of flowers was supposed to be due to the presence of indigo; but M. Chevreul has demonstrated in the most positive manner that the blue substance of flowers always reddens on the addition of acids, and that it is very different from indigo, which always retains its blue color, even when acted on by the most powerful acids.

Thus, we see that the coloring-matters of flowers had hitherto been examined only in a superficial manner, and that it was very important to entirely recommence the study of them; these substances are interesting to chemists, because they serve, in the laboratory, as fit reagents to characterise the alkalies; and, if better studied, they might, perhaps, guide horticulturists in the varieties of color which they endeavor to give to the flowers which they cultivate.

We considered that, before having recourse to elementary analysis, it would be better to study carefully the processes which might be employed to extract the coloring-matters from flowers, and to ascertain whether these substances should be considered as separate proximate principles, or whether they are derived from one single body influenced in different ways by the juices of vegetation.

This note contains the results of our first investigations on this subject.

BLUE COLORING-MATTER OF FLOWERS—*(Cyanine)*.

We give to the blue matter of flowers the name of *cyanine*. To extract this substance we first treat with alcohol the petals of the blue bell, violet, or iris; the flower loses its color and the liquid becomes immediately of a beautiful blue tint.

When the coloring-matter has been left for some time in contact with the alcohol; the blue color slowly disappears from the liquor, and is found to be replaced by a brownish-yellow color; the coloring-matter has, in this case, been reduced by the prolonged action of the alcohol, but it resumes its original color when the alcohol is evaporated in contact with the air; the alcohol must not, however, be left too long in contact with the coloring-matter, for then the alcoholic extract does not resume its blue coloration by the action of oxygen.

The product of the evaporation of the alcohol is treated with water, which separates a fatty, resinous substance; the aqueous solution containing the coloring substance is then precipitated by neutral acetate of lead; this precipitate, which is of a beautiful green color, is well washed with water, then decomposed with hydrosulphuric acid, the coloring-matter remains in solution in the water, this liquor is evaporated carefully in the sand-bath; the residue is taken up with absolute alcohol, and, finally, the alcoholic liquor is precipitated with ether, which separates the cyanine in the form of blueish flocks.

Cyanine is uncrystallisable, soluble in water and alcohol, insoluble in ether; acids and acid salts change its color immediately to red; alkalies communicate to it, as is well known, a green color; cyanine

appears to behave like an acid, or, at least, it forms with lime, baryta, strontia, oxide of lead, &c., green compounds, which are insoluble in water.

Bodies, having a great affinity for oxygen, such as sulphurous acid, phosphorous acid, and alcohol, act upon and decolor it; it resumes its color under the influence of oxygen.

We must here mention that M. Moroz has recently removed from blue bells a beautiful blue substance, by treating these flowers with absolute alcohol.

ROSE-RED COLORING MATTER.

We have used alcohol to extract the substance which gives a rose color to certain dahlias, roses, peonies, &c. The process which we employed for the extraction of this coloring matter is precisely the same as that for obtaining cyanine; the substance was precipitated by neutral acetate of lead, then purified by means of absolute alcohol and ether.

On attentively comparing the properties of this coloring matter with those of cyanine, we have ascertained that the pink coloring matter is the same as the blue substance, or at least that it results from a modification of the same proximate principle; it is found under the rose-colored modification, when the vegetable juices, with which it is found in contact, possess an acid reaction. We have always found this acid reaction in the juices of flowers having a red and pink coloration, whereas, the juices of blue flowers have always been neutral.

We have subjected to the influence of alkalies most of the pink or red flowers cultivated at the *Muséum*, and we have always found that they have become first blue, and then a beautiful green.

It is not uncommon to see certain pink flowers, such as mallows, and in particular the *hibiscus syriacus*, become first blue, and then green as they wither; this change is due, as we have found, to the decomposition of a nitrogenous organic substance, which is very abundant in the petals of the flower. This body, while being destroyed, produced ammonia, which gives to the flowers the blue and green tints which manifest themselves as the flower withers; moreover, the action of a weak acid will restore the pink color to the petals.

The change of color of certain pink flowers may be observed likewise when their petals undergo a very rapid dessication; for instance, *in vacuo;* when this is the case, it is difficult to admit that a nitrogenous organic substance can have undergone a sufficiently advanced decomposition to produce ammonia; but we must first remark, that in this case the modifications of tint are more of a violet color, and never reach the green, and that then they are always accompanied by a disengagement of carbonic acid, which we have ascertained by direct experiment. Thus the petals, which were at first pink, and which by a slight dessication become violet, disengage carbonic acid; we may, consequently, allow that it is this carbonic acid which retains the pink color of the flower, and which, in setting itself free, allows the

petals to take the blue tinge which characterises those flowers whose juices are neutral.

We, therefore, think that we may with certainty assert that pink, violet-colored, and blue flowers, owe their coloration to the same substance, differently influenced by the juices of the flowers.

Scarlet flowers likewise contain cyanine, reddened by an acid; but this substance is then mixed with the yellow-coloring matters, which we are now about to describe.

YELLOW COLORING MATTERS.

The most simple experiments show that no analogy exists between the substances which give flowers a yellow color, and that of which we have just spoken; in no case can reagents communicate to the yellow substances extracted from flowers, the blue, pink, or green tints so easily produced with cyanine.

On examining the various yellow flowers, we found that their coloration is due to two substances, differing in their properties, and which do not appear to be derived from the same proximate principle; one is completely insoluble in water— we call it *xanthine*. The name of xanthine was given by Runge to a yellow substance, extracted from madder-root. As this name has not been adopted in science, we have thought that we might use it to designate one of the coloring principles of yellow flowers. The other is very soluble: we have called its *xantheïne*.

YELLOW COLORING MATTER INSOLUBLE IN WATER—(*Xanthine.*)

We have extracted this coloring-matter from several yellow flowers, but chiefly from the great sunflower (*Helianthus annuus.*)

To obtain it, we treat the flowers with boiling absolute alcohol, which, while hot, dissolves the coloring matter and deposits it completely on cooling. The yellow deposit thus obtained is not pure xanthine, it contains a very considerable quantity of oil; to remove this fatty body, we have recourse to careful saponification; we heat the yellow precipitate with a small quantity of alkali, so as to saponify the fatty body mixed with the xanthine, and which remained with it even in solution. As the yellow coloring matter is soluble in soapy water, we do not take up the mass with water; but we decompose it by an acid, which isolates the fatty acids resulting from the saponification and the xanthine; we treat this precipitate with cold alcohol, which dissolves the fatty acids and leaves the xanthine. This substance is of a beautiful yellow; it is insoluble in water; soluble in alcohol and ether, which it colors of a golden yellow.

It appears to be uncrystallisable, and presents the general properties of the resins.

It is xanthine, which, mixed in variable proportions with cyanine, differently modified by the vegetable juices, gives to flowers orange, scarlet, and red colors.

YELLOW COLORING MATTER, SOLUBLE IN WATER—(*Xantheïne.*)

When we extract the substance which colors some dahlias yellow,

we at once perceive that it has no analogy with xanthine; this latter is, as we have seen, insoluble in water, whereas the coloring principle of which we are now about to speak, is very soluble in water; we call it *xantheïne*.

To obtain xantheine, we treat the petals of yellow dahlias with alcohol, which quickly dissolves the yellow coloring matter, and likewise some fatty and resinous bodies; the liquor is evaporated to dryness; the residue is again taken up by water, which precipitates the resins and fatty bodies; this liquor is again evaporated to dryness, the residue is subjected to the action of absolute alcohol; this solution, diluted with water, is treated with neutral acetate of lead, which precipitates the coloring matter; the salt of lead is then decomposed with sulphuric acid; the xantheine remains in solution in the water; it is finally purified with alcohol.

Xantheine is soluble in water, in alcohol, and in ether, but does not crystallise in any of these solvents. Alkalies communicate to it a very rich brown coloration; its tinctorial power is considerable; it produces very bright yellow tones on various kinds of tissues.

Acids cause the disappearance of the brown coloration manifested by the action of the alkalies. Xantheine unites with most metallic bases, forming insoluble yellow or brown lakes.

Such are the properties of the coloring substances which we have extracted from flowers. These first experiments shew that the yellow coloring matters are entirely different from those which color flowers rose or blue; this fact is found in accordance, moreover, with all the observations which have long been made on the coloration of flowers. We know, in fact, that blue flowers may become red, or even white, when the color is completely destroyed, but they never become yellow; and in the same manner yellow flowers never become blue. It is not uncommon to see an orange-colored flower become red; that is, when the xanthine has become destroyed, and the cyanine modified so as to be red by the action of the vegetable juice, is predominant.

We have consequently proved the existence in flowers of three coloring principles—cyanine (blue or rose color), xanthine (yellow matter insoluble in water), and xantheine (a yellow matter soluble in water).

These three substances may, in the state of purity, and more frequently by their mixtures, produce the colors found in most flowers; but our investigations have not been continued for a sufficient length of time for us to assert that the substances which we have isolated, are the only ones which color all flowers.

We shall return to this question in another series of experiments, for the purpose of ascertaining the elementary composition of the three coloring matters which we have isolated.

Journal de Pharmacie et de Chimie, April, 1854.

Experiments *on* Germination. By M. Vogel, Jun., of Munich.

We may consider, as an ascertained fact, that carbonic acid and water are formed in the course of germination; at least these two produces never fail to appear in the course of germination; this chemical action consequently appears to be a very simple operation. There are, nevertheless, some facts which lead us to believe that this process is more complicated than has hitherto been imagined. Germination may certainly be compared to a slow combustion, for the carbon of the seed enters into combination with the oxygen of the atmosphere; but we know also that there are formed, during the combustion of organic bodies, besides carbonic acid, other combinations of carbon with oxygen, principally gaseous oxide of carbon. Analogy, therefore, would lead us to suspect the existence of another gaseous product containing carbon, which is disengaged during germination, as well as carbonic acid.

M. Boussingault, to whom we are indebted for some very interesting experiments on vegetation, was the first who rendered very probable the presence of a gaseous oxide of carbon, formed during the process of germination.

M. Boussingault's work, the well-known results of which we need not now recapitulate, shews in a decisive manner that all the carbon lost by the seed during germination cannot be transformed into carbonic acid, but that this loss should rather be calculated as oxide of carbon. As may be seen, the presence of oxide of carbon has been demonstrated by calculation.

The following experiments are only intended to complete M. Boussingault's work in some respects.

The discovery of gaseous oxide of carbon, besides carbonic acid, is based on the fact that not only oxide of carbon, but likewise the combinations of carbon with hydrogen, mixed with oxygen gas, and exposed to a high temperature, form carbonic acid gas. To procure a sufficient quantity of the air which had served for germination, moistened grains of barley were placed in a vessel containing three quarts of air. After two days, when the process of germination was in full activity, this air was introduced into a phial, and was agitated with a concentrated solution of caustic potassa to remote the carbonic acid. The grains of barley were afterwards put in contact with a fresh quantity of air. Towards the end of the germination we introduced the atmospheric air into a gasometer, and tested it as follows:—The air, in passing from the gasometer, was introduced into a cylinder filled with a solution of caustic potassa, and thence through a Liebig's apparatus, to remove the last traces of carbonic acid. This air, dried by means of chloride of lime, was then introduced into baryta water, to make certain that the very last traces of the carbonic acid had been extracted. The air, thus purified from carbonic acid gas, contained oxygen gas, nitrogen, and the combination of carbon in question; the latter should give, at any rate, carbonic acid when heated with oxygen gas. To render the combustion as complete as possible, I passed the air thus deprived of carbonic acid, and mixed the oxygen gas with it,

through a small glass tube filled with twisted platinum wire, heated to redness, and collected it in baryta water. It is evident that the least vestige of carbonic acid formed by the combustion would render the water cloudy, and this always took place.

It is necessary to ascertain previously, by several experiments, that there are no organic substances in the apparatus. Common air thus treated never gave the least trace of carbonate of baryta.

The greatest difficulty in this experiment is in removing the carbonic acid completely from the air. For this reason it is absolutely indispensable that the gas should pass through baryta water previous to the combustion, so as to make perfectly sure of the absence of carbonic acid.

We have thus often analysed the air in which seeds of various descriptions had germinated, and we have always found that the baryta water became more or less turbid. There is, therefore, no doubt that during germination, there is formed, not only carbonic acid, but another gaseous product with a base of carbon, which is really gaseous oxide of carbon.

In a future work, I shall endeavor to determine the quantity of this gas produced in the different periods of germination.

Journal de Pharmacie et de Chimie, April, 1854.

On the Influence *of the* Air *on* Fermentation *and* Putrefaction.

By MM. Schroeder and Dusch.

Fermentation is one of the chemical phenomena whose causes have been the most actively discussed since chemistry became an experimental science. When Gay-Lussac showed that a mixture of sugar and water with a ferment will remain good *in vacuo* for an indefinite period, and that the reaction commenced on the introdution of a single bubble of oxygen, he attributed the fermentation to electricity. This view was supported by the experiment of M. Colin, according to which, in liquids which do not ferment, the phenomenon of fermentation is instantly produced under the influence of the voltaic current; it was reproduced by M. Thénard at the time of his researches on oxygenous water, and then replaced by the physiological theory of M. Cagnard-Latour, then by the catalytic force of Berzelius, or by that of contact of Mitscherlich.

According to M. Cagnard-Latour's theory, yeast is formed of a number of minute vegetable growths living at the expense of the saccharine matter; the transformation of this matter into alcohol and carbonic acid, that is fermentation properly so called, is a physiological act, which M. Quevenne likens to germination, likewise accompanied by the decomposition of the sugar, disengagement of carbonic acid, and an elevation of temperature. To these theories, exclusively applicable to alcoholic fermentation, and which with difficulty explain why a small quantity of yeast is sufficient to decompose a great quantity of sugar, Liebig opposes a purely mechanical theory, which he ex-

tends to catalytic effects, as well as to the phenomena of putrefaction and eremacausis, and which he bases on the principle laid down by Laplace and Berthollet—that *a molecule being set in motion by any force whatever, may communicate this movement to another molecule in contact with it;* this principle, which manifests itself whenever the resistance (cohesion or affinity), is insufficient to be an equilibrium to the movement, moreover accords with the proposition of M. Millon that, in certain cases, *the infinitely large mass submits to the law of the infinitely small quantity;* a proposition applied by him to catalytic actions, and justified even by purely physical phenomena.

Of all the facts adduced against this mechanical theory, doubtless none have been so valuable as those observed by Schwann, and, after him, by Helmholtz and Dr. Ure. According to them, Gay-Lussac's experiment only succeeds when the oxygen destined to provoke the fermentation, has previously passed through a procelain tube heated to redness; blood, muscular flesh, a mixture of ferment with sugar and water may be preserved intact in atmospheric air, if kept at a heat of at least 212° F. This led Schwann to think that the spontaneous decomposition, called fermentation or putrefaction, was nothing but the result of the vital manifestation of some cryptogamia or microscopical animacules produced by the spores and germs contained in the atmosphere, and becoming developed when they found themselves in a favorable medium.

The miasmata and contagions which Liebeg considers as connected with the cause to which he attributes fermentation, would, therefore, only proceed themselves from these microscopical germs, introduced into the blood by the respiration, and becoming propagated there at the expense of this nourishing liquid.

This other revived Roman physiological theory which Varronius mentions in the *De Re rustica*, has in some degree led to the following researches, whose starting-point goes up to certain facts first observed by Rigaud de l'Isle in the Pontine marshes, and which M. Becquerel mentions in the following manner:—"A forest interposed to the passage of a current of moist air, charged with pestilential miasmata, sometimes preserves all behind it from its effects; whereas, the uncovered portion is exposed to diseases. The trees in such cases appear to filter the air, and to purify it by removing the miasmata."

Applying Schwann's ideas to these facts, MM. Schroeder and Dusch proposed to eliminate the germs of the infusoria by a species of filtration, copied from that which Nature has organised on the large scale, and knowing that cotton wadding condenses pestilential miasmata on its surface, and thus renders them liable to be transported to a distance, they have availed themselves of this substance to filter the atmospheric air which might enter into their experiments.

The general result of these investigations is, that filtered air behaves like calcined air; it is incapable of producing fermentation or putrefaction.

The filter used by MM. Schroeder and Dusch consists of a tube of about two centimetres in diameter, and fifty centimetres in length; it

is filled with cotton lightly put in, which had been previously heated in the sand-bath.

The apparatus itself is composed of a glass balloon, hermetically closed by a cork, surrounded with wax, and provided with two tubes, one of which is in communication with one of the extremities of the filter, which is itself terminated by a small tube at right angles. The second tube serves as an aspirator; it plunges almost to the bottom of the balloon, and communicates hermetically with a gasometer. The balloon contains the fermentible substance. When quite certain that all the joints are perfect, the balloon is placed on a sand-bath, and it is kept there until the different tubes of communication are hot; after which the security of all the joints is again ascertained, and the tap of the aspirator is so placed that the water may flow drop by drop.

The first experiment was made with muscular flesh, with the addition of water; a second balloon, containing the same substance, was placed by the side of the apparatus; the second balloon was open, and served as a comparison.

At the close of the second week the matter contained in this balloon was quite putrid; it diffused an insupportable odor, and was obliged to be removed from the laboratory.

On the contrary, the balloon, which only received the filtered air, had in no respect changed its appearance; the matter remained in perfect preservation; and when, at the end of twenty-three days, the apparatus was opened, the meat was found wholly uninjured.

In a second experiment, undertaken at a higher temperature, the filtered air was not renewed during the night; the treatment continued for twenty-eight days; the results were precisely similar to the preceding.

In a third experiment the air was not renewed at all, and the filter of cotton was replaced by a glass tube of about thirty-five centimetres in length, and one-half millimetre of internal diameter; the apparatus was exposed to the sand-bath, as in the previous experiments, and left to itself. After nine days the meat was covered with a great deal of mouldiness. Nineteen days after putrefaction had not commenced; the odor had become very mouldy.

Finally, in a fourth balloon, similar to the preceding, muscular flesh was boiled with water; it was slightly corked with a bung of cotton, and the whole was covered with a pad of the same material, fastened to the neck of the balloon with a silken string.

This apparatus was arranged at the same time as the two last, and was opened at the end of twenty-four days; the meat was neither mouldy nor putrid; but there were several whitish patches on it, similar to some which had been observed on the meat of the second experiment.

The liquid surrounding the meat had all the characteristics of unsalted broth, and, like that, it slightly reddened litmus paper.

From these researches it therefore results, that *meat, recently boiled, as well as fresh broth, remain intact for several weeks in an atmosphere which has previously been filtered through cotton.*

Similar results have been obtained with unfermented beer-must; the experiments, which were made in the summer, lasted for at least twenty-eight days; the comparative balloon, which was in direct communication with the air, was rapidly filled with mouldiness; the liquid became thick; whereas, the must contained in the balloon, into which the air was filtered, retained all its limpidity. But on removing the cotton from the tube, so as to admit the free air, this liquid, which had kept so well, very soon became mouldy; the vegetation commenced at the very spot at which the unfiltered air came in contact with it, and the liquid became turbid, and behaved in every respect like that which was in the open balloon.

According to the authors, filtered air has no effect on recently boiled milk; it does not retard its coagulation. They likewise think that the putrefaction of caseum took place as quickly in the one way as in the other; however, the filtered air prevented mouldiness.

The results were likewise negative with meat boiled in a sand-bath in a balloon closed by a pad of cotton; in this case, putrefaction took place as quickly as in the open balloon; the only difference was in the infusoria which animated the greenish-brown liquid formed round the meat, during the progress of its decomposition. The open balloon contained a multitude of animalcules belonging to two distinct species; the *monas termo* and the *vibrio lineola*, whereas the liquid produced under the influence of the filtered air was entirely free from vibriones, monades, and infusoria in general. However, in a second experiment, undertaken in July, the product of the putrefaction in the filtered air contained infusoria of a peculiar species, formed of cellules of the size of a cellule of ferment.

MM. Schroeder and Dusch are not satisfied with these last results; they fear that, by cooking the meat without water, they may have entirely destroyed the germs deposited in the interior of the substance; under these circumstances they intend renewing these experiments in another way; they propose replacing the cotton by other substances, for they think that the interposition of one substance might have a different effect to that of another.

It is to be regretted that the authors have not examined the cotton used for the filtrations; the germs which, in their opinion, the air abandons in the filter, should be recognisable with the aid of a microscope. Who knows even whether, in this case, we might not be able positively to prove their evolution?

Hitherto the existence in the air of these organisms has been contested; the part first attributed to them by MM. Schroeder and Dusch, may be filled by some substance with powerful affinities, such as ozone. The innocuity of calcined or filtered air may be as well explained by this hypothesis, as by that of Schwann; he at any rate proved that air always contains ozone; we likewise know that this allotropic oxygen possesses an affinity of a hundred times the strength of common oxygen, and it will be easily understood that, in the presence of the vegetable fibre of the cotton, this affinity would be able to satisfy itself amply. The filtered air may therefore be free from ozone, and it is

not probable that it would resume this body in repassing through the filter, whereas it would probably resume the organic germs which the cotton could only fix physically, and which would be accumulated at the entrance of the tube, as it results from the observations made with fermentable substances contained in balloons simply closed with a pad of cotton. A direct experiment might decide this question.

It likewise remains to be ascertained whether a fermentable substance, preserved in filtered air, would resist an electric current; the experiment would be easily made; from the commencement of the operation, two metallic wires should be placed in the balloon communicating externally; when the preserving influence of the filtered air has been thoroughly proved, a sufficiently intense current should be passed in, to obtain a distinct proof of the decomposition of the water.

Annalen der Chemie und Pharmacie.

ANALYTICAL CHEMISTRY.

RESEARCHES *in* ANALYTICAL CHEMISTRY. BY M. EBOLI, *Professor of Chemistry and Mineralogy, at the Lima School of Medicine.*

I THINK that the publication of the following results of some experiments which I have made on several organic substances may be useful, not only in a scientific point of view, but because many of them acting as violent poisons on the animal economy, they may be of service to forensic chemistry by shewing, in a simple and certain manner, the presence of these poisons.

1 or 2-thousandths of a gramme of the substance to be analysed is put into a watch glass, on to which are to be poured 5 or 6 drops of sulphuric acid, diluted with an equal quantity of distilled water. Into the liquid is put a small piece of chromate of potassa (about 30-hundredths of a gramme), and the changes of color which take place are to be observed.

The following is a list of the changes of color which I observed with each substance. I must observe that each change lasted for several hours :—

Morphine.—A green color, resembling the solution of nitrate of nickel (I shall call this color green of nitrate of nickel), then a green similar to the solution of sulphate of copper (I shall call this green of sulphate of copper); the reaction finished with a dark, dirty green.

Sulphate of Morphia.— Green of nitrate of nickel, then green of sulphate of copper, finishes of a dull yellow.

Acetate of Morphia.—Green of nitrate of nickel, then dirty green of sulphate of copper; finally, greenish-blue.

Quinine.—Green resembling the green precipitate produce, by pouring a solution of sulphate of copper into a solution of arsenite of potassa (I shall call this tint green of arsenite of copper), then a beautiful yellow green; finally a dark green.

Sulphate of Quinine.—Green of nitrate of nickel, then green of sulphate of copper; ends with a dirty yellow.

Ferrocyanate of Quinine.—Dirty green, then grass green, dirty greenish-yellow; finishes of a chocolate yellow.

Cinchonine.—Green of arsenite of copper, then bright yellow-green; finally a dark, dirty yellow.

Sulphate of Cinchonine.—Green of arsenite of copper, then green of sulphate of copper; finally a dark, dirty green.

Veratrine.—Dirty green, then bottle-green, dirty green of nitrate of nickel, which then brightens, green of sulphate of copper; finally dark, dirty yellow.

Atropine.—The first reaction, which is green of nitrate of nickel, is only produced after the lapse of some minutes; then greenish-yellow; finally, dirty greenish-yellow; then a yellowish resinous precipitate is deposited, which is soluble in alcohol.

Delphine.—Dirty green, then the liquor clears and passes to dirty green of nitrate of nickel, and finally to a dirty yellow.

Lupuline.—The reaction is some time in manifesting itself; dirty greenish-yellow, finishing with dirty yellowish-green.

Codine.—Green of arsenite of copper, then green of nitrate of nickel, green of sulphate of copper, ending with dark, dirty green.

Daturine.—Green of sulphate of copper, finishing with greenish-blue.

Strychnine.—Very intense violet color, nearly black where it is in contact with the chromate; afterwards a yellowish violet color resembling wine leys; after two days it ends by becoming blue. Messrs. Graham and Hoffmann were the first to observe the violet color produced in the reaction of chromate of potassa on strychnine, with the intermediation of sulphuric acid. Their experiments were published last year. To the observations of these chemists should be added the passing from one color to another, as I havejust described.

Hydrochlorate of Strychnia.—Intense violet color, nearly black in contact with the chromate; afterwards a dirty violet resembling wine leys; finishes of a deep, dirty yellow.

Cafeine.—Nothing.

Naphthaline.—Nothing.

Piperine.—Pretty greenish yellow, then green of nitrate of nickel; finishes of a dirty green.

Cantharidine.—The sulphuric acid in this case must be concentrated, and it must be gently heated in a spirit lamp so as to dissolve the cantharidine unaltered; when the solution nearly boils it is removed from the lamp, and the chromate is added; a brisk effervescence is produced, after which a soft mass is found of a magnificent green color, which after some hours dissolves; the color is then less beautiful; it concludes of a thick grass green.

It is remarkable that it is not indifferent whether we add to the substance of the sulphuric acid a solution of chromate of potassa instead of crystallised chromate. In the first case the reaction is instantaneous and tumultuous, so that it is impossible to follow the

changes of color, or even to seize upon any one definite change; whereas, in the second case the crystallised chromate of potassa acts gradually, and in proportion as it dissolves its action is different; it thus produced the above-mentioned colors.

The difficulty of obtaining chemicals here has been the only reason why I have not continued my studies on other organic substances; I think that many of them are susceptible of such characteristic reactions.

Messagero de Lima.

On the Loss *which* Minerals *sustain from* Heat. Determination *of the* Nature *and* Quantity *of such* Loss, *especially with regard to* Fluorine. By MM. H. Sainte-Claire Deville and Fouque.

The work which we have now the honor of presenting to the Academy was commenced by the advice of M. de Senarmont. He asked us to prove, by chemical analysis, the conclusions to which he had been led during his beautiful investigations on the variations sustained by the inclinations of the optical axes of certain substances, apparently identical, and consequently presenting completely similar crystalline forms. We must at once mention that his opinions respecting topazes have received the most complete confirmation from analysis. We likewise wished to furnish M. Ch. Sainte-Claire Deville with some analyses in support of the theories of lithogeny, which he has developed in his lessons at the *College de France*. We can therefore, in the following paper, claim only the chemical portion, the methods, and the experiments to which we have been thus led.

The losses sustained by silicious minerals when exposed to heat are in general due to the presence of water, fluorine, and boron. A great number of analyses have proved that there is a great difference between the temperature at which water escapes from minerals and that at which commences the disengagement of fluorine. They may be characterised by mentioning that we used two lamps in the calcinations; one (which we shall call the small lamp) is fed with a mixture of alcohol and essence of turpentine, and has a pipe; the other (we shall call it the large lamp) is a lamp of vapors of essences; at the temperature produced by the latter all the fluorine is disengaged.

Chemists have proved that almost all silicious minerals contain fluorine. The nature of the losses which they undergo when exposed to heat necessarily vary with their composition. What relation exists between the composition of the volatile portion and the composition of the mineral? Such was the question which we undertook to answer.

We experimented upon two substances of completely different compositions. At first we formed a basic silicate of soda, which lost nothing with the large lamp, we added a known quantity of pure fluorine of calcium; the mixture when fused in the small lamp did

not lose weight. With the large lamp all the fluorine was disengaged in such a form as not to remove the least trace of silicium. The platinum crucible in which the calcination was performed was not in the least *opalised*, which would have been caused by the decomposition of fluorine of silicium, if the least trace had been disengaged, and we found on analysing the vitreous residue almost the whole of the silica which we had put into it. No fluorine remained, and the lime formed at the expense of the fluorine of calcium replaced, equivalent for equivalent, the quantity of soda which had disappeared. The volatilised matter was consequently pure fluorine of sodium.

Another striking case was furnished us with topaz. We ascertained that its loss with heat was about 13 per cent., and that this enormous loss consisted of pure fluoride of silicium.* We proved this by receiving the vapors in lime by means of a combination of small concentric crucibles, in the centre of which the topaz was perfectly isolated; the system lost nothing perceptible after calcination. By a new process, whose description cannot be given in this extract, we assured ourselves that the lime contained fluoride of calcium and silicate of lime, in which the fluorine and silicium amounted to 3 equivalents of the first and 1 equivalent of the second. The volatile matter was therefore pure fluoride of silicium.

The angles formed by the optical axes of topazes are variable; the loss from heat is greater with white topazes, less with yellow topazes, changing, like the inclinations of the optical axes, at the same time as the coloration. It would be natural to imagine that this phenomenon was due to the variation of two isomorphous elements susceptible of replacing each other, without sensibly affecting the relative positions of the facettes of the crystal. This appeared to us exactly correct, for the topaz, admitting, as representing its formula, the symbol $3Si \left\{ \begin{matrix} \dot{O} \\ Zl \end{matrix} \right\}^3 4Al^3 O^3$, in which the isomorphous elements are placed in the same vertical line; white topazes then differ from yellow topazes, at least in the specimens we have examined, only by a greater proportion of fluorine being substituted for oxygen. M. Forchhammer's experiments likewise lead to this result.

Between topazes which only lose fluoride of silicium and the basic and fluoric glasses, which only lose alkaline fluorides, are found a great number of minerals to which we have applied a special method of analysis, founded on volatilisation in determinate conditions. We should mention that we have but little confidence in the determination of the fluorine into the state of fluoride of calcium—a substance which decomposes with extraordinary facility when calcined, and which, moreover, is rarely obtained pure by precipitation.

In conclusion, we would remark, among the fluorised minerals which behave in the fire like substances which are intermediary be-

* The aluminous silicate, which is the residue of this operation, is remarkable for its infusibility. It resisted such a temperature that we found it unaltered on a button of perfectly melted platinum, formed by the crucible which had been used in the experiment.

tween those which disengage and those which retain the silicium, the minerals with a base of lithium, particularly lepidolite. It gives with the large lamp a very intense red flame, and the volatilisation of a considerable quantity of lithium might be thus proved, as well as by analysis. This fact confirms the opinions of M. Ch. Sainte-Claire Deville, on the subject of the irregularities presented by the analysis in which lithium and fluorine have been proved after calcination..

Comptes Rendus, No. 17, February 13th, 1854.

INORGANIC CHEMISTRY.

Researches *on the* Fluorides.

By M. Fremy.

Some years ago a Belgian chemist, since dead, M. Louyet, announced to the Academy several important facts respecting fluorine, hydrofluoric acid, and the fluorides. According to M. Louyet, fluoride of mercury, heated in tubes of fluoride of calcium, was decomposed by dry chlorine and gave fluorine; anhydrous hydrofluoric acid, prepared by the method mentioned by M. Louyet, did not attack glass; and, moreover, the equivalent of fluorine determined by Berzelius should be replaced by a new number.

Having assisted at the performance of several of M. Louyet's experiments, and not having considered the results satisfactory, I determined to submit the facts which he had mentioned to a strict examination: this is the aim of the researches of which I now communicate the first portion to the Academy.

In such a work, the difficulties of which are known to all chemists, and following such men as Gay-Lussac, M. Thenard, Berzelius, and Davy, I could not anticipate that any fortunate scientific accident might lead me to the immediate discovery of fluorine; but I knew that a general study of the fluorides would, under any circumstances, be very interesting to scientific men; it would complete the history of a series of compounds hitherto but little known, and which, notwithstanding, play an important part in geological phenomena; it might indicate the direction which should be followed to attain the discovery of fluorine. This hope has sustained me through the lengthy work whose principal results I am about to describe.

The first part of my Memoir relates to the preparation of pure anhydrous hydrofluoric acid. I prepare this acid by a new method, by submitting to distillation in a platinum still, hydrofluate of fluoride of potassium.

Anhydrous hydrofluoric acid, when thus obtained, is gaseous at the ordinary temperature, but may be condensed by a mixture of ice and salt. It has then the appearance of a very fluid liquid, volatilising when removed from the refrigerating mixture, acting very powerfully on water, diffusing in the air white fumes, whose intensity may be compared to those of fluoride of boron. Contrary to M. Louyet's assertion, anhydrous hydrofluoric acid attacks glass rapidly.

I have likewise obtained anhydrous hydrofluoric acid, by decomposing in a platinum tube, by means of dry hydrogen, fluoride of lead, which I had placed in a charcoal boat, so as to avoid the action on the platinum of the reduced lead.

To avoid all the errors made by my predecessors in the study of impure fluorides and in endeavors to isolate fluorine, I have always used in my investigations an acid obtained from an absolutely pure, crystallised hydrofluate of fluoride of potassium. I have thus obtained sometimes new fluorides, sometimes fluorides whose characters had not been given by Berzelius.

Thus my Memoir contains a complete study of the fluorides of zinc, iron, and lead, which I obtained in a crystallised state. I have produced protofluoride of tin in very clear and voluminous prisms: I have likewise obtained bifluoride of mercury in well-defined crystals.

Fluoride of silver, which was considered uncrystallisable, may, on the contrary, be deposited from a concentrated solution in crystals, whose form presents the greatest regularity.

I shall now give some of the consequences resulting from this general study of the fluorides.

All the fluorides which I have analysed, have been obtained directly by uniting the pure acid with the anhydrous or hydrated metallic oxides.

Hydrofluoric acid does not react on all the oxides which are attacked by hydrochloric acid. Thus, I found it impossible to combine hydrofluoric acid with auric acid and peroxide of platinum; finding that in this case hydrofluoric acid behaved like an oxyacid, I endeavored to ascertain whether hydrofluoric acid, which had been long called *fluoric acid*, did not in reality contain oxygen. These experiments, which presented almost insurmountable difficulties, are described in my Memoir. I shall here only mention that they confirmed the constitution of hydrofluoric acid admitted by all chemists, and that they give, in my opinion, the character of a rigorous demonstration to it, which has hitherto been wanting. The result of my researches is, that the fluorides should be divided into three classes, and each of these classes forms a collection of important general properties.

The first class comprehends the acid fluorides or hydrofluates of fluorides: these compounds are very easily formed, heat decomposes them, and when they are anhydrous they give neutral fluorides and pure hydrofluoric acid: in many experiments they may replace hydrofluoric acid. I have employed the salt of potassa for the production of a new organic compound, which is somewhat interesting; I speak of the hydrofluoric ether from common alcohol, I prepare this ether by submitting to distillation, in a platium apparatus, a mixture of sulphovinate and hydrofluate of fluoride of potassium. I thus obtained gaseous hydrofluoric ether, which, in its general properties, resembles the corresponding compound of wood-spirit, discovered, as is well known by MM. Dumas and Peligot.

The second class is composed of neutral and hydrated fluorides: these bodies are characterised by the ease with which they are decom-

posed into oxides and hydrofluoric acid, when we endeavor to remove the water which enters into their composition; they behave exactly like real hydrofluates. Thus, crystallised fluoride of silver, which belongs to the class of hydrated fluorides, disengages hydrofluoric acid and produces oxide of silver when dried even *in vacuo*: when hydrated fluoride of silver is heated it disengages hydrofluoric acid and oxygen, and leaves a residue of very pure silver; in this case, then, it acts like a hydrofluate of oxide of silver. Fluoride of mercury, which is likewise hydrated, is decomposed by heat in the same manner as the preceding salt, disengaging hydrofluoric acid, mercury and oxygen.

The third class comprehends the anhydrous fluorides. These salts are undecomposable by heat, and may be, according to the nature of the metal which they contain, decomposed by oxygen, hydrogen, chlorine, sulphuret of carbon, and steam.

I confess that I attach great importance to this division of the fluorides into three classes; it is because they were not aware of it, that the observers who have preceded me have often committed serious mistakes in the study of the fluorides. Thus, M. Louyet thought that he could isolate fluorine by decomposing fluoride of mercury by chlorine with heat; as fluoride of mercury belongs to the second class, and is hydrated, it behaves in all its reactions like a hydrofluate. The gas of M. Louyet was consequently a simple mixture of oxygen and hydrofluoric acid. In my Memoir will be found several analyses of anhydrous and hydrated fluorides, which have enabled me to determine the equivalent of fluorine; they do not agree with those of M. Louyet, and, in general, confirm those of Berzelius.

After having studied and classified the principal fluorides, my attention naturally turned to those which, from their nature, might lead to the preparation of fluorine.

I first carefully studied the fluorides formed by the difficultly oxidisable metals, hoping that by the action of heat, or some other agent, they might disengage fluorine. My researches in this direction have not given any satisfactory result.

In fact, I found, to my great surprise, that hydrofluoric acid would not combine either with the oxides of gold or of platinum.

Fluoride of silver, when hydrated, behaves like a hydrofluate, and, with heat, only disengages oxygen and hydrofluoric acid; when anhydrous, it is undecomposable.

Fluoride of mercury does not exist in the anhydrous state, and when hydrated, it produces by the action of heat, oxygen and acid vapors.

We must therefore abandon the attempt to obtain fluorine from these fluorides. I was then led by a series of experiments, which it is impossible to describe in this extract, but which are all given in the Memoir, to subject the anhydrous fluorides to powerful decomposing agents.

Guided by some experiments which I am at this time making with M. Ed. Becquerel, in which chloride of calcium, in fusion, is very rapidly decomposed by the battery; I first subjected the fused anhydrous fluorides, such as those of potassium, lead, and calcium, to an

electric current. The decomposition was easily effected; I saw a gas disengaged at the positive pole, which powerfully attacked platinum. But the many difficulties surrounding this experiment have hitherto prevented me from collecting the gas thus disengaged, so as to be able to study it properly.

Sulphur acts, under the influence of heat, on a certain number of anhydrous fluorides, replacing the fluorine; but combinations of fluorine and sulphur are then formed, which will be studied in another work.

The action of chlorine on the anhydrous fluorides, especially on fluoride of calcium, gave me important results. All my experiments were made in platinum tubes, which are not attacked, at a red heat, by chlorine; the gas was dried with the greatest care by several tubes of anhydrous phosphoric acid, so as to avoid the rapid action of steam on the fluorides. I found that, at a white heat, dry chlorine very slowly decomposes fluoride of calcium, and disengages a gas which powerfully attacked glass, and which appeared to be fluorine.

Oxygen passing at a white heat over fluoride of calcium, decomposes it more quickly than chlorine, and produces, as in the preceding experiment, a gas which attacks glass. I have been obliged, much to my regret, to suspend these interesting experiments on the separation of fluorine from the fluorides, because they have already caused the perforation of three platinum tubes.

Finally, there will be found in my Memoir some experiments relative to the action of carbon, hydrogen, and the vapor of sulphuret of carbon on the different fluorides; they were intended to establish, in a direct manner, the constitution of the anhydrous fluorides.

Such is a summary of my experiments on the fluorides. I think that, in this Memoir, I have exposed some errors introduced into science, and completed the history of a series of compounds which required fresh examination. But my work is not yet finished, and the Academy will understand that I shall not consider my task as completed, until I have really isolated a body, of which I have hitherto merely caught a glimpse.

Comptes Rendus, No. 9, February 27th, 1854.

On GLUCINA *and its* COMPOUNDS.

By M. H. DEBRAY.

IT is well known that the emerald contains a base, discovered by Vauquelin, which he called glucina. M. Wöhler obtained the metal from it by means of the reducing action of potassium on chloride of glucinum. The following are the properties assigned to it by that illustrious chemist :—

"Glucinum is a dark grey powder, having the complete appearance of a metal which has been precipitated in finely divided particles. Under the burnisher it has a dark metallic lustre. As it does not agglomerate at the violent heat in which it is reduced, it may be

imagined that it is difficult to melt it. It does not oxidise in the air at the ordinary temperature, nor in water even when boiling. Glucinum, heated in the air on a sheet of platinum, inflames and burns with a brilliant light, and becomes transformed into white glucina.

"It dissolves readily in sulphuric, hydrochloric, and nitric acids; in the first two, with a disengagement of hydrogen, and in the last, with a disengagement of nitrous gas."

The late researches on aluminium have led me to renew the study of glucinum, and I have been able, with M. H. Sainte-Claire Deville's advice, and by means of his processes, to obtain some new results, which I think are worthy of the attention of the Academy.

Glucinum is the lightest of all the known metals which do not decompose water at the ordinary temperature, or at the boiling point: its density is 2·1; it is, therefore, lighter than aluminium. Its appearance might cause it to be confounded with zinc; but its less fusibility (which is between that metal and aluminium), its stability in the fire, and its small density, are so many physical properties which sufficiently distinguish it from zinc.

Unalterable at the ordinary temperature, it oxidises superficially at the highest temperature of the blow-pipe; but without ever presenting the phenomenon of ignition produced when zinc or iron are exposed to the same circumstances. Hydrochloric and sulphuric acids, even dilute, dissolve it with disengagement of hydrogen. A concentrated solution of potassa dissolves it when cold, but ammonia has no effect upon it.

Glucina, from which we can thus extract a true metal, likewise gives well crystallised salts, which I have carefully examined, and which study will form a complete work, which I hope soon to have the honor of presenting to the Academy.

Comptes Rendus, No. 17, April 24th, 1854.

PHYSICS.

New Researches *on the* Principles *which* Govern *the* Disengagement *of* Electricity *in* Chemical Actions. By M. Becquerel.

All questions relative to the disengagement of electricity are of great interest in the physico-chemical sciences, and their applications to the arts and industry, whilst they are of the same order as those which relate to the production of heat in combustion.

The quantity of electricity which is associated with the molecules of bodies is sufficient to alarm the imagination, so immense is it, as I proved in a Memoir which I presented to the Academy on the 9th of March, 1846; unfortunately, only an exceedingly small portion of it can be collected, owing to the recomposition which takes place on the contact of the bodies. The efforts of physicists must, doubtless, contribute to the discovery of the means of preventing this recomposition, if they wish to arrive at endowing the world with a motive power which may compete with steam, and produce much more varied effects;

since it acts not only as a mechanical force, but also as a physical force for producing heat and light, and as a chemical force for decomposing bodies and effecting the combination of their elements.

Such is the object which I have proposed to myself ever since 1823, at which period appeared my first researches on the disengagement of electricity in chemical actions; these researches have enabled me to establish the principles which govern this disengagement, and by means of which I have been enabled to reproduce a certain number of mineral substances, and to extract lead and silver from their respective ores, without any other intervention of heat than that which is necessary for simple roasting, and even without that. This last work, which occupied several years of investigation, is completed, and I intend very shortly to present it to the Academy. The experiments were made on more than 30,000 kilogrammes of ores, not only from Mexico, but also from various parts of the globe, and on a scale sufficient to shew whether or not the process is practicable.

The principles which regulate the disengagement of electricity in chemical actions have been generally adopted, as I made them known. I may only remark that some physicists have lost sight of the circumstance that the experiments which established them were published thirty years ago, and that the results of them were given either in the scientific journals or in my works. I have thought, that, after so long a period, I ought to resume these researches, not only in order to recal to mind that which has been done, but also to rectify any uncertainty that might exist in the results obtained; to add new facts, and especially new principles, which I have been able to do, only devising apparatus proper for demonstrating them, or discovering properties which had hitherto escaped observation.

The apparatus employed are the depolarising apparatus which I recently presented to the Academy, and of which I have given a complete description in this Memoir. I had announced that they ought to serve me for repeating all my researches relative to the disengagement of electricity in chemical actions. It is the result of these researches which I have given in my communication of this day.

I first treat of the electrical effects produced in the reaction of acid, alkaline, and neutral solutions on water, and on each other, avoiding especially the effects of polarisation resulting from the products which are deposited on the platinum plates employed to transmit the currents.

I have arrived at this conclusion, that water is negative in relation to all the acids and to the saturated solutions of neutral salts, and positive in relation to the alkalies; that, in the reaction of the acids on the alkalies, the most oxidising acids are the most positive; and that the acids, in their combinations, transmit to the latter their electro-positive properties, so that, in their mixture or combination, the solutions of the nitrates are positive in relation to the sulphates, and the sulphates in relation to the chlorides.

We see from this why the intervention of aqua-regia and nitric acid in electro-chemical pairs, as I demonstrated in 1827, when I described the battery, each pair of which was formed of nitric acid,

potassa and platinum, produces a more considerable disengagement of electricity than the other acids.

The experiments which I made with the depolarising apparatus have led me to this general fact. When water and several neutral acid and alkaline solutions are in contact, two and two, so as to combine or mix very slowly, the electrical effect produced is the result of individual electrical effects which take place on each surface of contact. This fact is opposed to the principle advanced by Volta, namely, that when several solid or liquid substances are in contact, some in connection with others, the electrical effects which result are the same as if the two extreme substances were immediately in contact.

The principle which I have just pointed out leads to this consequence, that electrical chains are formed with liquids alone, as I shewed to be possible in the Memoir which I presented to the Academy on the 27th of March, 1847. Such chains must also exist in living organised bodies, and hence was conceived the possibility of electro-chemical effects produced in the organic tissues. In my Memoir, I give two examples of currents of this kind, in the stalks of vegetables, during the movement of the sap, and in the tubercles of the potato.

I have also shewn how, with liquids and platinum, or charcoal without alkaline solution, we might obtain constant batteries. I have concluded my Memoir by explaining, in great detail, the disengagement of electricity in the combustion of gases and charcoal. In 1824, I treated this question, with the aid of the condensing electrometer and plates of platinum; a year after, M. Pouillet analysed these effects, and particularly those which are produced in the combustion of charcoal. Having already proved, at that period, that in the contact of flames and plates of platinum, thermo-electrical effects were produced, I repeated all my experiments in 1849, not with the electrometer, but with the multiplier, because I had found that the flames were conductors of electricity, even at very feeble intensities, as well as glass heated below redness. It was on this occasion that my son, Edmund, made a series of researches to prove that hot air, and all gases heated to a suitable temperature, were conductors of electricity. It is extremely probable that bodies which are non-conductors, at a sufficiently high temperature, are conductors of electricity, as they are likewise when they are taken in a state of extreme tenuity.

I have been led to a new property of platinum of which we had no idea—that of presenting different thermo-electrical effects at more or less elevated temperatures. With these new means of action, I was enabled to demonstrate accurately that we should never employ platinum, in sheet or wire, for collecting the electricity of flames, as the physicists who have recently studied this subject have done, since it produces only thermo-electrical effects. I have examined with the same means of action, different from those which had been hitherto employed, the disengagement of electricity in combustion, and I have been led to the same result as M. Pouillet, only it is much more marked, and its intensity may be augmented by increasing the activity of the combustion with saltpetre.

From the facts laid down in this Memoir the following conclusions may be drawn :—

1. In all chemical actions whatever, there is a disengagement of electricity.

2. In the reaction of acids or acid solutions on metals, or on alkaline solutions, the acids and acid solutions always take an excess of positive electricity; the metals and alkaline solutions, a corresponding excess of negative electricity.

3. The disengagement of electricity in combustion is governed by the same principle—that is to say, that the combustible body disengages negative electricity, and the supporter of combustion positive electricity.

4. Decompositions produce inverse electrical effects.

5. There is a disengagement of electricity only when both bodies present are conductors of electricity; thus, in the combination of a metal with dry oxygen, iodine, or bromine, there is no production of electricity.

6. In the mixture of the acids with water, or in their combination with it, water acts as a base; whereas it acts as an acid in relation to alkaline solutions.

7. Concentrated solutions of neutral salt act in regard to water in relation to the electrical effects produced, as the acids do in relation to bases.

8. The acids, in their combination or mixture with other acids, behave in such a manner, that the most oxidising acids are the most electro-positive; the acids in their combinations with the bases appear to retain the same property, so that in the reaction or mixture of two saturated solutions of neutral salt, the nitrate is positive in relation to the sulphate, and the sulphate in relation to the phosphate, &c.

9. When several acid, neutral, or alkaline solutions are placed beside each other, so as to mix very slowly, the electrical effects produced are the result of individual effects, which take place at each surface of contact.

10. Contrary to the opinion of Volta, we may form an electrical chain, or rather a closed circuit, solely with liquids, in which circulates an electric current, and whence result phenomena of decomposition and recomposition, if there exist in this circuit corpuscules which are conductors of electricity. Living organised bodies present numerous examples of this kind of circuit, capable of giving rise to electrochemical effects which have not yet been studied.

Comptes Rendus, No. 17, April 24, 1854.

Thermo-Chemical *Investigations Concerning* Combinations *formed in* Mutiple Proportions. By M. P. A. Favre.

(Concluded from page 494.*)*

Summary and Conclusions.

I have condensed into a table which will be found at the close of this article the calorific numbers obtained for the various combinations which I have studied. I have also included in this table, in view of the numbers consigned in this memoir, a few results lately obtained by other physicists. A glance at this table will enable us to verify most of the consequences which I think may be drawn from my experiments.

I may observe, in the first place, that the combination of two metalloids in one or several proportions brings into play a certain quantity of heat, which is most ordinarily represented by an elevation of temperature.

The compounds of chlorine and oxygen are among those which form exceptions to the ordinary rule; their formation gives rise to a considerable *absorption* of heat. It may be remarked that the bodies which present this exception *are not capable of being united directly.* We are permitted to think that if we could obtain one of the elements in the state which constitutes its aptitude for combination, the union of the two elements would take place with disengagement of heat, and, consequently that the separation of these same elements would be accompanied by an absorption of heat. If, in reality, chemical segregation is accompanied by a production of heat, it must doubtless be attributed to the constitution of the elements in a molecular state quite different from that which constitutes aptitude for combination. Such might be for example, the transformation of oxygen into *ozone* or *electrised oxygen.* Thus the oxygen leaving certain combinations, might disengage, in being modified, a quantity of heat which would exceed that which would be absorbed by the chemical segregation.

The disengagement of heat which accompanies the decomposition of the oxygenous acids of chlorine accounts for the violence of the explosions which accompany this chemical segregation. Indeed, the decomposition is operated at a rather higher temperature, and engenders gases whose volumes would not differ sufficiently in primitive volume to account for the expansive force observed, if this expansive force was not increased considerably by the elevation of temperature arising from the decomposition itself.

I have shewn that these phenomena must have an influence on the properties possessed by gunpowders made with chlorate of potassa; for the high temperature which is produced by the deflagration of these powders, has a necessary ratio to the rapidity of their deflagration and their fulminating properties. I may remand the reason that the elements determined in this work, joined to those already published by M. Silbermann and myself, enable us to calculate the quantity of heat developed by a known weight of various powders, when the chemical nature and the proportion of the products of the combustion are known.

It is very probable that the study of the calorific phenomena, which

accompany the decomposition of the fulminates, will give rise to the demonstration of phenomena of the same kind as those which belong to the chlorates, that is to say, to a considerable disengagement of heat.

I will here call to mind that it results also from experiments detailed in this memoir, that binoxide of nitrogen, in being transformed into nitrous acid, gives rise to an *absorption* of heat. M. Silbermann and I have already proved that nitrogen and oxygen, in uniting to give rise to protoxide of nitrogen, *absorb* heat.

It will not be without interest to remark that binoxide of nitrogen is a compound, comparable, as regards stability, with hyponitric acid, and, like it, resembling an elementary radical in many of its reactions. Protoxide of nitrogen and nitrous acid, on the contrary, are unstable bodies and never act like radicals. There would therefore be an analogy between binoxide of nitrogen and nitrogen itself, as regards the calorific effects observed, when they both pass to the subsequent degree of oxidation.

Conclusions.

I. In general, the quantities of heat disengaged by the successive compounds, resulting from the union of oxygen with a metalloïd, go on increasing in proportion as the oxidation rises; this is what occurs with the oxygencus compounds of phosphorus, arsenic, sulphur, and selenium.

II. The compounds of nitrogen and oxygen do not present the same phenomena; they may give rise alternately to an *absorbtion*, or a *disengagement* of heat, in proportion as the oxidation rises.

III. Without wishing yet to formulise absolutely a law relative to the heats disengaged by the various metalloïds with oxygen, I will remark that, from the experiments hitherto made, the heats disengaged by the equivalent of the same metalloïd, in its combinations in multiple proportions with oxygen, are perceptibly in the direct ratio of the number of equivalents of oxygen contained in the successive compounds.

IV. An analogous ratio is not manifested when we compare the calorific equivalents of the chloruretted compounds of the same metalloïd with the number of equivalents of chlorine contained in the compounds. Observation, as yet, has only led to the surplus over the chlorides of phosphorus.

V. Whilst the calorific equivalent of a metallic chloride is always considerably higher than the calorific equivalent of the corresponding oxide, the reverse phenomenon is observed in regard to the combination of the metalloids; thus, the calorific equivalents of the chlorides of phosphorus and chloride of arsenic are lower than the calorific equivalents of the corresponding oxygenous compounds.

VI. The heat disengaged by the formation of phosphorous and phosphoric acids is about double that which accompanies the formation of arsenious and arsenic acids. It is the same with the acid compounds of sulphur, compared with those of selenium. All the properties of arsenic shew a less affinity for oxygen than that of phosphorus; it is the same with selenium compared with sulphur.

VII. In point of view of calorific equivalents, there is no similarity between the comparison of phosphorus acid with protochloride of phosphorus and that of arsenious acid with that of chloride of arsenic.

VIII. The experiments detailed in this memoir furnish different thermic numbers, both for phosporus and for sulphur, according to the state which these metalloids present.

Indeed the heat of combustion of red phosphorus is considerably less than that of *ordinary* phosphorus. In fact, the calorific equivalent of dissolved phosphoric acid is

181230 units, setting out from red phosphorus.
109476 „ „ „ ordinary „

Thus, red phosphorus, far from being comparable to soft sulphur, the production of which is accompanied by an absorption of heat, would, on the contrary, be comparable only to ordinary sulphur.

In being changed into red phosphorus, ordinary phosphorus should therefore disengage a considerable quantity of heat, in all probability 911 units per grm., or 28246 per equivalent.

IX. The sulphur insoluble in sulphuret of carbon presents a remarkable state, in which its heat of combustion differs considerably from that of native sulphur. It would result from these experiments, that ordinary sulphur, in being changed into insoluble sulphur, should disengage 194 units per grm., or 3102 per equivalent.

The sulphur, insoluble in sulphuret of carbon, would be to ordinary sulphur, as regards thermic effects, what red phosphorus is to ordinary phosphorus.

X. The peculiar modification of sulphur which may be kept for a long time *in the oily state* at the ordinary temperature, when this sulphur arises from the decomposition of an alkaline hyposulphite by an acid, involves a very considerable absorption of heat; this heat again becomes free, when the sulphur resumes the pulverulent state. The thermic effect which accompanies this modification should be higher than 194 units per grm.

CALORIFIC EQUIVALENTS OF COMPOUNDS OF THE METALLOIDS.

	Units of heat disengaged or absorbed by the formation of the equiv. of the compound (H=1.)			
	Abria.	Andrews.	Despretz.	Favre.
Hypochlorous acid in solution	.	.	.	7870
Chloric acid in solution	.	.	.	65234
Phosphoric acid in solution with red phosphorus	.	.	.	181230
Phosphoric acid in solution with ordinary phosphorus	.	.	.	209476
Anhydrous phosphoric acid by ordinary phosphorus	181408	183904	.	190490
Phosphorous acid in solution by ordinary phosphorus	.	.	.	140394
Phosphorous acid dissolved, becoming phosphoric acid dissolved	.	.	.	69082
Hypophosphorous acid dissolved by ordinary phosphorus	.	.	.	48302
Id. becoming phosphoric acid dissolved	.	.	.	161174
Proto-chloride of phosphorus by ordinary phosphorus	.	.	.	94804
Perchloride of phosphorus by ordinary phosphorus	102368	109504	.	100373
Opaque arsenious acid in solution	.	.	.	71690
„ „ solid	.	.	.	75363
Vitreous „	.	.	.	76694
Arsenic acid in solution	.	.	.	110787
Opaque arsenious acid in solution, becoming arsenic acid dissolved	.	.	.	39097
Chloride of arsenic $AsCl^3$	.	74550	.	71883
Protoxide of nitrogen	.	.	.	—8724*
Conversion of binoxide of nitrogen into dilute nitric acid	.	.	.	20655
Conversion of binoxide of nitrogen into dilute nitrous acid	.	.	.	—6614
Conversion of nitrous acid into nitric acid	.	.	.	27269
Dilute hyposulphurous acid	.	.	.	40467
Decomposition of dissolved hyposulphurous acid into SO^2+S oily	.	.	.	—1097
Id. „ SO^2+S pulverulent	.	.	.	2707
Sulphurous acid in solution	.	.	.	39373*
Id. anhydrous	.	36912 flower of sulphur	.	35550*
Passage of dissolved sulphurous acid to the state of dissolved sulphuric acid	.	.	.	27839
Sulphuric acid in solution by native sulphur	.	.	.	67212
Id. by S insoluble in CS^2	.	.	.	64110
Id. „ S oily	.	.	.	67211
Id. „ S pulverulent of the hyposulphites	.	.	.	63407
Selenious acid in solution	.	.	.	23206
Selenious acid dissolved becoming dis. selenic acid	.	.	.	13154
Selenic acid in solution	.	.	.	36360
Oxide of carbon	.	.	.	14838*
Conversion of oxide of carbon into carbonic acid	.	.	.	33642*
Carbonic acid	.	46068	47478	48480*
Conversion of oxalic (carbonous) acid into carbonic acid ($CO^{3/2}+O^{1/2}$)	.	.	.	15070
Oxalic acid in solution ($CO^{3/2}$)	.	.	.	33410

N.B.—The numbers to which an asterisk is affixed relate to experiments made in common with M. Silbermann, in a former work.

TOXICOLOGY.

How FAMILIES *are* POISONED.—SCHEELE'S GREEN *in* BAKERS' SHOPS.

THE fact which I am about to mention shows in what an insidious manner arsenic may be introduced into articles of food supplied to a family. Cases now and then occur to a medical practioner in which certain symptoms, strongly indicative of irritant poisoning, appear simultaneously in several members of a family. No source of poisoning can be discovered, no motive may exist for its administration; the persons affected recover, and the matter is forgotten. In another and more serious class of cases, several members of a family die suddenly, one after another. Poison is suspected to have been the cause of death, and traces of it may be actually found in the body; but it will be impossible to discover how or when it entered the body. In a case which I was required to investigate in January last, the father, mother, and three children were carried off within the short period of about ten days; as it was snpposed, by disease. The body of the mother was exhumed a month after interment, and arsenic was found in the liver. This created a strong suspicion that all had died from poison; but the most minute inquiry has failed to show how or in what way the arsenic could have entered the body of the mother. Nevertheless, it was quite clear that she must have taken it during life, as it had been absorbed, and was found deposited in the liver.

On the 17th of the present month, when about to cut the loaf on my table for breakfast, I observed some green patches and streaks over the partially-burnt under-crust. The appearance was something like that caused by green mould; but, on examining the slices of toast in the toast-rack, I found similar green spots in the depressed portions of the crust of the toast. I then examined the green spots with a lens, and subsequently a portion was scraped off, and examined under a microscope of low power. The colored particles then resolved themselves into a mineral powder, having all the appearances of Scheele's green, or arsenite of copper. I scraped from the bottom of one loaf from one and a-half to two grains of this powder; and, on applying Reinsch's process, I separated a quantity of metallic arsenic, sufficient to coat two square inches of copper. The metallic arsenic was converted to crystallized white arsenic, and examined under a high magnifying power, when the arsenious acid was plainly perceptible. The green substance adhering to the crust of the bread and toast, was therefore proved to be Scheele's green, a pigment which contains about 50 per cent. of white arsenic.

On inquiring of the servants, I found that five loaves had, the day before, been taken from the baker, a respectable tradesman, with whom I have dealt for twelve years. On examining these loaves, three of them were stained in the under-crust with closely adherent patches of the green arsenite of copper. It appeared clear from the circumstances, that there had been no intentional admixtnre of poison, or this would have been found inside and not on the outside of the

bread. It was quite certain that the loaves must have been delivered with the poison adhering to them, because there is no green paint in the house.

Thinking that some accident might occur in other families, I immediately went to the baker, and showed him the Scheele's green and metallic arsenic extracted from it. On entering his shop, I at once saw the cause of the poisoning of the bread. He had had his shop newly-decorated, and there were eight long shelves (holding some hundreds of loaves) painted of a bright grass-green color, top, front, and sides. They had been evidently very substantially coated with the green paint. The warm loaves, placed on the recently painted shelves, had imbibed a quantity of the poisonous pigment, which had dried in the under-crust. He took down five or six loaves in succession, and all were found stained with the arsenite of copper. The baker assured me that he had not the least idea there was arsenic in the green paint; but the painter, who was summoned, took the matter more coolly and professionally, remarking that it was impossible to get a good green without arsenic, that he had used it for many years, and had never heard of any one being killed by it. Although he knew the shelves were for the purpose of storing loaves of bread, he was evidently waiting for some fatal cases in his own experience before he could make up his mind to lay aside the practice of using this paint.

The baker, at my strong request, had the whole of the loaves rasped, and, without loss of time, the tops of the shelves were planed off, so that the loaves should rest upon nothing but plain and unpainted deal. There was enough on the crust of one of the loaves to have killed an infant; and I can suppose a case in which a nursemaid, about to quit a service for misconduct, might be seriously implicated by such an occurrence as this, unless the cause was at once traced.

In foreign countries there are strong restrictions placed on the sale and use of this noxious pigment. In England, however, green sweetmeats are actually colored with it; and lemon acid, and other drops, are sometimes sold, wrapped in paper coarsely tinged or dyed green with this dangerous compound of arsenic.

I am, &c.,

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15, *St. James Terrace, Regent's Park, March* 29, 1854.

Pharmaceutical Journal, May, 1854.

THERAPEUTICS.

On the EMPLOYMENT *of* TORULA CEREVISIÆ *in* DIABETIS MELLITUS.*

BY W. BIRD HERAPATH, M.D., London, F.R.S., Edinburgh.

IT has been abundantly shewn, that the morbid product in diabetes is the presence of a saccharine substance in the urine, of a low

* Read at the Quarterly Meeting of the Bath and Bristol Branch, March 25th, 1854.

character, perfectly identical with the sugar of grapes and other analogous fruits. The source of this sugar or glucose has been traced to the mal-assimilation of the starchy or amylaceous element of diet, which substances, as soon as they have entered the stomach of a patient suffering from this disease, are at once converted into glucose. This is as rapidly taken into the current of the circulation by the venous system, and more slowly by the lacteals and thoraic duct, and is there eliminated by the kidneys, together with an abundant excretion of water. The name of glucosuria has therefore been most appropriately given to the disease.

In healthy digestion it appears probable that starch, although previously converted by the saliva into glucose, is further metamorphosed in the stomach into lactic and acetic acids. The former predominates; hence the amylum of the food is the probable source of most of the lactic acid of the gastric juice; which has been shown by Lehmann to owe its digestive powers in great measure to the presence of hydrochloric and lactic acids.

In diabetes, therefore, it is evident that the conversion of starch stops in the metamorphic series at glucose, a substance highly diuretic and abnormal to healthy blood, and which, appearing to be incapable of undergoing further metamorphosis in the circulating current, and being useless in the production of adipose tissue, and also incapable of subserving to the process of respiration, is eliminated from the system as an effete substance, constituting the most evident sign of the diseased condition of the patient.

Although the quantity of sugar is materially lessened in diabetic cases by the abstraction of all the starchy elements of diet, yet it does not wholly diappear; and some recent experiments point to another source of this morbific matter, but at present we are not in a condition to say more than this, that starch is "*the principal* source of sugar in glucosuria, if not the only one."

We are hence led to the following conclusions: either 1. That the existence of sugar is an abnormal product in the digestive process; or, 2. That, if produced, it should be merely a transition state to some other proximate element of less stability, and nearer to the ultimate element of its assimilation; either lactic acid on the one hand, or adipose tissue or carbonic acid and water on the other.

We have merely to recollect with what facility certain chemical processes change glucose, and induce it to continue in metamorphic condition. Of all these processes, that by means of certain vegetable agencies appears to be the most suited for the purpose we have in view—namely, the prevention *of the arrest of the conversion of starch at the stage of sugar*, when undergoing the process of digestion.

It has lately been shewn, too, that certain vegetable organisms have the power, whilst endowed with vitality, of resisting the digestive powers of the stomach. The sarcina ventriculi may be mentioned as a case in point. It therefore easily follows, that the torula cerevisiæ may be presumed to have an effieient action within the stomach, similar to its power of inducing fermentation in saccharine fluids out

of the body, if it were capable of resisting the assimilative powers of this viscus for a sufficient length of time.

These views led me to employ the torula in diabetes; and I am happy to say, that in the only case in which I have yet had an opportunity of trying this remedy, it answered fully the expectations which I had previously formed of it. Under ordinary circumstances, glucose, at a temperature of 60° to 70° F., would be converted into alcohol and carbonic acid by the fermentative agency of torula cerevisiæ or yeast, as is very well known to most persons; but if the action were to take place in the dark, in the presence of albuminous substances or other protein compounds, and at a temperature of 98° F., the products would be lactic and acetic acids, with possibly alcohol and carbonic acid.

It is clear, therefore, that the former products would assist in the conversion of the protein compounds, as in the normal state of digestion, and would pass out of the system in combination with some alkaline or earthy base, and be eliminated by the kidneys, skin, or other emunctories; whilst the alcohol and carbonic acids would act as agreeable vital stimuli, whilst existing in the system, and could eventually escape by the pulmonary mucous membrane, after having perhaps served to assist the respiratory process, being thus converted into carbonic acid and water.

It is known that the yeast-plant, during fermentation, undergoes a progressive growth and decay; and that after long action it becomes spent and exhausted, when it is found that the cells of which it consists have become ruptured, and the vitality destroyed. Presuming, therefore, that the yeast plant became no longer capable of continuing the process of lactic or alcoholic fermentation in the stomach, it would die; and then would itself become a nutritive nitrogenous substance, like any other article of vegetable diet.

In January 1853, I determined to put these views in practice; and having a patient then under my care, I commenced the administration of the remedy. Before the treatment, the patient had been voiding urine of specific gravity 1044, and containing 850 grains of sugar in the imperial pint. Within two days of the employment of this substance, the specific gravity sank to 1020, and the sugar to 300 grs. per pint. By steadily pursuing the same course during six weeks, the sugar eventually disappeared altogether; the urine assumed its healthy character; and the patient lost all symptoms of its ailment, regaining his usual health and strength. Since that time he has rapidly recovered his flesh.

It was exhibited, in this highly satisfactory case, in that form in which it is usually employed by confectioners, and known by them as German yeast. This is prepared on the continent by expressing ordinary yeast in linen bags, by which means all the fluid portions are separated, and a semi-solid mass remains, consisting entirely of the active vesicular structure of the plant. This is packed in bladders like lard, and keeps fresh for some time. Supplies are sent to the

London confectioners regularly twice a-week; and it can be always obtained from the trade in any large town, at a very moderate price—about sixteen-pence per pound.

The dose given in this case was one tablespoonful twice or three times daily, in milk. It was thus rendered very palatable. At first, the only inconvenience felt was a slight nausea, sometimes passing into actual vomiting, with eructation of carbonic acid probably; by giving the remedy after a meal, these disagreeable symptoms disappeared, and it was then borne very well. It was further serviceable in removing the obstinate constipation which had previously existed, and produced regularity in the alvine evacuations.

As diabetes is a comparatively rare disease in private practice, it is to be hoped that by communicating these results, even in their present incomplete shape, it may be the means of hastening the progress of the inquiry, and thus establish the real value of the remedy, as the attention of numerous experimenters will be called to the investigation, and the causes of success or of occasional failure will be at length ascertained.

Ass. Med. Journ., April 28th, 1854.

PHARMACY.

Observations *on the* Preparation *of* Perchloride *of* Iron. By M. Gobley.

Perchloride of iron had been almost abandoned as a therapeutical agent, when M. Pravaz, a distinguished chemist of Lyons, made known a new method for the treatment of aneurisms and varicose veins, which consists in injecting into the arteries or veins a few drops of a concentrated solution of this salt. Under the influence of this liquid, all the blood for an extent of 3 or 4 centimetres is transformed, in a few minutes, into a solid clot. This new mode of treatment is, however, still in the experimental stage.

The real action of perchloride of iron on the blood has not yet been ascertained. According to some, it coagulates all the elements of the blood into a mass; according to others, it acts especially on the fibrine; others again assert that it acts only on the albumen.* The nature of the clot has yet to be determined, whether the perchloride is a simple hemostatic or a hemospasic, whether it acts as a caustic irritant, or as a toxical agent, and what is the limit of the dose to be used.

We see by these questions, which have recently been laid before the *Académie de Médicine*, what importance should be attached to the preparation of perchloride of iron. M. Burin du Buisson, who, with

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* M. Burin du Buisson has ascertained that ten drops of the solution of perchloride of iron marking 45°, mixed with the white of an egg diluted with 30 grms. of water, was sufficient to reduce the whole into a mass, which, on reversing the vessel containing it, remained adherent to the bottom, and did not become detached for a considerable time.

M. Pravaz, made the first experiments of the new method of which we speak, prepared their chloride in the following manner: the reaction is allowed to continue without heat for five or six hours, the vessel is then placed in boiling water, and heated until 200 grms. of peroxide of iron calcined at a red heat, are nearly dissolved in 1000 grms. of pure, white hydrochloric acid; this operation should be performed in a porcelain capsule of known weight; the liquid is decanted to separate the undissolved oxide, and it is carefully evaporated in a sand-bath, continually stirring it until it attains the consistence of a thick syrup, the weight of which is to be ascertained. Half of this weight of distilled water is then added; it is heated for a few minutes longer and thrown on a filter; the capsule is washed, then the filter, with an equal quantity of fresh water; and to the first liquid sufficient of the latter is added to obtain a liquid of the density of 43·5 to 44°.

It has been said that the chloride thus prepared retains too large a proportion of hydrochloric acid. If, instead of ceasing the evaporation, as indicated by M. Burin du Buisson, it is continued for a longer time, a further quantity is removed. It is likewise well known that there is a difficulty in keeping a solution of perchloride of iron for long, without a deposition of insoluble oxychloride, and without its becoming, as a consequence, more acid. To obviate these two inconveniences, M. Burin du Buisson, in a Memoir recently addressed to the Academy of Sciences, proposes the substitution of the following process for that formerly given by him. Pure, white hydrochloric acid is to be saturated as far as possible with hydrate of peroxide of iron, and the liquor is to be evaporated to rather less than one half over a gentle fire; then the evaporation is to be continued in a sand-bath, taking care to avoid the aqueous vapors, which would give rise to the formation of hydrochloric acid, and the deposition of an insoluble oxychloride. When the liquid has attained the consistence of a thick syrup (in this state it coagulates on cooling without becoming quite a solid mass), the evaporation is stopped, and an excess of gelatinous hydrate, diluted with a little water, is added to the liquid; it is stirred for a quarter of an hour, and the mixture is then left to repose for several hours. The quantity of distilled water necessary to bring it to the density of 30° Baumé is then added, and it is left in contact with the excess of hydrate for eight days, after which it is filtered, and the filtered liquid is allowed to stand for fifteen days. This density of 30° is that which appears to be preferred by MM. Valette, Desgranges, and, Petrequin, for the treatment of varicose veins. For the cure of aneurisms these skilful chemists appear to consider a density of only 20° or even 15° preferable.

This new method of preparing the solution of iron does not sensibly differ from the former. The contact of the liquor with the gelatinous hydrate does not remove the hydrochloric acid as well as evaporation; this fact has been proved by M. Burin du Buisson himself. Moreover, M. Burin du Buisson has ascertained that the solution of perchloride of iron, even when prepared with sublimed chloride, always possesses an acid reaction. If it is impossible to prepare solutions of perchloride of

iron without containing free acid; and if it is important that they should be as far as possible deprived of it, would it not be preferable as proposed by M. Soubeiran, to have recourse to the process which I described in 1844? The iron solution then becomes a magistral preparation, which the surgeon prescribes as it may be wanted. The dry chloride, moreover, may be kept for a long time without becoming decomposed; it gives with water perfectly limpid liquors, which may likewise be preserved for some time without any sensible alteration taking place in them. The chloride of iron prepared with hydrochloric acid is chemically pure, and more certain than that obtained with the gelatinous hydrate; it likewise contains less free acid. I may even assert that it does not contain even a trace; for when in its preparation the evaporation is continued beyond the time required to produce a solid consistence on cooling, it is remarked that it decomposes into hydrochloric acid and oxychloride. The liquor so much praised by M. Burin du Buisson is only slightly acid when just prepared, but it soon becomes more so by the deposition of oxychloride. It is true that, as he says, it is to this excess of acid that it is indebted for remaining so long without any perceptible precipitate; but then, to avoid this acidity of the liquor, would it not be better to use, as I have said, the extemporaneous solution of the dry chloride?

There is one difficulty in the employment of this latter salt, for the quantity of solution required for an operation is always very small; I have overcome this by determining by experiments the proportion of chloride of iron and water necessary to form the solutions to the degree of density which surgeons may desire. The numbers to which I have arrived, by Baumé's areömeter, were as follow:—

	Chloride.	Water.
45° . . .	53·85 . .	46·15
30° . . .	34·65 . .	65·35
20° . . .	21·30 . .	78·70
15° . . .	16·35 . .	83·65

The dry chloride contains exactly one-fifth of its weight of water; it will therefore always be easy to calculate the quantity of anhydrous chloride contained in the iron solutions. Thus, those at 45°—30°—20°—15° contain respectively 43·10—29·70—17·05—12·10 per cent.

MM. Pravaz and Burin du Buisson consider the solution of chloride of iron as a valuable hemostatic. I have seen it succeed in various kinds of hemorrhage. It acts especially as a powerful astringent, strongly contracting the tissues of the organism. There can be no doubt that, in this respect, chloride of iron will be of very great service in medicine and surgery.

Journal de Pharmacie et de Chimie, April, 1854.

MR. WILLIAM ACKLAND'S SHORT AND EASY METHOD OF MAKING DISCOVERIES AND INVENTIONS.

*To the Editors of "*THE CHEMIST.*"*

GENTLEMEN,—Among the "original" communications contained in your journal for May, I perceive a paper entitled "*Centigrade Testing and the Preparation of Test Solutions*, by WILLIAM ACKLAND." As you, no doubt, published that paper in the belief that it contained a genuine narrative of the inventions of its writer, and as the paper is grossly unjust to me, I beg leave to be allowed to shew, that you, to whom it was sent for publication, and the Society before which it was read, have been deceived.

Mr. Ackland's pretended "original" communication embraces four principal subjects:—1, A decimal division of the imperial measures; 2, a peculiar set of graduated measures adapted for centigrade testing; 3, a method of preparing standard test-liquors; 4, the application of these contrivances to the rapid analysis of impure acids and alkalies.

These four discoveries, such as they are, belong to *me*, not to Mr. Ackland. He was in my employment for three years, during which time I taught him all that he knows upon the subject. Having left my employment, and entered into that of Messrs. Horne, Thornthwaite, and Wood, he publishes, for money-making purposes (*vide* the advertisements in your Journal), as *original discoveries of his own*, the information for which he is indebted to me. He thus *uses* the Society before which he reads his paper, and the Journal to which he sends it, to cloak with their respectability his spurious inventions.

The proof of my statement is easy. Ten years ago I published, in Glasgow, an account of:— 1, The decimal division of the imperial measures; 2, the system of graduaded measures for testing; and 3, the method of preparing standard test-liquors. A more particular account of these matters was afterwards given in the edition of my "Chemical Recreations," that was published in 1847, and yet farther details in the tenth edition of that work, published last January, and in various papers read before the Chemical and Pharmaceutical Societies. In the year 1843 I wrote a work on *Centigrade Testing*, for the use of chemical manufacturers, and I had it printed to the extent of nearly 100 pages. Want of health and a pressure of business prevented the completion of that production, and it has not yet been published. A copy of it was placed in Mr. Ackland's hands, while in my employment, for purposes connected with my business, and some of the details contained in his communication to you, and which do not appear in my published works, have been appropriated by him—of course, without permission—from those printed, but not published instructions. Other matters of fact, of which I have yet published no description, but on which I had collected information, have been, in like manner, published by Mr. Ackland as original discoveries due to his own fertile genius. Thus, he says—"The divisions on the chlorimeter are different from those USUALLY *employed*; this alteration is INTRODUCED to obviate the necessity of the calculations attendant on the OLD plan." The divisions thus introduced by Mr. Ackland, as novelties, are those of the *Burette réciproque* of M. Gay-Lussac, invented by that eminent chemist at least thirty years ago. A copy of that instrument, made by M. Collardeau of Paris, I placed, with other standards, in Mr. Ackland's charge while he was in my employment, and thus afforded him the opportunity of "inventing" it.

The method of preparing and using equivalent test-liquors, may perhaps be considered so simple and obvious as to render a controversy about the priority of discovery needless; but I may reply, that it is like the famous experiment with Columbus's egg, very easy when it is known, but not till then. Permit me to cite one or two illustrations of this point.

M. Gay-Lussac, certainly the most able experimenter in this department of chemistry, had no *general plan* for preparing test-liquors. His standard for *chlorimetry* was a litre of chlorine gas, absorbed in a litre of solution, at the temperature of 0° C, and under the barometric pressure of 0·760 M. This unity he divided into 100°. His standard for *alkalimetry* was five grammes of concentrated sulphuric acid, diluted with water to the bulk of fifty cubic centimetres, and this

divided into 100°. These standards have no common relation to the atomic weights of the two substances, nor to the atomic weights of any other substance; and it was impossible for a chemical manufacturer to apply such a mode of testing beyond the special cases for which the test-liquors were prepared.

Dr. Andrew Ure, scarcely less celebrated in Britain than M. Gay-Lussac was on the Continent, for skill in Centigrade Testing, has recently published the following instructions for the systematic preparation of test-liquors;—

"TEST LIQUORS.—To reduce an alkaline, acid, or a neutral saline solution of a certain strength to one of any other strength. Let a=the given strength per cent. of one liquid; b=100; c=the desired strength; x=the volume of the diluted solution.

"*Example.*—Let an alkaline solution contain 40 per cent. of alkali; if it is to be reduced to one containing 24 per cent., then the above formula gives $\frac{ab}{c}=x=\frac{4000}{24}=166{\cdot}6$; hence, if 100 measures of the liquid a be diluted into 166 measures, it will then contain 24 per cent."—*Dictionary of Arts, Manufactures, and Mines*, Fourth edition, 1853, vol. ii. p. 820.

These instructions are entirely fallacious. Test-solutions cannot be prepared by this process, which could give a true result only when the "alkaline, acid, or neutral saline" substance contained in the solution, had under all circumstances, the same specific gravity as water, and could be diluted to any extent without experiencing either expansion or condensation. As no such substance exists, this method of preparing test-liquors, the result of Dr. Ure's long experience, is of impossible performance.

These examples shew that the construction of an universal system of centigrade testing, and of preparing test-liquors, is not so easy a matter, as it might seem to be, to one who merely looks at such a system when it is finally brought into working condition.

The system which, in the works referred to, I have recommended for preparing test-liquors, is this:—To fix upon the tenth part of a gallon (=1 decigallon=1 lb. of water) as the standard of liquid measure, and upon 100 grains of oxygen as the standard of chemical strength. A *test atom* of any substances, namely, 100 *grains* of oxygen, or its equivalent of any other chemical substance, brought into solution in water, so as to measure one decigallon at 62° F., makes a solution of 100°. All solutions of chemical compounds, so prepared, are equivalent to each other.

This fundamental idea, with the details of the process for putting it into practical operation, in reference to commercial and manufacturing processes, Mr. Ackland learnt from me while in my employment; and when he publishes this system as being *a discovery of his own*, he plays the part of the jackdaw in borrowed plumes.

It is true, indeed, that, with a view to cover his plagiarisms, he has made a few trivial alterations in the details; but the entire system of testing which he has concocted into the "original communication" addressed to you, is substantially mine. Even the pictures of apparatus which adorn his paper are copied from mine. There is not a single piece of apparatus, not a test-liquor, not a process, not a single bit of information of any kind, given in his entire communication, other than is contained in the sources of information to which I have referred, excepting the following particulars, of which, in justice to Mr. Ackland's powers of original discovery, I wish him to have the full benefit.

1. He has given *new names* to my decimal measures. What I called a *decigallon*, he calls a decem. The thousandth part of the decigallon, which, as it contains seven grains of water, I called a *septem*, he calls a decimillem.

2. He recommends the operator to use a separate pipette for every liquor that is to be tested—a contrivance of no other use than to make work for the instrument-maker.

3. Finally, he "introduces" Gunter's sliding rule for performing short sums by the Rule of Three; and he assumes that the virtues of this instrument are now for the first time made known to chemists.

The following is an example of the profound problems that this sliding rule is intended to solve;—

"How much muriatic acid of 80 per cent. is required to make, when diluted with water, 56 lbs. of muriatic acid of 10 per cent?"

As 56 lbs. of acid of 10 per cent. evidently contain 5·6 lbs. of real acid, and as we have acid of 80 per cent. for use, the longest possible calculation required to solve this problem by common arithmetic is as follows:—

$$\frac{5{\cdot}6 \text{ lbs.}}{{\cdot}8 \text{ lbs.}} = 7 \text{ lbs.}$$

This calculation is *simplified* by the sliding rule, thus:—

"Set 10 on E to 560 on D; then against 80 on E will be found 70, the answer on D; but as we must in this case call 560 fifty-six, we must also call 70 seven; therefore, 7 lbs. of the strong acid will make 56 lbs. of the strength required."

That is the way in which the sliding rule is "to disseminate scientific information without calculation."

But the sliding rule has other and less innocent duties to perform. By means of it Mr. Ackland manages to set manufacturers astray in this style:—

(The problem for solution is this;—To determine the comparative money value of two samples of the same acid of different per centages.)

"Sulphuric acid, of 90 per cent., is worth twopence per lb.; what is the value of an acid of 36 per cent.? Set 90 on B to 20 on A; then against 36 on B will be 8 on A; and as in this case we represent the 20 on A as equal to twopence, 8 is, therefore, eight-tenths of a penny; the acid is, therefore, eight-tenths of a penny per pound."

It is sometimes a question with a chemical manufacturer whether he should buy sulphuric acid in the weak state, as it is drawn from the lead chambers, or in the concentrated state to which it is brought by the evaporating pan or the platinum still. The vitriol maker quotes two per-centages and two prices, and Mr. Ackland's incomparable rule is to guide the buyer's choice between them. Woe to the manufacturer who trusts to such guidance!

Sulphuric acid, containing 90 per cent. of oil of vitriol, has an atomic volume of 54. The acid, which contains 36 per cent., has an atomic volume of 198. Hence, as much real acid as in the first case fills 54 carboys, will in the second case fill 198 carboys. We ought, consequently, to take into consideration, as an element in the valuation of the two acids, the cost of bottles, freight, warehouse-room, and similar charges on 139 *extra carboys*. But, on this head, the rule for giving VALUE "without calculation" says *nothing*. I must do Mr. Ackland the justice to own, that this part of his system of testing is entirely original, discovered by him since he left my employment, and of which nothing is copied from my standards or my writings.

I am sorry, gentlemen, to trouble you with a communication on so unpleasant a subject; but as you have admitted Mr. Ackland's privateer to sail in your waters, I trust you will permit my cutter to follow him. This happens to be the second occasion on which he has publicly arrogated to himself scientific inventions that belong to me. On the first occasion I treated his pretensions with silent contempt; but since impunity has increased his audacity, I think it a duty to let the public know under what false colors this philosophical pirate ploughs the ocean of science. If his method of making discoveries, and writing "original communications," is tolerated, men of science and literary men will stand in entirely new relations to the public. In future, when Liebig, or Graham, or Faraday makes a discovery, we must not expect to have a genuine account of it from the philosopher himself, but a blundering "original communication" from the famulus who sweeps his laboratory, and Mr. Dickens's next new novel may be *originated* from rough proofs, and presented to us by his housemaid, or a discarded valet. An appreciation of the advantages which science and literature would derive from such a system, no doubt justifies, in Mr. Ackland's eyes, his endeavors to supersede the publication of the work on Centigrade Testing, which he knows too well I have spent so many years in bringing to perfection.

I am, Gentlemen,

Your obedient servant,

JOHN JOSEPH GRIFFIN.

10, Finsbury Square, London, 20th May, 1854.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the ADULTERATION *of* MALT LIQUORS *with Cocculus Indicus; with* REMARKS *on the* DETECTION *of* PICROTOXIA. By THORNTON J. HERAPATH.

IT is, I believe, a well-known fact that many brewers and publicans are in the habit of making use of the extract of *cocculus indicus*, or berries of the *Menispermum cocculus*, as a means of imparting a false strength to their liquors. At least, such is the statement that has been made by many eminent authorities, though the publicans of Bristol, strange to say, have recently* repelled the charge with the most virtuous indignation. How far, however, they are justified in so doing, the following extracts from the works of some of our most celebrated chemists will show:—

Dr. Ure (*"Dictionary of Chemistry,"*—article *"Beer"*):—

"Writers who profess to discuss the secrets of the trade mention most of these (*c. indicus, essentia bina*, &c.), and some others, as essentially necessary" in the manufacture of beer.

Professor Christison (*"Treatise on Poisons"*):—

"It (the *cocculus*) is a familiar poison for destroying fish, and has also been extensively used by brewers, as a substitute for hops."

Dr. Pareira (*"Elements of Materia Medica"*):—

"An extract of *cocculus indicus*, called '*black extract*' or '*hard multum*,' is occasionally used by dishonest dealers to augment its (the porter's) intoxicating quality."

Professor Taylor (*"Medical Jurisprudence"*):—

"London ale and porter are considered, and in some instances with propriety, to owe their intoxicating properties to a decoction or extract of these berries (*c. indicus*); a fraud not readily susceptible of detection."

Mr. Accum (*"Treatise on Adulterations of Food"*):—

"Every person is aware that bread, beer, wine, and other substances employed in domestic economy, are frequently met with in an adulterated state. * * * To increase the intoxicating quality of beer, the deleterious substance called *cocculus indicus*, and the extract of this poisonous berry, technically called 'black extract,' or by some 'hard multum,' are employed. * * * The latter is ostensibly destined for the use of tanners and dyers. * * * The quantities of *c. indicus* berries, as well as of black extract, imported into this country, for adulterating malt liquors, are enormous. * * * There are particular chemists (druggists) who make it a regular trade to supply drugs and nefarious preparations

* See *Bristol Mercury* for May 13th and 27th.

to the unprincipled brewers of porter and ale. * * * Most of the articles are transmitted to the consumer in the disguised state, or in such a form that their real nature cannot possibly be detected by the unwary. * * * An extract, said to be innocent, sold in casks, containing from half a cwt. to five cwt., by the brewers' druggists, under the name of '*bittern*,' is composed of calcined sulphate of iron (copperas), extract of *cocculus indicus* berries, extract of quassia, and Spanish liquorice. * * * This fraud constitutes by far the most censurable offence committed by unprincipled brewers; and it is a lamentable reflection to behold so great a number of brewers prosecuted and convicted of this crime. * * * From these statements, and the seizures that have been made of illegal ingredients at various breweries, it is obvious that the adulterations of beer are not imaginary." * * * "The fraud of imparting to porter and ale an intoxicating quality by narcotic substances," Mr. A. goes on to say, in another part of his work, "appears to have flourished during the late French war: for, if we examine the importation lists of drugs, it will be noticed that the quantities of *cocculus indicus* imported in a given time prior to that period, will bear no comparison with the quantity imported in the same space of time during the war, although an additional duty was laid upon this article.* * * * It was at this period to which we have alluded, that the preparation of an extract of *cocculus indicus* first appeared as a new saleable commodity. * * * * From that time, also, the fraternity of brewer's chemists took its rise. They made it their chief business to send travellers all over the country with lists and samples exhibiting the price and quality of the articles manufactured by them for the use of brewers only. Their trade spread far and wide; but it was amongst the country brewers chiefly that they found the most customers, and it is amongst them, up to the present day, as I am assured by some of these operators, on whose veracity I can rely, that the greatest quantity of unlawful ingredients are sold."*

Another author, in a work entitled, "The Beer Houses of England," observes :—

"There is annually imported into England a very large quantity (a good many years ago, and before the Beer Act was passed, which increased the importation, it amounted to many tons weight) of the fruit and berries of the plant *cocculus indicus* (properly the *menispermum cocculus*), a narcotic, acrid poison, which is not used by the apothecary, neither I believe, in manufactures, and is virtuously disowned by the brewers, large and small. What, then, becomes of this large annual importation of a vegetable poison? Nobody will acknowledge its employment, unless, it is true, the druggist, who does sell it occasionally in pennyworths to the idle and mischievous, for the purpose of poisoning fishes. Is not this circumstance, alone, of its being so universally disowned, suspicious? Experiment proves that the *cocculus indicus* berries produce effects on the sensorium exactly analogous to those of the variety of strong ales, porter, and beer, in proportion to their degree of potency. * * * * * The coincidence is remarkable."

And, lastly, Mr. Normandy, in his "Commercial Handbook of Chemical Analysis," remarks, after alluding to the Acts 56 Geo. III. and 1 Wm. IV., which relate to the adulterations of malt liquors,—

"This then is the law. In theory, it seemingly provides for everything; in practice, it is a dead letter. It is a well known and authenticated fact, that many dealers in or retailers of beer, in the verbose phraseology of the Acts, have in their possession, and do make use of, mix with, or put into their beer, liquors, extracts, preparations, calx, and all manner of things, except brown malt. It is a publicly known fact, that carts may be seen bearing the inscription in staring paint, of '—— brewers' druggist.' Such a cart I have myself seen, a few days ago, standing in broad day light, at mid-day, before a publican's shop or gin-palace."

Similar evidence also has been given by many practical brewers

* This extract is well worthy of attention from the public at the present moment, when war has again broken out, and an extra duty has been laid upon malt.—T. J. H.

who have written upon the subject. Up to the present time, however, no good test has been published by which the presence of this deleterious substance in adulterated beer can be discovered; indeed, the detection of the adulteration of malt-liquor with *cocculus indicus* has been hitherto considered to be almost beyond the reach of chemical analysis; but it is my intention in the present article, to describe a process which experience has proved will enable the analyst to detect this sophistication with almost as much readiness as he can now discover many of the other fraudulent adulterations of the principal articles employed in domestic economy. The process in question is based on the property which animal charcoal possesses of separating the alkaloïd or active ingredient of *coc. indicus* (picrotoxia) from its aqueous solution, and precipitating it (so to speak) in an insoluble state, thus enabling us to isolate it from most of the other substances with which it is associated in the beer.* The practical details of this process may be thus described:—The beer or other malt liquor to be operated upon is precipitated by an excess of acetate of lead, the lead precipitate is separated by filtration, and the excess of lead in the filtrate is thrown down by a stream of hydrosulphuric acid gas. The solution is then boiled to expel the excess of the sulphuretted hydrogen, and the insoluble sulphide of lead is removed by filtration. The clear solution is finally evaporated at a moderate temperature, until it becomes somewhat thick in consistence, when a small quantity of pure animal charcoal† is added, and the solution is well agitated for a short time with a glass rod, and allowed to cool‡. It is then thrown upon a filter, and the charcoal, which is thus separated, is washed with the smallest possible quantity of water, and dried in a steam-bath, at 212° F. The next step is to separate the picrotoxine from the char-

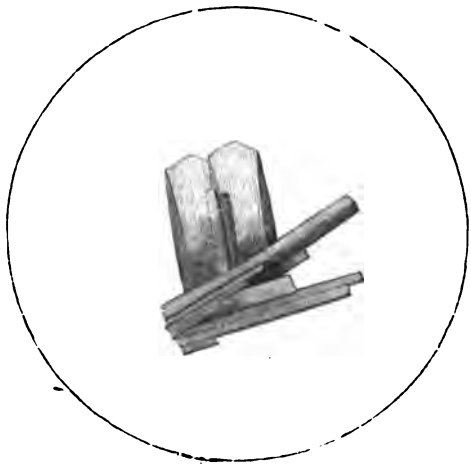

Fig. 1.

* By means of this test I may observe, that my father, Professor W. Herapath, has, in several instances, succeeded in detecting picrotoxia in the bodies of fish that have been poisoned with *coc. indicus*.

† The charcoal used in this process should consist of the ordinary bone-black of the sugar-refiner purified by digestion with hydrochloric acid, washed as long as any saline matter is abstracted, and then dried and heated to redness.

‡ Sometimes I find it is advisable to add a little caustic soda to the solution before boiling it with the animal black.

coal. This is effected by boiling the latter with a little pure alcohol, in which solvent the alkaloïd readily dissolves. On concentrating this alcoholic solution by evaporation, and then allowing the alkaloïd to crystallise spontaneously, the latter may be easily recognised by its peculiar form. If the picrotoxine is present in large quantity it separates from the alcoholic solution as fine, well-formed prisms (Fig 1.,) or when hurriedly crystallised, as beautiful foliated or plumose crystals (Fig. 2.). When the contrary, it occurs in but small quantity, it assumes the form of long, radiating needles, which, if the crystallisation is conducted, as in general, between two slips of glass, exhibit a peculiar tendency to place themselves nearly parallel with the edges of the *upper* glass (*see* Fig. 3); a property which I have not observed to be possessed by any other crystals.

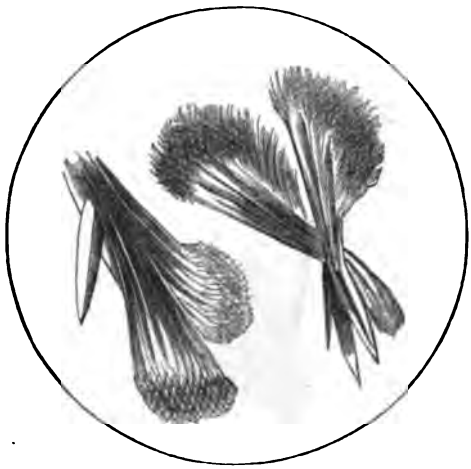

Fig. 2.

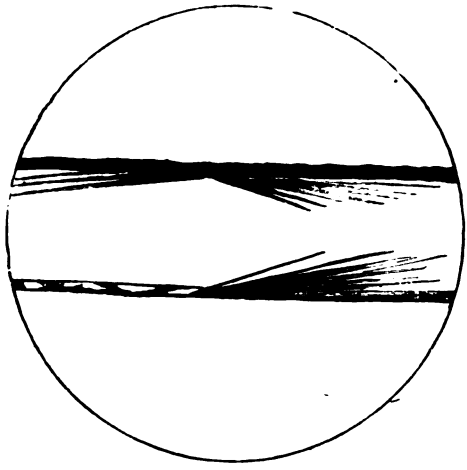

Fig. 3, shewing the peculiar tendency which the crystals of Picrotoxia exhibit of placing themselves nearly parallel to edge.

When genuine malt and hop beer is subjected to this process, only very minute crystals are obtained (*see* Fig. 4.); and the same observation may be made with regard to beer that has been adulterated with Guinea pepper, wormwood, quassia, gentian, and an infusion of camomile flowers. Picrotoxia, it should be further remarked, may be also distinguished from every other substance with which it is likely to be confounded, by the singular form which it assumes when slowly crystallised from its solution in acetic acid. The

acetate (?) of picrotoxia separates under such circumstances in crystals more or less resembling those shewn in Fig. 5., which will serve better to explain their peculiarities, than a mere verbal description. In order to determine the value of the tests I have just described, as a means of enabling the chemist to detect the adulteration of beer with *cocculus indicus*, I tried a series of comparative experiments with pure malt liquor, beer, and porter, which had been purposely adulterated with *cocculus indicus*, Guinea pepper, &c., and contrasted the results with those obtained on examining different samples of malt liquor, which I caused to be purchased from various localities.

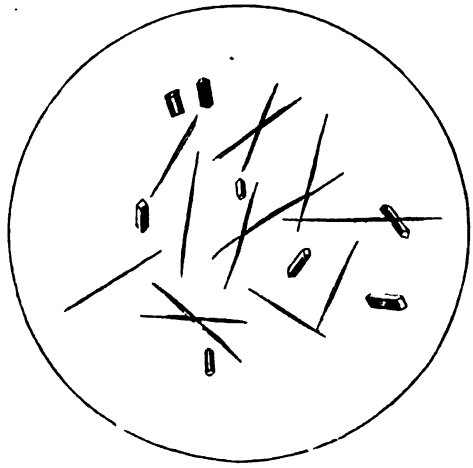

Fig. 4.

Some of my results I have already alluded to; the most important of the others may be thus summed up:—

1st. That by means of this test a careful operator can detect picrotoxia in beer even when the extract has been added at the rate of only a quarter of an ounce to the barrel, and the experimentalist has but a pint or two of beer to subject to analysis.

2nd. That in those cases where a larger quantity of the liquor can be operated upon, he can detect a much smaller amount of adulteration.

3rd. That out of nineteen different samples of beer and porter that were obtained from different tradesmen, and examined by this test, seven were proved to be adulterated with *cocculus indicus*, and in another the indications obtained were doubtful.

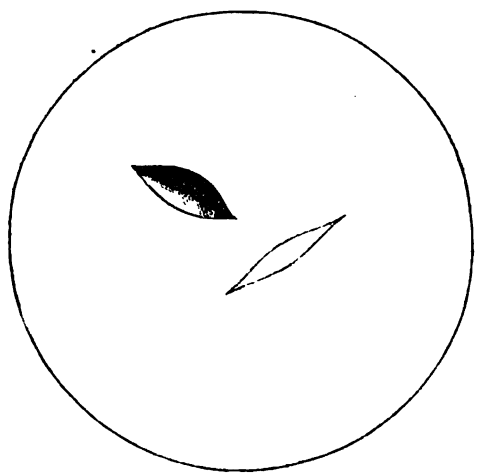

Fig. 5. Acetate of Picrotoxia (magnified) from stomachs of poisoned fish.

In conclusion, I may observe that a respectable chemist and druggist of this city has favored me with the formula that is employed in the manufacture of *one kind* of brewers' extract. It is as follows :—

℞.—Cocculus indicus berries	1lb.
Quassia chips	2lbs.
Camomiles	1lb.
Tobacco	1oz.
Water	1gal.

Boil together for several hours, press, and strain. Then boil gently down to one quart, and, when cold, add *burnt treacle* sufficient to make up two fluid quarts.

In the note in which he communicates this information, he says, "I believe a quarter of the above quantity is generally added to every 100 gallons of the beer or porter."

A specimen of brewers' extract, however, which I have myself examined, I found to contain no tobacco, and to consist apparently of burnt treacle, extract of quassia chips, and "black extract," in about the following proportions :—

	In 1000 parts.
Dry extract of *C. indicus* . . .	86.1
Ditto quassia	19·4
Burnt treacle	894·5

CHEMICAL TABLES.

Compiled by Thornton J. Herapath.

No. II.

Thermometrical Equivalents *and* Effects *of* Temperature.

Reaumer divides the interval between the temperature of boiling water and melting ice on his scale into 80; Celsius into 100; Delisle into 150; and Fahrenheit into 180 equal parts or degrees. The first two place the O or Zero at the melting point of ice, Fahrenheit 32° below that temperature, and Delisle at the boiling point of water:—

Hence 9° F. = 7·5 D. = 5° C. = 4° R.

One degree of Wedgewood's pyrometer = 161½° of Fahrenheit, according to the inventor, and the first degree, he says, corresponds to 1108°F.; Guyton Morveau, however, says, the first degree corresponds to 518°F., and each degree is equal only to about 93¼F.

—1000°F. Oxygen becomes liquif. (hypothet.)?

—240°F. A mixture of bisulphide of carbon and solid carbonic acid *(in vacuo)* produces a cold of—240°.

—150°F. Liquid nitrous oxide solidif.

—139°F.;—95°C. Anhydrous alcohol flows like melted wax.

—220°F. Liquid chlorine does not freeze.

—135°F. The mixture of bisulphide of carbon and solid carbonic acid (in the open air) produces a cold of—135°.—*Thilorier.*

—130°F.;—90°C. A jet of liquid carbonic acid emitted upon the bulb of a spirit thermometer sinks it to—130°F.
Anhydrous alcohol becomes thick and oily.
—122°F. Liquid hydrosulphuric acid solidif.
—121°F.;—85°C. Solid compound of alcohol and carbonic acid becomes fluid.
—103°F.;—75°C. Frozen ammonia (anhyd.) liquif.
—100°F. Liquid hydrobromic acid freezes.
— 92°F. Solid hydrobromic acid liquif.
— 91°F. Greatest artificial cold measured by Walker.
— 85°F.;—65°C. Liquid carbonic acid freezes.
— 65°F. A solution of 1 part of *potassa-fusa* in 1 part of water does not freeze.
Greatest natural cold, observed by M'Clure in the Arctic regions (1853).
— 62°F. Bisulp. of carbon does not solidify.
— 61°F. Ammoniacal gas is liquif.
— 60°F. Greatest natural cold observed by Ross in the Arctic Regions.
Strongest *liquor ammoniæ* freezes into a grey amorph. mass.
—58·6°F. Temperature of planetary space (hypothetical).
—58°F.;—50°C. Fluid hydriodic acid freezes.
—55°F. Lowest temperature observed by Parry at Melville Island.
Strongest nitric acid freezes.
—49°F.;—45°C. Chlorarsin remains fluid.
—46°F. Æther congeals.
—40°F;—40°C. Æther boils *in vacuo?*
[Liquids in general boil at about 145° below their normal B.P., when the pressure of the air is removed].
Terchloride of nitrogen and hydrocyanic acid (anhydrous) remain fluid.
Arseniuretted hydrogen and ammoniacal gas are liquif.
—39°F. Solid mercury fuses.
—38°·7′F. Mercury freezes.—*Hutchinson.*
—37°F. Lowest temperature observed at Hudson's Bay (1775).
—36°·25′F. Oil of vitriol freezes.—*Thomson.*
—36°F. *Liquor ammoniæ* freezes at from—36° to—42°.—*Many authorities.*
—31°F.;—35°C. Liquid cyanogen freezes.
—28°F. Lowest temperature observed at St. Petersburgh.
—22°F.;—30°C. Oxygenated water does not freeze. Chloride of sulphur remains fluid.
—21°F. Stibæthyle remains fluid.
—20°·5′F. Amyl becomes thick and oily.
—20°F. Bichloride of tin does not congeal.
—15°F. A mixture of equal parts of snow or ice and the crystal. hyd. of potassa lowers the temperature from 30° to—15°.
—13°F.;—25°C. Bromine freezes.—*Liebig.*

—13°F.;—25° C. Mean temperature at the North Pole.—*Parry.*
—10° F. Chloride of æthyle solidif.
— 9°F. Alkarsin freezes.
— 8°F. Phosphorus ceases to be luminous in the dark.
— 7°F. A mixture of equal parts of water and alcohol; also proof spirit and brandy freeze.
— 6°F. Terchloride of acetyle condenses.
— 5°F. Sulphurous gas may be liquif. at common pressures.
— 4°F.;—20°C. Oxide of chlorine and hyponitric acid condense.
Fusel oil *(many authors)* and salicylous acid freeze.
1—hyd. hydrofluoric acid and eupion do not freeze.
— 3°F. Nitric acid, sp. gr. 1·3, freezes.
— 1°F. Bromine solidif.—1° to—13°.
Methyl is incondensible.
Œnanthylic acid remains fluid.
O or Zero, F.;—17°77′ C.;—14°22′ R.
Ice gives off vapor in a current of air far below O.
Castor and poppy-oils freeze.
Greatest cold observed in the shade in England—(A.D. 1709 and 1710).
1°·33′F. Mean temperature at Melville Island.
3°·5′F. Bromine-water does not freeze.
4°F. Fousel-oil freezes?
A solution of 1 part of salt in 3 pts. of water freezes.
5°F;—15°C. Bihyd. of formic acid freezes.
5°·5′F. Chlorocyanic acid solidif.
7°F. Solution of 1 salt in 4 water freezes: also mixture of 1 alcohol + 3 water.
8°F. Solution of 1 sal-ammoniac + 4 water freezes.
9°F. Mercaptan remains fluid.
10°F. Hydrofluoric acid gas is incondensible.
14°F.;—10°C. Liq. hyponitrous acid and sulphurous acid boil.
Liquid modification of styrone remains fluid.
15°F. Walnut oil freezes.
16°F. The thermometer, in this climate, seldom falls below 16°.
Oil of turpentine freezes.
18°F. Caoutchouchine, solid, fuses.
20°F. Strong wines freeze. Temperature in winter in the higher Apennines.
21°F. Kakodyl freezes.
23°F.;—5°C. Oils of bergamot and cinnamon freeze. Frozen wine smelt.
24°F. Very cold—in this climate.
28° F. Vinegar and sea water freeze 27° or 28°: also rape oil. Liq. ammonia, anhyd. boils.
29° F. Oil of beech-nuts freezes.
30° F. Whale oil deposits stearine. Almond oil congeals. Milk freezes at about 30°.
31° F. 1-hyd. formic acid freezes: 31° to 30°.
32° F.; O or Zero, C. and R.

32° F.; Ice melts [Latent heat of Water, 140° F.—*Black.* 142° F. *Dumas.*]

Dry air and other gases expand at the rate of 1–480 (*Mariote*) or 1–494 (*Rudberg*) for every degree above 32°.

Potassium becomes hard and brittle; but sodium remains soft, and an alloy of 1 potassium + 3 sodium remains fluid.

Hydrate of chlorine freezes.

33° F. Lime, barytes, and strontia (anhyd.) combine with water, evolve great heat, and fall to powder, at all temps. between 32° and 212°.

34°·5′ F. Nördhausen sulphuric acid freezes.

36° F. Olive-oil freezes. Mean temp. in London (*January*). Glacial acetic acid solidifies?

38° F. Lowest temp. at which fermentation takes place. Nitro-benzide freezes.

Mean temp. in London (*February*).

39° F. Mean temp. in St. Petersburgh.

39°·1′ F. Water attains its greatest density (*Despretz*).

39°·3′ F. Mean temp. in London (*December*).

40° F. Mercury does not give off vapor spontaneously below 40°. (*Graham*).

Water attains its greatest density. (*Most Authors*).

Caoutchouc becomes rigid.

Sulphurous acid (9 eq. Water) freezes.

Cold in this climate; but 40° to 50° is considered agreeable by inhabitants of cold countries.

41° F.; 5° C. The isothermal line of 41° is nearly coincident with the northern boundary of oak and wheat.

42°·3′ F. Mean temp. at Stockholm.

42°·9′ F. Mean temp. at London (*November*).

43°·9′ F. Mean temp. at London (*March*); also, in winter, in Devon.

44°·3′ F. Benzole, crys., liquif.

45° F. Tempering steel dies: a red-heat and water at this temperature are the most desirable.

46°·5′ F. Mean temp. at Berlin.

46°·9′ F. Ditto, at Gottingen.

47°·8′F Ditto, at Edinburgh.

48°·6′ F. Ditto at Dublin.

48°·9′ F. Mean temp. at London (*October*).

49° F. Caprylic acid solidif.

49°·9′ F. Mean temp. London (*April*).

50° F.; 10° C; 8° R. Medium temp. of the surface of the earth. Usual temp. of spring water. The isothermal line of 50° nearly corresponds in Midland Europe with the limit of the advantageous cultivation of the wine grape.

Sulph. hydrogen (17° Atmos.) becomes liquif.

50°·5′ F. Mean temp. London.

50°·9′ F. Average heat in Lond. for the first 16 years of this century.
51° F. Mean temp. at Paris.
51°·6′ F. Temp. in water in mines (granite) 60 Fathoms.
52° F. Average temp. in England.
53° F. Mean temp., Ireland? Ditto, France.
54° F. Mean temp., London *(May)*.
55° F. Horses' fat fuses.
56° F. Temp. of water in mines (in granite) 50 to 100 Fathoms.
57° F. Temp. in mines (in slate) 50 to 100 Fathoms.
[The temp. increases at the rate of about 1° F. in 60 feet of depth.]
57°·8′ F. Mean temp, London (*September*).
58° F. Mean temp., Marseilles. Caoutchouchine boils.
58°·7′ F. Mean temp., London (*June*).
59° F; 15° C. Mean temp., Madrid.
1-hyd. Hydrofluoric acid boils.
Isothermal line corresponding to the northern boundary of the olive and fig.
60° F. Proper temp. for taking sp. grs.—*Dublin and Edin. Phar.*
Mean temp., Rome.
Perfectly dry phosphorus inflames.—*Higgins.*
In our climate we are best pleased with the heat of the air from 50° to 60°.
Iodine, when moist, volatilises even below 60°.
Bromide of acetyle, chloride of cyanogen, and chloride of æthyle (*Brande**) boil.
61° F. Mean temp. London (*July*).
Nitrous æther boils. Palmic æther fuses.
61°·3′ F. Temp. in mines. 50 to 100 Fathoms (in slate).
61°·6′ F. Mean temp., London (*August*).
62° F. Temp. for taking sp. grs.—*London Pharm.*
62°·5′ F. Mean temp., Mexico.
64° F. Human fat is solid. 1-hyd. acetic acid fuses. Air of this temp. is said to be warm.
64°·2′ F. Mean temp. of the hottest month (*July*) in London.
64°·5′ F. Mean temp., Naples.
65° F. Mean temp. in summer, England.
Temp. in mines, 100° to 150° F. (in granite).
Best temp. for napthalising gas.
66° F. Potassium becomes soft as wax.
Anhyd. sulphuric acid freezes.—*Bussy.*
67° F. Water will boil in a good vacuum.—*Graham.*
68° F.; 20° C. Liq. hypochloric acid boils.—*Millon.*
Best temp. for lactic fermentation; 68° to 77°. Heat of Matlock waters, 66° to 68°. Temp. in mines, 100° to 150° F. (in slate). Proto-chl. of arsenic fuses; 68° to 73°. The isothermal line expressing 68° of temp. corresponds nearly to the northern limit of the sugar-cane and coffee, and the southern limit of snow.

69°·25′ F. Mean temp. in summer, in Cornwall and Devonshire.
Liq. hypochlorous acid boils.
69°·8′ F. Mean temp., Algiers.
70° F. Ricinic and cevadic acid fuse. Fused saligenine, palmitic æther, and cocoa-nut oil solidif.
Temperate in tropical countries.
70°·25′ F. Palmitic æther fuses.
72° F. Anhyd. sulphuric acid fuses, 72° to 75°.
72°·3′ F. Mean temp. at Cairo.
74° F. *Liq. ammoniæ* of 0·89 or 24·7⅞ boils.
74°·5′ F. Mean temp. Rio Janeiro.
Lard fuses.—*Some authors.*
75° F. Beet juice readily ferments: 75° to 84°.
76° F. Duck's fat fuses.
76°·4′ F. Mean temp. Havannah.
77° F.; 25° C. Ænanthic acid fuses about 77°.
78° F. Mean temp. at Vera Cruz.
79° F. Mean temp. at Madras.
Oil of roses and oil of cedar-wood solidify.
80°. F. Anhyd. sulphuric acid is vol. somewhat above 80°.
Liquid hydrocyanic acid boils.
Heat of atmosphere when very warm and sultry (in this climate).
The *vitex agnus castus*, or Chaste Tree of the ancients, a species of osier, several cryptogamous plants, and *ulva thermalis*, the hot spring laver, vegetate in hot springs of a temp. from 80° to 150°.
81°·3′ F. Temp. in mines, 200° F., in granite.
81°·5′ F. Mean temp. of the air at the equator on a level with the sea.—*Humboldt.* Also of the region of the finest spices, palm and banana tribes.
82°·5′ F. Mean temp. at La Guayra.
Elephant's fat fuses.
84° F. Temp. of the Buxton waters; 82° to 84°.
84°·74′ F. Mean temp. of the surface of the sea at the equator.—*Humboldt.*
85° F. Mean temp. at Pondichery.
Anhyd. nitric acid fuses; also bassia and palm-oil.
85°·6′ F. Temp. of mines of 900° F. and upwards (in slate).
86° F.; 30° C. *Liq. ammoniæ* of 0·9 or 22·2⅛ boils.
Gutta percha is supple, tough, and extensible at all temps. between 40° and 86°.
Acetous fermentation is best conducted at this temp.
Capric acid fuses.—*Gorgey.*
Temp. of the Naters'-waters.
87° F. Aldehyde boils; 86° to 87°.
Summer Heat.
88° F. Mean temp. at noon at La Guayra, said by Humboldt to be one of the hottest places on the earth.

88°·5′ F. Mean temp. same place, from June to November.
89° F. Temp. in Monkwearsmouth coal-mine, Durham (depth, 1,590 feet) 78° to 89°.
90° F. Highest temp. of the thermometer in the shade at London.
Best temp. for drying medicinal plants.
Rose's fusible alloy (fused) solidif.
91°·4′ F. Anhyd. benzoïc acid *fuses.—Gerhardt.*
94°·7′ F. Æther boils.—*Andrews.*
[Latent heat of vapor = 206·6 *And.*]
95° F.; 35° C. Valerone boils.
96° F. *Balneum tepidum*, tepid bath, 62° to 96°.
97° F. Lard melts.—*Many authors.*
98° F. Blood heat (in Europeans).
Liq. ammoniæ of 0·91° or 19·8⅛° boils. Temp. of St. Gervaise water.
98°·5′ F. Mean temp. of the body in infants; 98·5° to 100°.
99° F. Blood heat (in Negroes).
100° F. *Liq. ammoniæ* of 0·94 or 17·4⅛ boils.
A solution of bicarbonate of ammonia gives off carbonic acid rapidly and becomes alkaline.
Water boils in an (imperfectly ?) exhausted receiver; 98° to 100°.
An inferior heat (*Dub. Phar.*); a gentle heat (*London Phar*).—90° to 100°.
Balneum calidum, hot-bath—97° to 100°.
101° F. Temp. of the body in the porpoise—100° to 101°.
Temp. of the Moutier waters.
102° F. Mean temp. of the body in *amphibia* and *cetacea*.
Amylene boils.
103° F. Mean temp. of the body in *carnivora*.
104° F.; 40° C. Selenium dissolves in vitriol.
Fused phosphorus solidif.; 104° to 109°.
105° F. Anhydrous sulphuric acid boils; 105° to 122°.
105°·5′ F. Bromide and sulphide of æthyle boil.
107° F. Spermaceti fuses; 107° to 109°. Fever heat, in general.
108° F. Iodide of methyle boils; 108° to 110°.
109° F. Palmine, thialdine, and nitronaphthaline fuse.
Temp. of waters at Cross Bath (Bath).
110° F. Paraffine fuses; 110° to 115°.
Temp. of body in birds; 104°·5′ to 109°·75′.
Temp. has been observed in Pekin: also at Bagdad, at midnight, during the Simoom (1819).
111° F. Scheererite fuses.
112° F. Gutta percha softens.
Ether, commercial, boils. [Latent heat of V. = 313°.—*Ure.*]
113° F.; 45° C. Bromine boils.—*Löwig.*
Anhydrous nitric acid boils.
Acetylic mercaptan sublimes.

113°F.; 45 C. Crystal, perchloric acid fuses.
114°F. Chlor-acetic acid fuses.
Temp. of the waters of the King's Bath (at Bath); of waters at Aix les Bains, 114° to 117°.
116°F. Bisulphide of carbon boils; 113° to 116°.
Fichtelite *fuses.*
116°·5′F. Bromine boils.—*Balard.*
Fever Heat, as usually marked.
117°F. Bromoform fuses.
Temp. of waters at Leuk, 117° to 126°: also of the hottest of the Bath springs.
118°F. Æthal fuses; 118° to 119°.
120°F. Cetine, fused, solidif.; 120° to 121°.
Nitrated oil of bitter almonds, solidif., 115° to 120°.—*Bertagnini.*
Amylene boils.—*Some authorities.*
Mineral tallow fuses; 118° to 120°.
Margarine, chocolate nut oil, emetine, and myristicic acid fuse.
It is painful to touch metal heated to 120°.
121°F. Sodium softens.
122°F.; 50°C. *Liq. ammoniæ* of 0·93 or 15·1⅛ boils.
Coumarine from Tonka-beans fuses.—*Delalande.*
124°F. Bisulphide of æthyl *boils.*
125°F. Palmitonic acid and oxalate of methyle-oxide *fuse.*
At Bagdad, during the Simoom, the temp. (in the shade) is said to amount to from 120° to 125°.
126°F. Urethylane fuses; 126° to 131°. Acroleïne boils.
127°F. Tallow fuses; 92° to 127°.—*Graham.**
129°F. Sugar of lead fuses.
130°F. Condensing point of low pressure steam; 120° to 130°.—*Graham.**
Iodine rises in vapor rapidly; 120° to 130°.
Formic æther boils.
Temp. of *Balneum vaporis*; 100° to 130°.
Temp. on the west coast of Africa sometimes exceeds 130°.
131°F.; 55°C. Acetate of methyle-oxide boils.
Oxide of cetyle fuses.—*Fridau.*
132·5′F. Acetone boils; 132° to 133.
133°F. Iodide of acetyle boils.
134°F. *Liq. ammoniæ* of 0·94 or 12·8⅛ boils.
135°F. Peroxide of chlorine explodes.
Cetic acid fuses.—*Heintz.*
Cerotene fuses; 135° to 136°.
Diæthylamine boils.
136°F. Potassium fuses.—*Daniell.*
Bromine boils.—*Andrews.*—[Latent heat of V. = 126·8°.]
138°F. Cholestearic acid fuses.
140°F.; 60°C. Stale bread may be made fresh by heating it by a

water-bath, in a tin-plate cylinder, closed with a stopper, to 122° to 140°.—*Boussingault.*

Alum fuses between 140° and 212°, and becomes vitreous.

Chloroform boils; 140° to 141° 4′.

Carbamide volatilises.

Margaramide, palmitic acid, and stillistearic acid fuse.

Margaritic acid fuses; 140° to 144°.

Protochloride of sulphur distils; 140° to 160°.

Very hot sunshine-heat in 1727?—*Dr. Hales.*

145°F. Stearine fuses; 142° to 145°.

Sulphovinate of ammonia, cerine, melene, and hyd. camphoric acid fuse.

Acetate of methyle, bromide of allyle, and xylite boil.

Temp. of a mass of fermenting tan often exceeds 145° to 150°.

A temp. of 145° to 167° destroys the contagious matter of plague and cholera, and may be used to disinfect clothing.

Gutta percha becomes soft and pliant.

146°.F. *Liq. ammoniæ* of 0·95 or 10·5§ boils.

147°F. Chloraniline fuses, 147° to 149°; myricine, 147° to 148°; and sulphobenzoyle at 147°.

149°F.; 65°C. Syrup boils *in vacuo.*

150°F. Bees' wax fuses [latent heat = 175°.—*Irving*]; 142° to 150°.

Albuminous solutions, when concen., coagulate; 145° to 150°.

Potassium fuses.—*Fownes.*

Water scalds.

151°F. Wood spirit boils.

Blue iodide of starch becomes colorless, but regains its color on cooling.

153° F. Temp. of Ursprung water (Baden).

154° F. Bromide of styrole fuses.

Butyral boils: 154° to 158°.

(To be continued).

TRANSLATIONS AND ABSTRACTS.

CHEMICAL PHILOSOPHY.

On Chemical Affinity among Substances in Solution.

By JOHN HALL GLADSTONE, Esq., Ph. D., F.R.S.

AN historical sketch of the development of the ideas of chemists concerning "affinity" was first given. The dogma of Hippocrates that "like combines only with like," was shewn to be superseded by the view of Glauber and others, that unlike substances combine most readily; and that where two bodies have an affinity *for* one another, it is a sign that they have *no* affinity *with* one another. The views of Newton and Boyle in reference to the different degrees of strength of affinity were then considered, and particular attention was directed to the doctrine of Bergmann, that when a decomposition takes place by means of the greater elective attraction of a third body, that decomposition is complete. In opposition to this, Berthollet contended that in all such cases of composition or decomposition there takes place a partition of the base, or subject of the combination, between the two bodies whose actions are opposed; and that the proportions of this partition are determined, not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies—by their physical condition—and by that of the combinations capable of being generated. These views did not meet with a favorable reception at the time of their promulgation; and the attention of chemists had been drawn away from the subject until within these last few years, when Malaguti, Bunsen, Debus, and Williamson have published investigations bearing upon the point. The lecturer then stated, that before any of these papers had appeared, he had been thinking of and performing some experiments upon the subject in question, and that he was still continuing them.

After a few experiments illustrative of "chemical combination" and of "elective affinity," others were introduced to shew how easily this latter phenomenon was affected by circumstances. Thus ammonia will displace alumina from a solution of the sulphate, but on the other hand, alumina will displace ammonia when heated with the solid sulphate of that volatile base; whilst if solutions of chloride of aluminium and sulphate of ammonia be mixed and evaporated, crystals of the double sulphate, ammonia-alum will appear. There were on the table two white salts; the one had been carbonate of baryta, but by boiling with excess of sulphate of potash, it had been converted into the sulphate; the other had been sulphate of baryta, but by long-continued boiling with much carbonate of potash, it had suffered the opposite change into the carbonate. The lecturer then stated that so great is the influence exerted by these various circumstances, that some have doubted whether there be a true "elective affinity;" he however believed that after making every allowance for known causes, there is still a residuary phenomenon to which that name is the most appro-

priate. Allowing then, with Bergmann, that relative degrees of affinity exist, the question arises :—Is Berthollet's law also correct? It is very difficult to arrive at a satisfactory answer, since it is almost impossible to eliminate other influences. Several reactions, however, were mentioned as tending to shew that there is some truth in the law :—for instance, the solution of gold in hydrochloric acid upon the addition of nitrate of potash. The experiments of Bunsen on mixtures of carbonic oxide and hydrogen, exploded with a quantity of oxygen insufficient for complete combustion ; and those of Debus on the precipitation of mixed hydrates of lime and baryta by carbonic acid were explained ; as also the remarkable fact noticed by both, that the resulting products were always in certain atomic proportions to one another. But in both these cases the first products of the chemical action are removed at once from the field : it is quite another case when they remain free to act and react on one another. Supposing they all remain in solution, the requisite is fulfilled; but how are we to know what has then taken place? Malaguti thought to obtain an indication of this by mixing the aqueous solutions of two salts, one of which is soluble in alcohol, and the other is insoluble, and then pouring them into very strong alcohol, and analysing the salts immediately thrown down. His results are tabulated ; they are valuable, but to some extent open to objection, on account of the disturbing influence of the alcohol. Some observations of Professor Graham, and others of Professor Williamson, as yet unpublished, were then spoken of, and the lecturer proceeded to describe his own endeavors to arrive at a knowledge of the intimate constitution of a mixture of salts in solution by observing their physical properties, especially color.

If solutions of one equivalent of nitrate of iron, and a triple equivalent of sulphocyanide of potassium be mixed, a blood-red color results, owing to the formation of sulphocyanide of the sesquioxide of iron; the question arises—Has all the iron left the nitric acid to unite itself with the sulphocyanogen? It has not ; for on the addition of equivalent after equivalent of sulphocyanide of potassium, a deeper red is constantly obtained. The arrangement by which this deepening of color was quantitatively determined was explained, and imitated on the lecture-table. The result was, that even up to 375 equivalents a regular increase was observed to take place, more rapidly at first than afterwards, which was exhibited to the eye by the results being projected as a curve. Again, as in the mixture of equal equivalents of the two salts, some iron still remains in combination with the nitric acid, a portion of the potassium must still remain united to the sulphocyanogen. Accordingly, the addition of more iron-salt also gives a deeper color. The curve expressing the results of this experiment was a regular continuation of the curve formerly mentioned ; and neither of them exhibited any of those sudden transitions which the experiments of Bunsen and Debus present. Diagrams exhibiting curves of the gallate and meconate of iron were also exhibited. Various experiments were then performed, shewing the alteration in the resulting color upon any change of any of the elements in the pri-

mary experiment; for instance, the substitution of other acids for the nitric acid, or of other bases for the potash. On the addition of a colorless salt to a colored one, there results a diminution of the color greater than the mere dilution would have produced, as was exemplified in the cases of the red sulphocyanide of iron mixed with sulphate of potash, and of the scarlet bromide of gold mixed with chloride of potassium. The lecturer accordingly drew the conclusion that when two salts mix without precipitation or volatilization, the acids and bases frequently, if not universally, arrange themselves according to some definite proportion; and that this depends on the relative quantity of the two salts, as well as upon the proper affinities of the substances composing them. He was unable then to enter upon the influence of heat, or of dilution in certain cases, or to add any remarks connected with double salts, or with other metals, or upon certain practical applications of these views in chemical and physiological science.

The fact that we very frequently find the double decomposition of a salt to be *complete*, the *whole* of one of its constituents being precipitated, was shewn to be easily explained on the principles of Berthollet. Thus, for instance, when chromate of potash and nitrate of silver are mixed, at the first moment a division will take place producing four salts, but one of these,—the chromate of silver—is thrown down at once as a precipitate, and thus put out of the field of action. Another division of the acids with the bases must take place, producing of course more of the insoluble chromate, and so on, till at length the whole of the silver is removed. And that this is really what does take place is rendered almost certain by the fact that wherever by an interchange of acids and bases, a precipitate can be produced, that precipitate does form; and, if the substance be perfectly insoluble the whole is thrown down; this occurring in opposition to all rules of "affinity," and to all tables that Bergmann, or any other chemist, ever did or could construct. The volatility of one of the products acts in the same manner as insolubility, as is exemplified in the decomposition of carbonates by any other acid. Crystallisation also is but another phase of the same phenomenon. An experiment was exhibited in illustration of this. Dilute solutions of nitrate of lime and sulphate of soda, were mixed at the ordinary temperature without producing any separation of solid matter; but they were so proportioned, that upon heating the mixture, the crystallization of some sulphate of lime was determined, and when once this had commenced, it progressed rapidly; resembling in that respect the ordinary phenomena of precipitation. If in a double decomposition a far larger quantity of a sparingly soluble salt be produced at the first moment than the water can dissolve, the crystals wil be formed rapidly, and will accordingly be very small in size; but should there be formed at once only just sufficient to determine a separation in the solid form, the crystals will grow gradually, and will often attain a large size. This was exemplified on the mixture of nitrate of silver with the sulphates of copper and of potash respectively.

It is possible that the law of Berthollet may not be universally

applicable; yet the present advanced state of science shews that not only is there, as Bergmann insisted, a true chemical affinity, that is, a preference of one substance to combine with a certain other substance instead of a third, but, in a great number of instances at least, this substance will combine with both, according to certain proportions, whenever the whole of the affinities can be brought into play at the same time.—*Royal Institution, in Phil. Mag.*

ORGANIC CHEMISTRY.

Researches *on* some Products *of the* Transformation *of* Creatine. By M. Dessaignes.

Oxide of mercury, heated with an aqueous solution of creatine or creatinine, attacks these two bodies. The liquor exhales the odor of the products of the dry distillation of creatine, which M. Chevreul has compared to that of phosphorus. It disengages carbonic acid without any trace of ammonia; the oxide of mercury is partially reduced. The principal product of the reaction consists of a crystalline body, which I announced some time ago as a new alkaloïd, on account of its alkaline reaction. I have resumed the examination of this matter. The crystals obtained with creatinine are not homogeneous. By employing alcohol and repeated crystallisations, I ultimately obtained:—1. Crystals sparingly soluble in water, still less so in alcohol, neutral to test paper, efflorescent at 212° F., and possessing all the characters which I have recognised in creatine. 2. A greater quantity of flattened prisms, joined laterally and imbricated. Creatine, with a sufficient excess of oxide of mercury, produced only the second crystals.

This matter is very soluble in water. It has a disagreeable taste; it becomes opaque at 212° F., losing water. Heated on a platinum plate, it exhales the same odor as creatine; it slightly restores the blue color of reddened litmus paper. With potassa, and without heat, it disengages no ammoniacal odor; it precipitates the chlorides of barium and calcium, nitrate of silver, and acetate of lead. This last precipitate, washed and decomposed with sulphuretted hydrogen, gave me oxalic acid. That which I had taken for an alkaloïd, is, therefore, the oxalate of a very powerful base. I isolated this base by heating its oxalate with milk of pure lime, in very slight excess, and filtering. By evaporation *in vacuo*, I obtained a colorless matter, with a crystalline surface, which was due, perhaps, to this alkali having absorbed carbonic acid, which it attracts powerfully. It is also deliquescent; it has a very caustic, and at the same time ammoniacal taste. Heated on a sheet of platinum it volatilises almost entirely, with a strong odor of burning creatine; it expels ammonia from ammoniacal salts, without the aid of heat; it abundantly precipitates the chlorides of barium and calcium. The precipitate is soluble in a large quantity of water, and also in weak acetic acid without effervescence. It precipitates sulphate of alumina and chloride of iron, and the precipitates are redissolved in

an excess of the precipitant. It precipitates nitrate of silver of a yellowish white; it dissolves oxide and chloride of silver. It precipitates also the salts of mercury, lead, and copper.

With the oxalate of this alkali, and the chloride, nitrate and sulphate of lime, it is easy to prepare crystallised salts, which have a slight alkaline reaction. With the hydrochlorate and chloride of platinum, each in concentrated solution, we obtain, after some time, superb orange octohedra of a double salt, which, redissolved and crystallised by cooling, frequently occur in flat prisms, join side by side; the colored mother-liquors arising from the preparation of the oxalate, are very alkaline to test paper. Deprived of oxalic acid by chloride of calcium, they give with chloride of platinum a considerable quantity of the double salt of which I have just spoken.

The oxalate prepared with creatine lost at 212° F. 13·25 per cent. of water; the same salt gave oxalate of lime dried at 212° F., representing 38·41 per cent. of $C^2H^2O^4$. By combustion with oxide of copper and oxygen on one hand, and with soda-lime on the other, I found that the same salt dried at 212° F. contains,—

C	30·90
H	6·93
N	35·05

The oxalate resulting from creatinine lost at 212° F. 13·34 per cent. of water. Dried at 212° F., it contains, in 100 parts,—

C	30·98
H	7·27
N	35·43

The hydrated oxalate, according to the formula,—

$$C^8H^{14}N^6, C^2H^2O^4, H^2O^2,$$

should lose at 212° F. 13·23 of water, and contains 38·14 of monohydrated oxalic acid. The salt dried at 212° F., for the formula

$$C^8H^{14}N^6, C^2H^2O^4$$

should contain,—

C	30·50
H	6·77
N	35·59

I have also made the complete analysis of the chloroplatinate dried *in vacuo*.

	Experiment.		Calculation.	
C	. .	8·77	C^4 . .	8·60
H	. .	3·03	H^{16} . .	2·87
N	. .	14·85	N^6 . .	15·05
Cl	. .	38·71	Cl^6 . .	38·18
Pt	. .	35·19	Pt . .	35·30
				100·00

The composition of this remarkable base is then represented by the formula $C^8H^{14}N^6$. What is its constitution? As creatine may be represented by urea and sarcosine conjugated with elimination of water,

so does the new base seem to be urea and methylamine conjugated with elimination of water. Indeed,

$$C^4H^{14}N^4 + H^4O^2 = C^2H^4N^4O^2 + C^2H^{10}N^2.$$

This base, which may be called methyluramine, heated with a solution of baryta, is decomposed, disengaging ammonia accompanied by an odor of sea-water. The chloroplatinate exhales the odor of trimethylamine. Sarcosine may be represented by glycollate of methylamine minus water, and it is doubtless to glycollic methylamide what sugar of gelatine is to glycollamide. It is the elements of glycollic acid which, in creatine, are oxidised with oxide of mercury, so as to produce oxalic acid, carbonic acid and water, as expressed by the equation

$$C^8H^{18}N^6O^4 + O^5 = C^4H^{14}N^4, C^2H^2O^4 + C^2O^4 + H^2O.$$

I have tried, also, to throw some light on the constitution of creatine, by submitting it to the action of nitrous acid, and this investigation has enabled me to recognise in this body a character which escaped Liebig in his beautiful work. Creatine differs, it is true, from sugar of gelatine and other analogous bodies, in its inaptitude for combining with the metallic oxides; but it resembles them in uniting with the acids to form beautiful crystalline combinations with a powerfully acid reaction. When we pass a rapid current of nitrous gas into water containing an excess of undissolved creatine, the latter dissolves very quickly, and then a large quantity of small brilliant crystals appear. These crystals, which may easily be obtained in large, short prisms, by solution in tepid water and recooling, consist of nitrate of creatine. Their solution, which is very acid to the taste, is abundantly precipitated by ammonia; the precipitate, dissolved in warm water, gives, by cooling, small prisms, which effloresce at 212° F., and whose solution, which is neutral to test-paper, does not precipitate either chloride of mercury, chloride of zinc, or nitrate of silver. These prisms, dried at 212° F. and analysed, gave in 100 parts:—

C . . .	36·77
H . . .	7·13
N . . .	32·18

Now anhydrous creatine contains

C . . .	36·64
H . . .	6·87
N . . .	32·06

I have also estimated the nitric acid of nitrate of creatine, and have obtained for 100 parts 32·36 of monohydrated nitric acid. The formula

$$C^8H^{18}N^6O^4,N^2H^2O^6$$

requires 32·47 from $N^2H^2O^6$. I have produced the same combination by dissolving 1·057 grs. of crystallised creatine in nitric acid of known strength, which contained 0.447 grs. of $N^2H^2O^6$, and evaporating to 30 degrees. The crystals were homogeneous, and weighed 1·373 grs. According to calculation, they should have weighed 1·376 grs. The sulphate and the hydrochlorate of creatine are beautiful prisms, more soluble than the nitrate, like it, not deliquescent, and may be obtained by direct combination with the acids of known strength, and by evapo-

rating to 30 degrees, or *in vacuo*. I have separated from them creatine free from all traces of creatinine; I have also estimated them by synthesis, and they have then the formulæ

$$C^4H^{10}N^4O^4, SH^2O^2,$$

and

$$C^4H^{10}N^4O^4, Cl^2H^2.$$

The solution of nitrate of creatine treated with a current of nitrous gas, disengages much gas; but the reaction, however long it may have lasted, has never given rise to an acid free from nitrogen. By neutralising the product with potassa, separating the greater part of the nitro by crystallisation, then adding nitrate of silver, we obtain crystals soluble in warm water, and which, after several crystallisations, occur under the form of long white needles, which turn rather yellow in the light. This salt is a combination of nitrate of silver and a new alkaloïd. When calcined, it disengages an odor of sea-water. It contains, in 100 parts,

C . . .	16·29
H . . .	2·37
Ag . . .	47·97

which may be represented by the formula

$$C^6H^{10}N^2, N^2AgO^6.$$

This salt, decomposed with hydrochloric acid, not in excess, gave the nitrate of the alkaloïd which it contained. This nitrate crystallises in a fibrous mass or in small prisms; it is very acid to the taste. With chloride of mercury, it produces long needles of a double salt, from which it will be possible to separate the organic base; but in order to pursue the study of this base, I must wait until I have prepared a fresh quantity of creatine.

Comptes Rendus, No. 19, May 8, 1854.

Memoir on Caprylic Alcohol *and its* Derivatives.

By M. Jules Bouis.

In a former communication, I made the Academy acquainted with the formation and properties of a new alcohol—caprylic alcohol—reserving the complete study of this body for a monograph on castor-oil. Peculiar circumstances having prevented me from completing this long work, I now extract from it all that relates to caprylic alcohol, in order to clearly establish its composition, which has been so ill-comprehended by some chemists, who have repeated my experiments without knowing all the details of preparation, and have obtained impure products which have led them into error.

Caprylic alcohol, $C^{16}H^{18}O^2$, is a transparent, colorless, oleaginous liquid, staining paper like the essential oils; insoluble in water, soluble in ordinary alcohol, pyroxylic spirit, ether, and acetic acid; readily dissolving the fatty bodies, the resins, sulphur, phosphorus and iodine. It burns with a very beautiful white flame; it exerts no action on

the plane of polarisation; its density is equal to 0·823 at 62° F.; it boils without decomposition at 354° F., under the pressure of 0·760m.

Sulphuric acid converts the alcohol into sulpho-caprylic acid, which is capable of combining with bases, or else into a liquid hydrocarbon isomeric with olefiant gas, amylene, &c. This hydrocarbon is also produced by the action of fused chloride of zinc.

Caprylic alcohol is attacked by potassium and sodium, and gives compounds in which a portion of the hydrogen is replaced by the metal. Chloride of calcium combines with it, and furnishes very well-defined crystals; the combination is more soluble without than with heat; it is destroyed by water.

In this Memoir, I describe in full detail the preparation and purification of caprylic alcohol, and I make known, at the same time, the various products which are formed during the operation.

Castor oil, suitably treated with potassa, always gives one-fourth of its weight of sebacic acid, one-fourth of its volume of perfectly colorless alcohol, and the remainder consists of a mixture of fatty acids, one of which is liquid, resembling oleïc acid, and the other solid, presenting the composition of ethalic acid.

The alcohol, purified by several distillations over pieces of potassa, distils completely, without becoming colored and without its boiling-point varying.

Numerous analyses of products obtained by means of castor-oil, from America, France, and Germany, agree exactly with the formula

$$C^{16}H^{18}O^{2} = 4 \text{ vol.},$$

confirmed also by several determinations of the vapor-density. I may add, identical results have been obtained with the alcohol resulting from the treatment of pure ricinolic acid with potassa.

In order to perfectly establish the composition of caprylic alcohol, I give the properties and preparation of some of its principle derivatives.

Caprylene, $C^{16}H^{16}$, is a colorless refracting liquid, of rather strong odor, insoluble in water, soluble in alcohol and ether, and burning with a very luminous flame; its density is 0·723 at 62° F.; it boils without decomposition at 257° F., under the pressure of 0·760m.; its calculated vapor-density is 3·86 = 4 vol. The mean of several experiments gives 3·86.

This hydro-carbon was obtained by distilling the alcohol either over sulphuric acid or over fused chloride of zinc. I have studied the action of ordinary sulphuric acid, and have proved that, according to the duration of contact, we obtain sulpho-caprylic acid, $2SO^{3}$, $C^{16}H^{17}O$, HO, or else a mixture of caprylene and sulphuric ether; or, finally, a hydro-carbon isomeric with caprylene, but possessing very different properties; its density is 0·814; it boils at 482° F., and its boiling-point rapidly rises; its odor then becomes insupportable; it resembles that of perspiration.

Sulpho-caprylic acid is liquid, colorless, syrupy, and very soluble in water and alcohol; when it is heated, it turns black and decomposes; its solution, submitted to distillation, regenerates caprylic alcohol. It is obtained by carefully decomposing the sulpho-caprylate

of baryta with dilute sulphuric acid, or the lead salt with hydrosulphuric acid, and evaporating the liquor in the dry vacuum.

Sulpho-caprylate of baryta is white, of a pearly aspect, greasy, very soluble in water and in alcohol, from which it is deposited sometimes under the form of needles; it is decomposed towards 212°F., or by remaining too long *in vacuo*. Pressed between folds of paper, it gave by analyses numbers which agree perfectly with the formula.

$$2SO^3, C^{16}H^{17}O, B^aO + 3HO.$$

This salt is excessively bitter; it leaves a very sweet after-taste. It serves for forming the other sulpho-caprylates, of which I shall at present only mention the sulpho-caprylate of potassa. This salt is bitter, pearly, unalterable in the air, and very soluble in water and alcohol; with heat, it undergoes a commencement of fusion, and burns, without carbonising, with a luminous flame. It is obtained by double decomposition by means of the baryta salt, or directly by saturating the acid with carbonate of potassa. It is decomposed at above 212°F.; its composition is $2SO^3, C^{16}H^{17}O, KO, HO$. Calculation requires:—

SO^3, KO . . .	33·9
C . . .	37·3
H . . .	6·9

Experiment gave, for two different products:—

SO^3, KO . .	34·1	. .	33·9
C . .	37·1	. .	37·17
H . .	6·92	. .	6·93

Of the various ethers which may be obtained by means of caprylic alcohol and acids, I will here mention only the acetic, hydrochloric, and hydriodic ethers.

The acetic ether, $C^{16}H^{17}O, C^4H^3O^3$, is a liquid of a very agreeable odor, insoluble in water, and boiling at 374°F.; it may easily be obtained by means of caprylic alcohol and acetic acid, with a current of hydrochloric acid, or, which is better, with acetate of soda and sulphuric acid. The numbers obtained assign to it $C^{16}H^{17}O, C^4H^3O^3$.

The hydrochloric ether, $C^{16}H^{17}Cl$, is liquid, insoluble in water, and very sparingly soluble in alcohol; the solution does not precipitate the salts of silver; it burns with a smoky flame with green edges. It possesses a very strong odor of orange. Its boiling point is about 347°F. It was prepared directly with hydrochloric acid and the alcohol, or with perchloride of phosphorus. The analyses agree very well with the formulæ $C^{16}H^{17}Cl$.

The hydriodic ether, $C^{16}H^{17}I$, is very analogous to the foregoing; during its preparation, I observed various facts which I describe in my Memoirs. I thus obtained a great quantity of red phosphorus, and I point out the means of procuring it easily.

When sodium is made to react on the hydrochloric ether, all the chlorine is removed, and the result of the reaction is capryle $\left.\begin{matrix}C^{16}H^{17}\\C^{16}H^{17}\end{matrix}\right\}$ or caprylene $C^{16}H^{16}$, according as we have operated, without or with heat. In the operation without heat, the sodium is covered

with a white pellicle of chloride of sodium, which is detached by agitation, and replaced by another, until the matter contains no more chlorine.

Analysis gives the following numbers:—

Calculation.

$$C = 85{\cdot}04;\ H = 14{\cdot}99;\ \begin{cases} C^{16} = 84{\cdot}95 \\ H^{17} = 15{\cdot}04 \end{cases}$$

By causing the sodium to react with heat, until action ceases, a liquid is obtained which has the odor and density of caprylene, and which boils, like it, at 255°F. Its composition is $C^{16}H^{16} = 4$ vol.

Analysis gave:—

$$C = 85{\cdot}59;\ C^{16} = 85{\cdot}71.$$
$$H = 14\ 40;\ H = 14{\cdot}29.$$

Its vapor-density is 3·80 = 4 vol. Calculation requiring 3·86.

The formation of these hydrocarbons is accompanied by very interesting phenomena, and is intimately connected with the production of other substances, to the study of which I shall shortly call the attention of the Academy.

Comptes Rendus, No. 21, May 22, 1854.

Observations *on* Milk.* By M. Morin, of Geneva.

Ten years ago, the existence of albumen in milk was scarcely suspected, although two operations were daily performed in dairies, which alone were sufficient for removing all doubt in this respect.

Indeed, cheese, properly so-called, and second cheese, are obtained at different temperatures; the first below 104°F., and the second, by carrying the remaining liquid to a temperature near boiling. Cheese is coagulated by rennet or vinegar, whereas second-cheese in great measure, escapes the action of these bodies.

It is difficult to find characters which more clearly indicate the coexistence of caseïne and albumen, for these are precisely the reactions which serve for distinguishing them.

In studying the passage of milk through membranes, I have arrived, by another way, at the knowledge that this liquid contains at the same time caseum and albumen. This fact was formally announced in a Memoir communicated to the Society in 1851.

The more recent experiments of M. Doyère, published in the "*Annales de l'Institut Agronomique*," have finally placed it beyond doubt that the second cheese, which separated by boiling, is formed in great part of albumen. M. Doyère has, at the same time, pointed out processes for effecting the analysis of milk, the only means which can enable us to decide to what extent the accusations of fraud, so commonly brought against the venders of this liquid, are well-founded.

Admitting that milk really contains albumen, it appears surprising that this substance does not separate entirely when it is boiled, since albumen in solution in water has the principal property of coagulating at

* Read before the "*Société de Physique et d'Histoire Naturelle de Genève.*"

the temperature of 145°F. This property of milk, of not undergoing any apparent change by the action of heat, is even turned to account for evaporating it in a water-bath, that is to say below 212°F., and preserving it under the solid form, which admits of a kind of fresh milk being instantaneously prepared in sea-voyages.

This apparent resistance to the action of heat goes still further; for by employing, for boiling milk, an oil-bath heated to 257°F., at this degree only are formed in abundance, on the sides of the vessel, small coagulated networks, which are successively detached, and go on increasing the thickness of the pellicle already formed on the surface of the liquid.*

Although the pellicle appears on the surface of the milk, at the temperature which produces the coagulation of the albumen, the greater part of this body remains in solution in suspension in the boiled milk. But if, by complete evaporation in a water-bath, the albuminous particles are caused to enter into cohesion, they lose the property of redissolving, or of remaining in suspension in the milky liquid, which is produced by making an emulsion with water and the residue of the evaporation, and they are soon deposited under the pulverulent form. This proves the superiority of the process pointed out by M. Gaultier de Claubry, for preparing *lactoline*, over the employment of the sand-bath, since by using only the air for concentrating the milk, this liquid undergoes no modification.

When rennęt has been used for coagulating milk, as is done in most dairies, it is not sufficient to boil the whey to separate the second-cheese, but a certain quantity of this sour liquid must be added to it. This addition completes the separation of the caseum, and allows the albumen to undergo the action of the heat. These two substances are precipitated together.

In using vinegar for the separation of cheese, it is not necessary to add whey in order to obtain the second-cheese. The caseum having been much better isolated by the first of these operations, boiling is sufficient for coagulating almost all the albumen. A small quantity escapes, which is rendered evident by lactic fermentation and a further boiling.

These observations prove that caseïn is combined in milk with substances which abandon it, in order to unite with the acids; but that this combination itself contributes, in the ordinary state, to disguise the properties of the albumen, since its presence is detected only after the previous separation of the caseum.

The union of the caseum with the albumen appeared to me so intimate, and the properties, as well as the elementary composition of coagulated albumen, so similar to those of curdled caseum, that I thought it necessary to examine whether milk did not contain albumen only, but in two different states—one portion in combination with bases exerting a sufficiently powerful action on the other to prevent it from coagulating at the temperature of ebullition.

In this hypothesis, the combination of albumen in milk, would have

* "*Nutrition of the Fœtus*," by Prevost and Morin; 1842.

played the part of a salt with two equivalents of acid to one of base, one immediately becoming free under the form of coagulated caseïn, as soon as the base was saturated with acetic or lactic acid, and the other resuming the faculty of coagulating by means of a sufficient elevation of temperature.

The following experiments were made with the view of elucidating this question:—

First experiment.— Milk coagulated with acetic acid in excess, and filtered, furnished a second coagulum towards 212° F. This is what occurs in the manufacture of cheese and second cheese.

Second experiment.—Caustic potassa was added to a portion of the same liquid, freed from caseum and filtered. On saturating the alkali with acetic acid, an abundant coagulum was instantly produced without heat. The liquor retained only a very small proportion of albumen coagulable by heat.

Third Experiment.—The same effect was obtained, and even more completely, by employing only the porportion of potassa necessary for saturating the albumen, and scarcely producing any action on turmeric paper, but abandoning the liquor to itself for several hours. Acetic acid then effected, without heat, the complete coagulation of the albumen.

These experiments prove that *the albumen of the serum is not combined with a base in milk*, otherwise it would be coagulated by acetic acid, at the same time as the caseïn, whilst the previous treatment by a base is necessary for communicating to it this property.

Analogous experiments on albumen of white of egg led to the same results.

Small proportions of potassa or of caustic soda in solution in water modified this body, especially when the action was continued for several hours, so as to render it instantaneously and completely coagulable, without heat, by acetic acid. In these experiments, the albumen behaved like the caseïn of milk.

These researches appearing to prove that casïne or albumen, coagulable by acetic acid, was combined with an alkaline base in the milk, I desired to ascertain to what extent this supposition was well-founded.

I coagulated the caseum of a kilogramme of milk with a slight excess of acetic acid, and after having eliminated the greater portion of the other organic substances, I obtained by calcination the saline matters resulting from these various treatments. They were formed:—

Of phosphate of lime:

Of chloride of sodium:

And of carbonate of soda.

This last salt was in considerable proportion. It resulted probably from the decomposition of the acetate of soda formed by the coagulation of the caseum, and corresponded very well with the proportion of acetic acid employed for this operation. It represented 0·477 gr., or half a gramme of oxide of sodium per 100 gr. of milk, and the quantity of dry and pure caseïn obtained from this same liquid scarcely exceeded 36 grms.

The sodaic combination of caseïn in milk was therefore approximatively:—

72 of dry caseïn,
and 1 of oxide of sodium.

The analogy between caseïn and albumen being thus far complete, it occurred to me that it might be possible to form milk artificially by subjecting albumen—that of the egg, for example—in the proportions above indicated, to the prolonged action of caustic soda, until it should become completely coagulable without heat, by means of acetic acid.

These experiments did not succeed, although I repeated them frequently, taking care to put in contact all the elements which I had discovered, by means of analysis, in milk, not only in the same proportions, but in very various quantities, and proceeding to this association either by binary combinations or by the simultaneous action of all these substances.

I must acknowledge that if caseum is a derivative of proteïn, as Mulder's researches show, it is certainly not a combination of albumen. I should have persevered less in these fruitless experiments if I had known then what I have since learned from the study of the passage of nutritive substances through porous vessels:—*that albumen and caseum possess different electricities; that caseum is positive, whilst albumen has a powerful electro-negative tendency.*

Analysis of the Milk of the Cow.

One kilogramme of milk from a dairy of high repute, was coagulated with an excess of acetic acid.

The *caseum*, washed and dried, was separated by means of ether, from the butter which it had carried along with it in coagulating.

The *albumen* extracted from the serum by boiling, washed and dried, retained no portion of fatty matter.

The evaporation to a syrupy consistence of the liquid freed from caseum, butter, and albumen, produced a deposit of *subphosphate of lime*.

A second evaporation determined the crystallisation of *sugar of milk* impregnated with a little acetic acid.

Washed with distilled water, this product was obtained white.

The complete desiccation of the mother liquors gave the total weight of the other substances purified from free acetic acid.

In redissolving this residue in water, a small quantity of sugar of milk and sub-phosphate of lime was again separated. This salt was separated from the sugar of milk by calcination.

The remainder was almost entirely precipitable by concentrated alcohol, and the deposit produced by the addition of this liquid consisted of a *gelatiniform matter*, or albuminose of M. Mialhe, containing a little gelatine.

The alcoholic solution separated from these two substances, furnished, by evaporation, a residue of complex nature, soluble in water and alcohol, insoluble in cold ether, and giving rise, by the successive action of nitric acid and carbonate of baryta, to the formation of a

crystalline body, soluble in alcohol, possessing the properties of urea, but in such small proportion that this part of the analysis requires confirmation.

The same alcoholic residue left by combustion 4 per cent. of ashes, formed approximatively of

1 part of phosphate of lime,
6 parts of chloride of sodium,
8 parts of sub-carbonate of soda.

I have already dwelt on the reasons which led me to regard this last salt as representing oxide of sodium combined with the caseum of the milk.

This analysis may be summed up in the following manner for 1000 grs. of milk :—

	grs.		
Caseum	36·138	= 36·615	Combination of caseum dissolved.
Oxide of sodium	0·477		
Butter		13·782	
Albumen		3·904	
Sugar of milk		36·000	
Gelatiniform matter (albuminose) and Gelatine		3·824	
Complex matter, soluble in alcohol		5·424	
Sub-phosphate of lime		2·562	
Chloride of sodium		0 564	
		102·675	
Water		897·325	
Total		1000·000	

To judge by analysis, the milk had been very carefully skimmed, since it did not contain 1½ per cent. of butter. This fact is by no means surprising, since it is the custom in dairies to sell separately cream and skimmed milk from one milking. By tripling the quantity of butter found, the total mass of the solid parts would agree very well with the normal composition of the milk of the cow; they exist in it in the proportion of 14 or 15 per cent.

(To be continued.)

ANALYTICAL CHEMISTRY.

Reduction *of* Bismuth, Lead, *and* Tin, *by means of* Cyanide *of* Potassium. By H. Rose.

By fusing oxide of bismuth with cyanide of potassium, the bismuth is completely reduced to the metallic state. Sulphuret of bismuth is reduced in the same manner, giving rise to sulpho-cyanide of potassium. In both cases the reduction is so perfect, that the author proposes to apply this process to the estimation of bismuth.

Such, also, is his opinion with regard to lead in the state of oxide or sulphate, for he has ascertained that the cyanide of potassium com-

pletely reduces these compounds. It is not the same with the sulphuret of lead, which is only imperfectly decomposed; do what we will, a portion of this compound always escapes the action of the cyanide.

In the same circumstances oxide of tin very readily gives up its tin; the metal separates under the form of small grains, which are reduced by treating the mass with aqueous alcohol; the solution produced is nearly free from tin when it has been carefully separated from the metal deposited; in the contrary case, the tin dissolves, and the liquid soon contains considerable proportions of it.

It is not easy to reduce anhydrous protoxide of tin; the fused cyanide does not moisten this oxide, and when the matter is stirred, the protoxide comes to the surface, where it passes to the state of binoxide.

Anhydrous protochloride of tin is reduced without difficulty; when it contains protoxide, the latter is abandoned in the state of a brown powder.

The black sulphuret of tin yields a portion of metal, and passes to the state of yellow sulphuret, which forms with the sulphuret of potassium resulting from the reaction, a sulphuret soluble in water, which resists the action of the cyanide. This yellow sulphuret is the compound which constitutes mosaic gold.

The sulphurets of arsenic and antimony are partially reduced by cyanide of potassium; a little sulphocyanide of potassium is formed at the same time.

Journal für Praktische Chemie.

QUANTITATIVE SEPARATION *of* NICKEL *and* ZINC. By M. WÖHLER.

THE solution containing the nickel and zinc is reduced to a suitable state of concentration, an excess of caustic potassa is added, and a sufficient quantity of hydrocyanic acid to redissolve the precipitate formed is poured in; there are thus formed double cyanides which behave differently with regard to mono-sulphuret of potassium: the double cyanide of zinc and potassium is alone decomposed by this sulphuret; the zinc is precipitated, whilst the nickel remains in solution. When the deposit of sulphuret of zinc has been carefully collected at the bottom of the vessel, it is filtered, and washed with water containing a little monosulphuret of potassium; the operation is completed by means of known methods.

The filtered liquid contains all the nickel in the state of double cyanide: to isolate it, we must commence by destroying the cyanogen, which the author effects, either by means of aqua regia, or by boiling with chlorate of potassa; after this, we precipitate with potassa.

The monosulphuret of potassium, prescribed by Wöhler, cannot be substituted by hydrosulphate of ammonia; moreover, care must be taken to employ potassa free from silica.

Annalen der Chemie und Pharmacie.

INORGANIC CHEMISTRY.

NEW RESEARCHES *on the* METALS *which accompany* PLATINUM *in its* ORE. By M. E. FREMY.

THE Memoirs which I have had the honor of communicating to the Academy during some years past, on the metallic acids and on the hydrated oxides, belong to an entire work in which all the combinations of the metals with oxygen will be successively passed in review, with the object of filling up any vacuities in their history, and of furnishing some essential documents to the classification of the metals.

Long since I proposed to examine in this point of view the various oxides formed by the metals which accompany platinum in its ore; but hitherto the scarcity of the primary matter had prevented me from completing a work which I had formerly commenced.

MM. Démontis and Chapuis, the skilful makers of the platinum instruments, which are employed in our laboratories and in chemical manufactories, have been kind enough, with a liberality for which I cannot too highly thank them, to place at my disposal all the products which I could require in my researches; I have hastened to submit to a complete examination the various residues which were given to me, and I now communicate to the Academy the first results of this work.

Former researches had taught me that the residues of platinum ore present a variable composition, and give, when treated, uncertain products. Besides, all chemists know that the metals which accompany platinum are difficult of preparation, and that the characters of their solutions are not constant: thus, M. Claus has shewn us within the last few years, that the salts of iridium always contained a certain quantity of ruthenium, and I have myself had an opportunity of proving that the properties of the salts of rhodium often differed from those which had been given by Berzelius.

I have, therefore, thought that in the work I have undertaken, the important point was first to analyse with accuracy the various residues of platinum ore, and afterwards to find a certain method of preparing with facility the various metals which are found mixed with the platinum: I think that I have resolved this question satisfactorily.

From my analyses it results that the residues of platinum may, as regards their composition, be divided into three classes.

1. *The residue in powder* is a mixture of iridium and rhodium; it arises from acid solutions precipitated by iron, and retains only small quantities of osmium: the metals which form this residue are dissolved in *aqua regia* with the aid of bichloride of platinum.

2. *The residue in bracteæ*, which is known to all chemists, and to which the name of osminret of iridium is improperly given, is a quaternary alloy of iridium, ruthenium, rhodium, and osmium; rhodium is found in these bracteæ only in very small quantity.

3. The third residue, which I shall call *residue in grains*, is formed essentially of rhodium, osmium, and iridium.

Thus, to obtain rhodium, we should operate on the residues in powder and in grains; each of the three residues is suitable for the

preparation of iridium: the residue in bractea alone is to be used for the preparation of ruthenium: osmium must be chiefly extracted from the residues in grains and in bractea.

These analytical facts being established, I shall now make known the method which I employ for attacking the residues of platinum ore; it differs entirely from that which I described in a former work, and rests, on one hand, on the great fixity of oxide of ruthenium, and, on the other hand, in the striking analogies which osmium bears to arsenic. I will take as an example the residue in bractea.

Oxide of ruthenium being capable of supporting red heat without being decomposed, and osmium undergoing an actual roasting by the action of oxygen, giving rise to a volatile acid like sulphur and arsenic, I thought that the residue of platinum ore might be submitted to roasting and be decomposed like the metallic sulphurets and arseniurets; experiment most happily confirmed my view, and gave a very interesting result which I had not foreseen; for, in roasting the residue of platinum, not only did I obtain very pure osmic acid, in large quantity, but I produced also, in well-determined crystals, oxide of ruthenium, which was not previously known in that state.

Without dwelling here on the groping in the dark which I had to go through in order to regulate the roasting of the residue of platinum, I will describe the apparatus which I employed for a long time in my laboratory, and which, at my request, is now mounted in the manufactory of MM. Démontis and Chapuis; it will enable chemists to be furnished with products which were previously but little known, and which cannot fail to receive some interesting applications.

I thought, for a long time, that the residue of platinum ore could only be easily roasted in a current of oxygen; but I now effect this roasting by means of atmospheric air freed from the organic bodies which it contains by passing through a tube filled with pummice-stone soaked in sulphuric acid; the platinum residue is heated to redness, and placed in an earthern, or, better still, in a platinum tube; the air is drawn into the apparatus by means of an ordinary aspirator with a flow of water; the tube is connected with a series of glass tubes in which the osmic acid condenses; it contains, moreover, in the part next to the furnace and communicating with the glass flasks, some pieces of porcelain, which are covered, during the operation, with beautiful crystals of oxide of ruthenium, which is not volatile, but which is carried off by the vapors of osmic acid: the atmospheric which has traversed the flasks, and which is saturated with vapors of osmic acid, passes into a solution of potassa and returns finally into the aspirator; the osmiate of potassa thus produced is treated with a few drops of alcohol and reduced to the state of crystallised osmite of potassa, insoluble in alcoholised water.

This roasting does not then present any difficulty, and gives the following products:—1. Very pure osmic acid, the proportion of which often exceeds 40 per cent. of the weight of the residue of platinum employed; 2. osmite of potassa, from which metallic osmium and the derivatives of that metal may easily be prepared, by a method which

I have described in a former Memoir; 3. crystallised oxide of ruthenium; 4. an alloy of iridium and rhodium which remains in the roasting-tube.

The operation which I have just described, is not only important as regards the preparation of such bodies as osmic acid and oxide of ruthenium, which, heretofore, could be obtained only with difficulty, but it completes in some measure the history of osmium, by demonstrating that this body differs entirely, in all its properties, from the metals which accompany platinum, and which really seem to play in platinum ore the part which arsenic fulfils in the metallic arseniurets.

It may be anticipated, from this time, that osmium will combine with hydrogen, and that, like arsenic and phosphorus, it may enter into organic molecules, and produce some of those very interesting compounds which have been discovered of late years.

I have undertaken some researches in this direction, which I shall hereafter submit to the Academy; I will now content myself with announcing the existence of a more oxygenous acid than osmic acid, which I have produced by submitting the osmiates to the action of oxygen and oxidising compounds: if this acid is represented, as some analyses have led me to think, by the formula OsO^6, the series of oxidation of osmium will become:—

$$OsO;\ Os^2O^3;\ OsO^2;\ OsO^3;\ OsO^4;\ OsO^5;$$

and will present remarkable analogies with those of nitrogen, phosphorus, and arsenic.

The new acid is not very stable; it forms with potassa and soda salts, colored of a deep brown, which crystallise in alkaline liquors.

I have said above that the roasting of the platinum residue, left as a fixed product an alloy of iridium and rhodium; this alloy is often mixed with oxide of ruthenium, which the vapors of osmic acid have not carried away, and still retains traces of osmium.

I first extract the oxide of ruthenium by heating the residue with potassa in fusion, which dissolves the metallic oxides, and I afterwards separate the iridium from the rhodium by the following method, which differs little from that previously discovered by Wöhler.

I heat the alloy with 4 parts of nitre: the mass is treated with boiling water, which frequently abandons, in cooling, fine octohedral crystals of osmite of potassa; the residue is treated with *aqua regia*, which converts the iridium into chloride; this body afterwards combines with the chloride of potassium, forming a double salt, which dissolves in boiling water, and crystallises by cooling; the insoluble residue is mixed with common salt, and treated, at a dull red heat, with a current of dry chlorine; a double chloride of sodium and rhodium is then formed, which is soluble in water, and which is deposited from its solution in violet octohedral crystals, often of considerable size.

The very easy preparation of the salts of rhodium, by the process which I have just described, will enable us to complete the study of this metal, which is recommended to the attention of chemists, by its insolubility in *aqua regia*, its metallic lustre which approaches to that

of silver, the beauty of its crystalline combinations, and especially its analogies to platinum and iridium.

I have already ascertained that the salts of rhodium, under the influence of ammonia, give rise to a class of ammoniaco-rhodic salts, in which the quaternary base, containing the elements of ammonia and oxide of rhodium, forms, with the various acids salts which crystallise easily, and which correspond to those which platinum, palladium, and iridium form in the same circumstances.

All these new facts will be developed in detail in a second communication; for the object of this work is merely to make known a method of extracting with facility the rare metals which constitute the residues of platinum ores.

Comptes Rendus, No. 23, June 5th, 1854.

On the Decomposition, *with* Hydrochloric Acid, *of* Sulphate *and* Phosphate *of* Lime.

Letter from M. Cari-Mantrand *to* M. Dumas.

At the commencement of last February, having had occasion to prepare phosphorus in the ordinary manner, with acid phosphate of lime, I was struck with the complexity, the length, and especially the small produce of the operation. As this experiment had been performed in such conditions of success that it would have been difficult, I may almost say impossible, to realise them in the large way, I thought myself warranted in concluding that the process at present adopted for extracting this valuable body from bones was defective, and was not the best means of action. I then asked myself why, since Scheele and Gahn had furnished industry with this process, had no one thought of finding a reaction by which we might directly, and entirely extract the phosphorus from bone-phosphate of lime. Not having yet done anything in chemistry, and fearing, at the outset of my career, to venture my experience on questions of too high an order, and of a purely scientific interest, I set myself this problem to solve, feeling that this kind of work was more within my reach than the investigation of new bodies.

I therefore gave myself up to the pursuit of the reaction, which should give directly all the phosphorus of the bones. All at once the following struck me:—

$$PhO^5, 3CaO + 8C + 3HCl = 8CO + 3CaCl + 3H + Ph.$$

It now remained to ascertain whether experiment would confirm this theory, and whether the symbols of this formula were the expression of facts. For this purpose, I introduced into a porcelain tube an intimate mixture of equal parts of wood charcoal, in fine powder, and bone ashes. Placed on a long furnace, this tube communicated by one of its extremities with an apparatus ready for disengaging dry hydrochloric acid gas, whilst a glass adapter, bent at right angles, and adjusted to its other extremity, connected it with a flask half filled with water,

fulfilling the office of a receiver. All being thus arranged, I gradually heated the tube to bright redness, and then I passed the hydrochloric acid gas over the incandescent matter. Almost at the same moment abundant vapors of phosphorus, carried along by a rapid disengagement of carbonic oxide, began to condense in the cool parts of the adapter. The experiment began to justify my anticipations. Thus then, incapable of itself, of displacing the oxygen of the lime base of the phosphate (a fact which I ascertained in the course of my researches), the chlorine of the hydrochloric acid, under the influence of the great affinity of carbon for oxygen, combined with the calcium to form chloride of calcium, and the phosphoric acid liberated, decomposed in its turn by the charcoal, yielded the whole of its phosphorus. I continued the experiment so long as a disengagement of hydrochloric acid was attended by a disengagement of carbonic oxide. At the end of an hour, seeing that the sublimation of the phosphorus did not progress, I stopped the operation. When the tube was cooled, I found only chloride of calcium in globules in the excess of charcoal employed. Analysis did not detect a trace of phosphorus in the carbonaceous residue. The phosphorus contained in the adapter was easily recognised to be such by its physical and chemical characters.

Before devoting attention to the industrial bearing of this experiment, I wished to discuss it; and I at once asked myself whether the hydrogen of the hydrochloric acid performed a part in the operation, and whether the disoxygenisation of the lime should be attributed to it; in other words, whether the decomposition of the phosphate should be explained by the following equation :—

$$PhO^5, 3CaO + 5C + 3HCl = 5CO + 3HO + 3CaCl + Ph.$$

In order to satisfy myself on this point, I repeated the experiment substituting dry chlorine for the hydrochloric acid gas. Subjected in the same conditions of temperature to this agent, the mixture of bone phosphate of lime and charcoal was converted, with even more rapidity than by the intervention of hydrochloric acid gas, into phosphorus, chloride of calcium, and carbonic oxide. In this experiment, when the current of chlorine is properly regulated, not a bubble of gas passes over which is not entirely absorbed, and converted into chloride of calcium. Being unstable at the temperature of the operation, the chlorides of phosphorus cannot exist, and the phosphorus distils over entirely. This second experiment would tend, it appears to me, to prove that, in the decomposition of the phosphate by hydrochloric acid gas, in presence of charcoal, the part taken by the hydrogen is purely passive; however, I have ascertained that, when the temperature is not very high, this gas converts a small quantity of phosphorus into phosphuretted hydrogen.

Supposing that in practice this process does not present any serious difficulties in its execution, it would present the following advantages over the old one:—Reduction of manual labor, and the extraction of the whole of the phosphorus contained in the bones; and finally, the common salt and sulphuric acid employed for producing the hydro-

chloric acid gas would be compensated for, or nearly so, by the sulphate of soda formed.

As a corollary to these results, I have thought that it would be interesting, and even practicable, to apply this same method of decomposition to native sulphate of lime. The following are the observations I have made on this subject:—

Intimately incorporated with charcoal in sufficient quantity for this element to remove, under the form of oxide of carbon, the oxygen of the lime and that of the sulphuric acid, sulphate of lime, treated at a red heat, with hydrochloric acid gas, is very readily decomposed. The products of the combustion are chloride of calcium, carbonic oxide, vapor of sulphur, and a little sulphuretted hydrogen.

This experiment does not constitute a new fact, for it was already known that sulphate of lime calcined with charcoal gives sulphuret of calcium, and that sulphuret of calcium, treated with liquid hydrochloric acid, is decomposed into sulphur, sulphuretted hydrogen, and chloride of calcium. The difference consists only in the mode of operating. But I went further; I ascertained that sulphate of lime alone, treated at a red heat with hydrochloric acid gas, is converted into chloride of calcium; the sulphuric acid eliminated distils over, partly as such, the remainder, under the influence of heat, being decomposed into sulphurous acid and oxygen.

For a long time I indulged the hope that this reaction might be turned to account in an industrial point of view; but it would appear, according to M. Kuhlmann, who has been kind enough to repeat the experiment on a very large scale, that the great quantity of hydrochloric acid necessary for effecting the decomposition of sulphate of lime, and the necessity for drying the gas, are obstacles which are unfortunately fatal to the manufacture.

This reaction will, therefore, possess no merit but that of presenting a contradiction to Berthollet's law concerning double decompositions by the dry way, since, in this experiment, we find sulphate of lime, a compound endowed with great fixity, converted into chloride of calcium, a very volatile compound.

The action of dry chlorine gas on sulphate of lime, at a high temperature, also invalidates the law of that celebrated chemist. In this case also, the sulphate of lime is converted into chloride of calcium; a small portion of anhydrous sulphuric acid distils over; the remainder is disengaged under the form of sulphurous acid and hydrogen.

Treated at a red heat with dry chlorine, the sulphates of potassa and soda, which are more fusible, and, therefore, more easily attackable than the sulphate of lime, give a greater quantity of anhydrous sulphuric acid. I likewise found in the products of the decomposition of these two salts, a very small quantity of a brown, viscid liquid, fuming in the air; put in contact with a small quantity of water, this liquid produced a sharp hissing sound, followed by a brisk effervescence of hydrochloric acid gas, and the liquor contained ordinary sulphuric acid. This liquid burned the skin powerfully. This body, probably, results from the combination of the chlorine with the anhydrous sulphuric acid.

To close this series of experiments, I tried, only once, to extract directly phosphoric acid from the bone phosphate, by means of the same process. Contrary to my expectation, the failure was complete. On this point, Berthollet's law was victorious. But, on the other hand, by adding to the phosphate of lime only the quantity of charcoal strictly necessary for removing the oxygen from the lime, I succeeded in producing, by means of dry chlorine, a very large quantity of anhydrous phosphoric acid, mixed with a little phosphorus resulting from a partial reduction of that acid, and a little chloride of aluminium arising from an action on the porcelain tube.

Comptes Rendus, No. 20, May 15th, 1854.

On the Sulphites *of* Copper *and* Mercury.

Report of MM. Pelouze and Balard on several Memoirs, by M. Leon Pean de Saint-Gilles.

M. Pean de Saint-Gilles has given a summary of the results of long and careful labor in two Memoirs relative to the history of the sulphites of mercury and copper, and has elucidated this hitherto unexplored portion of the history of the salts.

The suboxide of mercury, which is an unstable and feeble base, appears incapable of combining with sulphurous acid. M. Péan de Saint-Gilles' endeavors to obtain this combination have been as vain as those that have been made to produce that of cyanogen and mercury corresponding to the protoxide; but he has studied in detail the histories of the sulphates of binoxide of mercury, which he succeeded in obtaining in a satisfactory state of purity.

He has avoided the causes of error into which all who preceded him in this kind of research had fallen; and who, trying to combine directly sulphurous acid, which has so great a reducing action, with oxide of mercury, which is so disposed to part with a portion of its oxygen, had obtained, instead of a pure sulphite, a mixture of protosulphate and deutosulphite, described as a simple deutosulphite, and whose singular constitution appeared to demand further examination. M. Péan de Saint-Gilles decomposed the alkaline sulphites by means of nitrate of mercury free from excess of acid, whether neutral or bibasic, and he obtained two species of sulphites of mercury, the one neutral, the other with 2 equivalents of base, whose formula is $SO^2 + 2HgO$.

The simple inspection of this formula shews, that if the oxygen of one of the molecules of base be added to the acid it would be transformed into a neutral sulphate of protoxide, a body which presents to us with the foregoing approximation, derived from inorganic chemistry, and is analogous, to a certain point, to that which M. Dumas pointed out long since between acetate of methyline and formiate of etherine. Hence we see, according to the well known instability of the sulphites, that a molecular transposition which could not be realised with these ethereal compounds may be obtained with the new mercurial sulphite. This

curious fact has been proved by M. Péan de Saint-Gilles. If this basic sulphite, when quite dry, be simply rubbed with a metal plate, it is transformed, without either losing or acquiring anything, into sulphate of protoxide, and this by producing a projection of matter, a disengagement of heat, and a slight noise resembling the deflagration of the chlorates.

The tendency of the neutral sulphite to form double sulphites is very decided. Likewise, when to the solution of an alkaline sulphite oxide of mercury is added, the latter dissolves, eliminating half the alkaline base which remains in the liquor in the caustic state, another approximation between the sulphites and cyanides. This fact, added to others which are already known, and especially to the action of oxide of silver and metallic silver on the cyanides, and even on the most resisting chlorides, shows how much the tendency to form complex compounds may enable some oxides, apparently not energetic, to balance, in certain cases, the affinities of the most powerful alkalies.

These alkaline sulphites have not a decomposing action of the same order on the haloïd salts of deutoxide of mercury; they simply combine with them, thus producing saline compounds, differing at once in acid and base, which aids in demonstrating that if, in some cases, the double salts may be considered as simple salts with multiple bases, there are others in which the molecular juxtaposition of two quite different salts, each retaining their individuality, can hardly be contested. The tendency to form these double compounds is, moreover, very decided; the alkaline sulphites can likewise transform the protochloride of mercury into metallic mercury and bichloride, with which they combine, by a mode of action analogous to those of the alkaline chlorides and cyanides.

The study of the sulphites of copper likewise gave M. Péan de Saint-Gilles some interesting results. He was unable, even with the greatest care, to obtain simple sulphites with any oxide of this metal. But he proved the great tendency which these salts, which cannot exist separately, have to produce double compounds of great stability. A constitution of this order is possessed by the red salt, obtained by M. Chevreul, and in which M. Péan de Saint-Gilles has found the composition assigned to it by M. Rammelsberg, who represents it as a cupreo-cupreous sulphite with 2 equivalents of water. But, besides this interesting compound, there is another characterised by a yellow color and by its easy solubility in acetic and sulphurous acids, which are without action on M. Chevreul's sulphite. This compound, confounded, through its properties, with hydrate of copper, only differs from the red sulphite by the presence of 3 more equivalents of water.

But it is difficult to believe that a simple difference of hydratation, which would doubtless explain the difference in color of the two products, should cause so great a modification in the chemical properties, and we cannot avoid inquiring whether there is not likewise a dimorphous modification which would deserve further investigation from M. Péan de Saint-Gilles.

M. Chevreul's red salt is a type representing the constitution of

other sulphites. If we suppose the cupreous sulphite replaced by that of potassa or ammonia, we obtain double sulphites with an alkaline base and protoxide of copper, of which M. Péan de Saint-Gilles describes several well defined species, at the same time that he eliminates others which have been ill studied. In this salt also the cupreous sulphite may be replaced by binoxide of mercury and a double sulphite thus obtained, remarkable for its great solubility in water, which contrasts with the insolubility of the simple salts which form it, as well as with the generality of sulphites, with the exception of the alkaline sulphites.

There are, moreover, double alkalino-cupreous sulphites of a different type. M. Péan de Saint-Gilles has obtained one which, produced in a great excess of sulphite of ammonia, contains to one equivalent of sulphite of copper, not one, but seven equivalents of this alkaline sulphite.

The salts of the first type will combine with each other, equivalent for equivalent, so as to form still more complex salts, and which must be called, with Berzelius, *double* double salts; salts which are analogous in their composition to the complex compounds, formed by the union of some double cyanides.

Most of these saline species, even those whose composition appeared most uncertain, have been obtained by M. Péan de Saint-Gilles with regular forms, a constancy of composition which has been several times proved,—in one word, with all the characters which can guarantee their existence as perfectly distinct compounds. Their isolation and the determination of their nature give evidence of careful and well-directed study.

Comptes Rendus, No. 8, 1854.

INDUSTRIAL CHEMISTRY.

On the APPLICATION *of* MUREXIDE *as a* COLORING MATTER *for* WOOL.

THE beautiful researches of Liebig and Wöhler upon uric acid and its derivatives made us acquainted with a peculiar substance, to which they gave the name of alloxan. This body is obtained by adding very gradually 1 part of uric acid to 4 parts of nitric acid, of a specific gravity of from 1·45 to 1·5. The uric acid is dissolved with evolution of nitrogen and carbonic acid, accompanied by a considerable rise of temperature, which must be prevented as much as possible; on cooling, the mass becomes nearly solid, from the deposition of white granular crystals of alloxan. If these crystals be drained and dissolved in a very small quantity of water, and exposed to spontaneous evaporation in a moderately warm room, large, brilliant, colorless crystals, in the form of short right rhombic prisms, will be obtained. Alloxan is remarkable for the facility with which it undergoes changes when treated with different substances, and for the number of curious compounds thereby produced. Thus if sulphuretted hydrogen gas be passed through a solution of it, sulphur is precipitated and a new body formed,

to which the name of alloxantine has been given; or if its solution be slightly acidulated and a slip of zinc placed in it, the same body will be produced under the influence of the nascent hydrogen evolved during the solution of the zinc. Alloxantine being sparingly soluble in cold water, readily separates in crystals, which may be obtained pure by solution in hot water, for, unlike alloxan, it is not decomposed by continued boiling. If 4 parts of alloxantine and 7 of alloxan be dissolved in 240 parts of boiling water, and 80 parts of carbonate of ammonia be added, a very peculiar body will be formed, which will crystallise on the liquor cooling. These crystals are of a beautiful garnet-red color by transmitted light, and have a beautiful iridescent green by reflected light. To this body the name murexide was given, from the Murex, or shell-fish, from which it was supposed the Tyrian purple was formerly procured. Previous, however, to the experiments of Liebig and Wöhler, Dr. Prout had described the same substance under the name of purpurate of ammonia, but obtained in a somewhat different way. So readily is this body formed, that a solution of alloxan will stain the skin purple in consequence of its production. This fact led its second discoverers to imagine that, like the Tyrian purple, it might be employed as dye-stuff. The difficulty however of obtaining it, and of fixing it upon the fabric when formed, prevented for the time the idea from proving fertile.

Some time since, however, Dr. Sacc turned his attention to the subject, and, led by the fact above mentioned, that a solution of alloxan stained the skin, came to the conclusion, that by impregnating a piece of woollen cloth with that substance, he might be able to produce the murexide directly in the tissue. He tried the experiment, and succeeded in dyeing a piece of cloth of an amaranthus tint. He communicated the results of his first experiments, still incomplete, to M. Albert Schlumberger, who has succeeded, by modifying and completing the experiments of Dr. Sacc, in rendering the process, merely indicated by the latter, perfectly practicable.

His process is simple enough. He prepares a solution of alloxan, formed of 30 grms. of alloxan to each litre of water, and soaks the tissue to be dyed in it, the excess of liquid being then squeezed out in the ordinary way, or by pressure between rollers. The cloth is then dried by a gentle temperature, and after an ageing of twenty-four hours the color is brought out by passing the cloth over a roller heated to 212°F. For this purpose the drying-machines, composed of several drums, would answer perfectly, the cloth being successively passed over each, the greatest care being taken to avoid folds; woollen yarn and wool should be put in a stove heated by steam. According as the heat is communicated to the cloth, a magnificent purple tint, makes its appearance as if by magic. The intensity varies according to the strength of the solution of alloxan which has been employed. It is only necessary to wash the cloth in cold water to give to the shade its full brilliancy.

M. Sacc found that the finest and most vivid shades could only be communicated to the tissues mordanted with salts of peroxide of tin,

and M. Schlumberger has confirmed this observation. Cloth not mordanted did not give very satisfactory results, even after a prolonged exposure to warm and damp air. He obtained the most satisfactory results by soaking the cloth in a solution composed of equal parts of perchloride of tin and oxalic acid, of a specific gravity of 1·006. In this solution, at a temperature of about 100° F., the cloth is to be allowed to remain for an hour, then rinsed and dried, and is then fit to be treated with alloxan. If stronger solutions of the mordant be employed, there is a considerable loss of coloring material, and a deterioration of the shade. This may be attributed to the presence of too great an excess of stannic acid, which from its opacity may mask the murexide, or by its acid reaction may decompose it. This is especially the case if chloride of tin be employed instead of stannate of soda. Experience has shewn that fabrics freshly mordanted give better results than those which have been mordanted for some time; the depreciation in purity and brillancy of tint in the latter may even amount to 20 or 30 per cent.

Murexide, as we have already remarked, being produced by the action of heat and ammonia, it occurred to M. Daniel Dollfus, and the other members of the committee for the chemical arts, appointed by the Société Industrielle of Mulhouse, to report upon the Memoirs of M. Schlumberger, to try the effect of exposing a piece of cloth, treated with alloxan, to the vapors of ammonia. The result confirmed their anticipations, for the color was immediately produced without the necessity of ageing the cloth after its impregnation with the alloxan. There can therefore be no doubt that the best results will be obtained in future by the employment of ammoniacal vapors, for, besides the saving of time, there will also be a saving of alloxan. This substance is very liable to decompose, especially in the presence of even minute traces of reducing agents, such as protochloride of tin or sulphurous acid; traces of the latter substance always remain in the cloth after the operation of bleaching, no matter how well washed it may be, and would be quite sufficient to prevent the formation of the murexide.

As yet all the attempts that have been made to communicate the murexide-purple to cotton or silk have failed, that substance having an affinity apparently only for wool, to which it gives a very durable and permanent dye. Sun-light, so destructive to other purples, appears to have but little action upon that of the murexide; a piece of cloth dyed of a rose color had its tint scarcely altered by exposure to the full action of the strongest sunshine during two days, and the color was only fully discharged by an exposure of more than two months. Boiling water and steam completely destroy the color produced upon cloth mordanted with salts of tin; the decoloration commences in water at a temperature of about 153° F., and augments with the increase of temperature. This destruction of the dye is caused by the action of the mordant, for cloth dyed without the use of a mordant not only supports to a certain extent the action of boiling water, but even acquires a uniform, and perhaps a more beautiful and deeper tint than that given by prepared woollen fabrics. Further experience may shew

that hot water and the application of ammonia alone may be advantageously substituted for the mordanting and the passage overheated cylinders.

Cold alcohol and ether have no action upon murexide-purple; the former liquid destroys it at the boiling temperature, without being colored purple as water is. Alkalies, especially in a caustic state, are very destructive to it; if a piece of cloth dyed with murexide be dipped into a solution of caustic soda, it assumes a violet-blue color, and is then decolored. Soap, acting as a weak alkali, after a time, alters it. Chlorine has no immediate action upon it, at least not in weak solutions. Acetic and oxalic acids are not sufficiently energetic to immediately discharge the color. Hydrochloric, nitric, and sulphuric acids act as decolorisers; nevertheless the latter acts less quickly than the first two, and, what is singular, the color almost destroyed by sulphuric acid reassumes a rose-violet on immersing the tissue in ammonia.

Bichromate of potassa, chlorate of potassa, acetate of lead, and acetate of alumina, are without action upon murexide. This is not the case however with reducing compounds, such as protochloride of tin, sulphuret of ammonium, and protosulphate of iron, which destroy the rose-tint very rapidly; the protochloride of tin produces a blue tint before it decolors it. The reduction of the murexide gives birth to a new substance, which in its turn may reproduce that substance by a properly conducted oxidation.

From these reactions it is evident that the rose, amaranthus, and purple shades produced with the murexide, and which exceed those produced by all other means in richness and brilliancy of tints, have also the advantage of being the most solid and durable, an advantage which will no doubt be soon appreciated.

We have now to speak of the sources from whence the supply of uric acid may be obtained, should the employment of murexide become general. At present the price of that substance, which has never hitherto become an article of commerce, would be so high that the murexide-purple would be far more expensive than that produced with cochineal; but if we recollect, that, independent of the excrements of serpents, from which hitherto uric acid has been made, those of pigeons, and especially of all carnivorous birds, silk-worms, &c., and above all Peruvian guano, which may be obtained in immense quantities, are very rich in uric acid, and it may be produced from them at a very moderate price as soon as it becomes an article of commerce. No doubt, if necessary, fowl might be so fed as to produce it in much larger quantities than they do naturally.

Connected with this part of the subject, we may mention, that in the making of the alloxan from the uric acid, a considerable quantity of the former remains in the acid mother-liquid, from which the crystals of alloxan separate. This portion could not be used to impregnate tissues, in consequence of the nitric acid present, and would cause a considerable loss of material, and a considerable enhancement of the cost of the dye, unless it could be utilised. If a piece of zinc be introduced into the acid mother-liquid, alloxantine will be formed, which

may be recovered by evaporating the liquid and allowing it to separate out. This substance, as we have before remarked, will also produce the purple color, and a mixture of it with alloxan will afford the best conditions for its production.

M. Schlumberger has indulged in some curious speculations relative to the existence of this coloring matter ready formed in nature, which it may be interesting to notice. M. Sacc has found that poultry, and especially birds with very brilliant plumage, such as the different parroquets, do not produce sensible traces of uric acid during their period of moulting, whilst the quantity is very large when their feathers are fully developed. The question naturally suggests itself, what becomes of the uric acid in the former case? May it not be transformed by some as yet unknown metamorphosis in the animal body into a substance like alloxan, capable of coloring the feathers? Murexide, as we have observed, is green by reflected light; a substance then, which gives violet (red and blue) and green (yellow and blue) can undoubtedly produce all shades of colors, which are made up of those three colors. How curious if it should hereafter be found that murexide was indeed the source of all the varied hues of birds' plumage! Still further, it is chiefly those animals which have but one means of exit for their excrements, and who produce large quantities of uric acid, that exhibit a display of coloring. Thus, for example, we have the skin of the serpent and lizard, the scales of fish, the wings of butterflies, often colored in the most gorgeous manner, whilst the skins of the mammalia are dull, and without that iridescence and metallic lustre which is so characteristic of the coloring of some of the classes of animals mentioned. These are, however, mere speculations, but they nevertheless lead to a very unexpected supposition. The ancients were acquainted with a process for dyeing wool of a fine purple, which has been lost to our days, or at least is only practised in the East. Tradition however tells us that this beautiful purple tint was produced by pounding a quantity of small shell-fish, and adding to the mass either a quantity of urine in the state of putrefaction, or water in which some of the same shell-fish had been allowed to putrify. The cloth soaked in the liquid produced by these mixtures only developed the beautiful purple color after long exposure to the air, and probably to heat. This mode of producing the color so strikingly resembles that by which the new color of murexide is produced, that one is tempted to believe that the Tyrian purple was produced by that substance; and that many centuries before the beautiful discovery of Liebig and Wöhler, murexide was formed by the action of ammonia in the putrid matter employed upon substances derived from the uric acid which must exist in the intestines of the shell-fish pounded up.

Bulletin de la Société Industrielle de Mulhouse, No. 123, p. 242; and *Dublin Journ. of Industrial Progress*, June, p. 173.

On the PURIFICATION *of* ESSENTIAL OIL *of* BITTER ALMONDS.

By GEORGE WHIPPLE, F.C.S.

THE purification of oil of bitter almonds, and the means by which hydrocyanic acid can be most advantageously separated from the crude oil, having recently occupied the attention of Pharmaceutical Chemists, I beg to communicate some of the results of my experience as a drawer of this and other essential oils.

The method recommended by Dr. Maclagan (*Pharmaceutical Journal*, vol. xiii., p. 277) for removing hydrocyanic acid, is not only complex, but fails to render the oil permanently innocuous.—(*Vide* Pereira's *Materia Medica*, 3rd edit., p. 1776.)

Some years ago I thought of obtaining hydrocyanic acid and essential oil of almonds at one and the same time, by receiving the contents of the condensing worm into a solution of nitrate of silver, and subsequently decomposing the resulting cyanide of silver with hydrochloric acid. Since the publication of Dr. Maclagan's paper I have tried this process, and the oil obtained by it was submitted to Dr. Stenhouse for examination, and pronounced by him to be entirely free from hydrocyanic acid. Whether it be innocuous I leave to be decided by others.

When commercial oil of bitter almonds is mixed with solution of nitrate of silver, and the mixture allowed to stand for some time, crystals of cyanide and nitrate of silver are abundantly formed.

Thirty-two parts of the crude essential oil, sp. gr. 1.079, were redistilled, and received into a solution of nitrate of silver, when an abundant deposit was formed, containing cyanide of silver. The oil was collected, added to a second portion of nitrate of silver in solution, and again distilled, yielding six parts of a colorless oil of sp. gr. 1.051, in which no trace of hydrocyanic acid could be detected. This oil retains the flavor of the bitter almond.

In distilling the essential oil from almond cake, three oils differing in density are obtained. The mixture of these oils is opaque, but becomes bright on being filtered. These results, however, are not peculiar to the production of almond oil. Sassafras yields two oils, a stéaroptène and an odorless salt, very like common alum in appearance, but perfectly soluble in the oils from which it is deposited. This stéaroptène is deposited like the crystalline matter in oil of almonds, but so far as my experience has gone, the product from sassafras is always the same, which in the case of the almond I have not found to be so. The largest crystalline deposit from oil of bitter almonds that has come under my notice was of the consistence and color of honey. This when washed with alcohol afforded the lemon-colored crystals which were examined by the late Dr. Pereira, and a specimen of which is among the collection of the Pharmaceutical Society. Another remarkable deposit which I obtained from oil of bitter almonds was examined by Dr. Faraday, and said to be either benzoïc or amygdalic acid.

Subsequently on using steam for distilling the oil, all the crystalline deposits I have obtained, which have been examined by Dr. Stenhouse

and others, have been designated benzoïc acid. After this crystalline deposit has been formed it is sometimes dissolved and again re-deposited, in which respect it differs from what occurs in the case of oil of sassafras. I have never found, if the stéaroptène of sassafras is dissolved ln the oil from which it was deposited, that it is deposited again, nor have I been able to crystallise it after it has been liquefied.

We know that benzoic acid is instantly and largely dissolved by essential oil of almonds, and again deposited with change of temperature; and I have placed on the table some of the crystalline deposit thus formed. It will be observed that the appearance of the crystals is different from that of the crystals which are formed spontaneously in oil of bitter almonds. The latter appears to me more nearly to resemble the crystalline product obtained from balsam of tolu, which by Thomson and others has been designated benzoïc acid, but which has subsequently been shown to consist principally of cinnamic acid. This cinnamic acid is said to be formed by the oxidation of hyduret of cinnamyle, just as benzoïc acid is produced from hyduret of benzoyle. I have observed that during the formation of the crystalline deposit in oil of bitter almonds, water is at the same time produced, which floats upon the oil, a fact which supports the opinion of the crystalline matter being benzoïc acid.

Although the crude oil of bitter almonds contains a large but variable quantity of hydrocyanic acid, yet the hyduret of benzoyle is found to be insoluble in hydrocyanic acid, at least in the aqueous acid. Whether it be so in the anhydrous acid, as was hinted by Dr. Stenhouse, is an interesting subject for inquiry.

I may here notice the impropriety of employing the water obtained in the distillation of the essential oil of almonds for culinary purposes. The quantity of hydrocyanic acid contained in this water varies very considerably, and the strength of it is sometimes such as to render it highly dangerous. The solution of the essential oil in spirit, called essence of almonds, as commonly sold, is also, as I conceive, unnecessarily strong. It is usually prepared of the strength of one part of oil and seven of spirit. If made in the proportions of three fluid drachms of oil to sixteen fluid ounces of spirit, it would afford a better flavor, and be much safer.—*Pharmaceutical Journal*, May, 1854.

AGRICULTURAL CHEMISTRY.

Researches *on* Vegetation. By M. Boussingault.

§ 1. The question whether vegetables fix in their organism the nitrogen which is formed in the air, is not only interesting in a physiological point of view; its solution must throw much light on the theory of the fertility of the soil. Indeed, if nitrogen gas is not assimilable; if its office is limited to tempering, in some measure, the oxygen gas with which it is mixed, we can readily understand the utility, in manures, of organic matters which, by spontaneous decomposition, supply to the plants the elements of the nitrogenous principles which they ela-

borate. If, on the contrary, the nitrogen is fixed during the act of vegetation; if it thus becomes an integral part of the vegetable, we are very naturally led to this conclusion—that the greater part of the fertilising properties of manures resides in the mineral substances, in the earthy and alkaline phosphates and carbonates, which are always found in them in considerable proportion, for the nitrogenous element would then be superabundantly furnished.

It is true that at a rather distant period, when the eudiometrical methods were devised, it was supposed that a manifest absorption was observed during the development of a plant; but afterwards, Theodore de Saussure, by employing more accurate means, did not succeed in proving this absorption; on the contrary, the researches of that eminent observer tended to shew a feeble exhalation of gas; and if there have remained some doubts concerning this phenomenon, it is because the manometrical processes used by Saussure gave very decided results only in as much as a very considerable change supervenes, either in the volume or in the composition of the atmosphere in which the plant has remained. These processes are amply sufficient, for example, for shewing the fact of the decomposition of carbonic acid by the green parts of vegetables; because the action of the solar rays is revealed immediately by the appearance of oxygen gas; but the manometrical method is most frequently insufficient when it is required to decide whether there are a few cubic centimetres of gas absorbed or exhaled by a plant confined in a few litres of air. Also, when, many years ago, after having collected the facts which were favorable and opposed to the idea that vegetables take nitrogen from the atmosphere, I found that the question might be regarded as undecided, I followed, in the hope of solving it, an entirely different way from that which had already been tried. I compared the composition of seeds with the composition of the crops obtained only at the expense of water and air. The plant was developed in a soil previously calcined, in order to destroy all traces of organic matters, and it was watered with distilled water. It was afterwards proved that the vegetable had gained in carbon, hydrogen, oxygen, and nitrogen, during the course of its development. The following, as regards the proportion of nitrogen, are the results obtained in 1836 and 1837 :—

Plant cultivated.	Duration of the cultivation.	Weight of the seed.	Weight of the crop.	Nitrogen in the seed.	Nitrogen in the crop.	Gain or loss of nitrogen.
		gr.	gr.	gr.	gr.	gr.
Clover . . .	2 months	1·576	3·220	0·110	0·120	+0·010
Clover . . .	3 „	1·632	6·288	0·114	0·156	+0·042
Wheat . . .	2 „	1·526	2·300	0·044	0·040	—0·004
Wheat . . .	3 „	2·018	4·260	0·057	0·060	+0·003
Peas . . .	3 „	1·211	4·990	0·047	0·100	+0·053

It is evident :—1st. That, cultivated in a soil absolutely free from manure of organic origin, and under the sole influence of air and water, clover and peas acquired, independently of carbon, hydrogen, and oxygen, a quantity of nitrogen appreciable by analysis ; 2nd, that wheat, cultivated in the same conditions, took from the air and water, carbon, hydrogen, and oxygen ; but that analysis did not detect either a gain or a loss of nitrogen, without our being able, however, to conclude definitively that wheat does not possess the faculty of fixing a certain quantity of nitrogen.* As regards the nitrogen assimilated in these circumstances, analysis is incapable of indicating it, for this principle might have entered directly into the organism of the plants ; or else, as Theodore de Saussure thought, it might arise from the ammoniacal vapor from which the atmosphere is never entirely free, although it contains only an infinitely small proportion of it. Thus, in 1838, in consequence of the researches which I had undertaken, the question had resolved itself into the following terms :—Does the nitrogen assimilated by a plant grown in the open air, in a soil deprived of organic matters, proceed from nitrogen gas or from ammonia? I may add that, since then, the experiments tried for the purpose of solving it, have led to perfectly contradictory conclusions.

When we consider how small is the proportion of nitrogenous substances elaborated by a plant placed in a sterile soil, even when the vegetation has been continued for several months, we are not much disposed to believe in the intervention of the nitrogen of the air ; for if this gas did intervene, we do not see why its assimilation should be so restricted, since it predominates in the composition of the air. We can better understand, on the contrary, the smallness of the quantity of nitrogen assimilated under the hypothesis of the sole intervention of ammoniacal vapors, for this reason—that as the atmosphere contains only, so to speak, traces of carbonate of ammonia, it can furnish only a very limited quantity of nitrogenous elements to a vegetation accomplished under the sole influence of air and water.

§ 2. The first idea which presents itself to the mind for deciding whether the fixed nitrogen arises from that which the atmosphere contains in the gaseous state, is to arrange an apparatus in which the plant should grow in air freed from ammonia, and which should be renewed incessantly during the day, in order to ensure it sufficient carbonic acid as a source of carbon.

However, on reflecting, it is to be feared that such an arrangement does not present all the guarantees to be desired ; for if the air traverse the apparatus with great speed, and it must be thus in the cases in which carbonic acid is not added, we should not be certain of retaining all the ammoniacal vapor, all the organic corpuscules in the purifying system, consisting naturally of a series of tubes containing fragments of pumice moistened with sulphuric acid. Moreover, supposing even that the purification of the air has been complete, and that, notwithstanding, nitrogen has been fixed during the vegetation, all that we could strictly conclude would be that this nitrogen did not proceed from the

* See "*Annales de Chimie et de Physique*," 2e série, t. lxvii., p. 52.

ammonia; for, in order that it may be admitted that it has formed part of the air in the gaseous state, we must be able to affirm that, independently of the volatile ammoniacal compounds and dust of organic origin, the atmosphere does not contain, in proportion small enough to escape the ordinary processes of analysis, other principles capable of aiding in the formation of nitrogenous substances in vegetables. Thus it would be only in the cases in which experiment proved that there was no assimilation, that the method could be regarded as satisfactory.

For these reasons, in the researches which I have undertaken, I have preferred to make the plant live in an atmosphere which was not renewed. My experiments were commenced in 1851 and concluded in 1853.

The experiments made in 1851 and 1852 were conducted in apparatus similar to that which I have just described. The seeds were put into pummice-stone reduced to small pieces, and freed from the too fine particles by sifting, then washed, calcined, and allowed to cool, with the precautions previously indicated. I always introduced into the pummice soil, after calcination, some ash obtained from farm-yard dung, by incineration at a low temperature. The manure was first chopped up, well mixed, dried, and then burned. As it is perfectly proved that dung is suitable for all cultures, its ashes naturally contain all the mineral substances necessary for the plant. The proportion was varied according to the soil; and, most frequently, there was also added some ash from seeds similar to those on which the experiment was made.

The pummice being well moistened with water free from ammonia, was allowed to remain under the bell-glass for twenty-four hours, before planting the seed in it.

The fundamental principle of the method consists, as I have said, in determining the quantity of nitrogen contained in a seed, and afterwards the quantity of nitrogen contained in the plant raised from a similar seed, the vegetation being, moreover, accomplished in such conditions, that all access of nitrogenous organic substances is strictly prevented. The question is, indeed, to ascertain, by means of analysis, whether the crop contains a quantity of nitrogen equal to or greater than that which the seed contains.

When the crop is gathered, we estimate the nitrogen in the plant, in the soil, and even in the pot; the material of which, owing to its porosity, absorbs and retains water charged with organic substances.

The plant, after desiccation in a drying-stove, maintained at a gentle heat, is cut into very small pieces by means of scissors; when it is thus divided, and all parts of it thoroughly mixed, a portion of it is analysed, and from this analysis the quantity of nitrogen contained in the whole is estimated. This is the ordinary mode of procedure, and this was the course which I formerly adopted; but now I think it necessary to criticise this practice. The plant, although divided and mixed, is not sufficiently homogeneous for us to be certain, in the case of so very delicate an appreciation, that the fraction acted on represents the constitution of the whole. It is preferable, as I have always done in my more recent researches, to operate on the whole of the

crop, employing combustion-tubes of large dimensions, and performing, when necessary, several operations. The error by which the result is then affected, is that which is inherent to the process itself, and whatever may be its amount, it is not multiplied by 3, 4, 10, or 100, according as we have analysed only 1–3rd, 1–4th, 1–10th, or 1–100th of the plant gathered. It is particularly in this instance of the estimation of the nitrogen in organic remains diffused in the soil which has contained the roots that it is important to operate on large quantities of matter. I have been able, by means of large tubes of Bohemian glass, to analyse either the whole of the soil, or large quantities of it; so that, in the most unfavorable cases, the error was not more than tripled. By proceeding otherwise, by submitting, for example, to analysis, only 1 grm. of matter, and performing two or three operations, we might arrive at the most erroneous result; because the dried soil of a single experiment sometimes weighs nearly 1 kilogramme. The error being made, and there is not an analysis free from error, and multiplied by 333 or by 500, if we suppose it to be half a millegramme only, that which would be committed in the quantity of nitrogen contained in the soil would amount to from 0·15 to 0·25 grs. It would certainly be better not to take account of the nitrogenous matter retained by the pummice or by the vessels; for when the plant has not languished, when no leaves have fallen, and when the remains of the roots have been careful y removed, the organic substance mixed with the soil is very trifling, and the quantity of nitrogen which enters into its constitution is not of a nature to change the tendency of the results deduced from comparative analyses of the seeds and the crop.

The estimation of the nitrogen was made by Warrentrapp's process, as modified by Peligot. The normal acid was prepared with the greatest care; however, as it was necessary especially to prove differences, I, as often as possible, employed the same acid for estimating the nitrogen in the seeds and in the crops. In operating on a large quantity of pummice soil, containing only a small proportion of remains of plants, from 20 to 30 grms. of matter were introduced into a large tube, after having been well mixed with soda-lime, and the ammonia resulting from several combustions was received into a single pipette of normal acid, in order to diminish as much as possible the error in the determination of the strength. By allowing the Bohemian glass-tube, in which the matter had been burned, to cool slowly, I almost always avoided breaking it. I have been able, by means of this precaution, to use the same tube eight or ten times.

I have called particular attention to the *clearing* which is effected at the end of each analysis, by the decomposition of oxalic acid placed at the bottom of the tube. It is known that the object of this operation is to convey into the acid liquor, with the hydrogen and the aqueous vapor produced in this case, the last traces of ammonia formed under the influence of the alkaline hydrate. This manipulation, when it is not properly executed, very perceptibly affects the results. The loss of nitrogen occasioned by an insufficient *clearing* is so much the greater, as the substance examined contains more nitro-

gen, or else, for equal quantities of nitrogen, as the substance which contains them contains less of organic matters capable of furnishing hydrogen gas or steam during the combustion. It is thus, for example, that for the same quantity of nitrogen, a very humid substance will give perhaps all the ammonia produced before the decomposition of the oxalic acid; whilst, if it has been dried before being introduced into the tube, all the ammonia will be driven out only by means of a well-sustained current of gas, or aqueous vapor. The reason of this is very simple:—it is that, in the first case, the ammonia will be carried away by the steam which is developed throughout the duration of the operation. From very numerous experiments, I am led to believe that 1 grm. of oxalic acid, in decomposing, is not always sufficient for completely expelling the ammonia, when we analyse a substance containing from 3 to 4 of nitrogen per cent.; thus I have always employed at least 2 grms. of this acid in the estimations executed in the course of my researches.

If, in a soil freed from organic matters, containing ashes of dung, and suitably moistened with water free from ammonia, we sow thickly seeds of good quality, and then enclose the seed-bed in a confined atmosphere, provided with a proper proportion of carbonic acid, the following is what ordinarily occurs:—All the seeds germinate; at a certain period, the color of the leaves, the size and rigidity of the stems, —in a word, the vigor of the vegetation is comparable to those of the same plants grown in a fertile soil. But if, from this prosperous condition, before gathering, we were to conclude that the plants have found in the confined air and the water contained in the soil, all the elements which have contributed to their development, we should be exposed to a mistake which analysis would speedily clear up. Indeed, if the plants have acquired great vigor, it is because in reality they have not vegetated in a sterile soil: it is sufficient to count them to ascertain that they are not so numerous as the seeds sown; there would not be room for all, and those which have perished have served as manure for those which continued to exist. In this case, the experiment, although interesting, will become complex, as I shall show in this Memoir. The soil naturally remains charged with a large proportion of organic substances; in fact, we are no longer in a position to judge how the vegetable has behaved, which, apart from the matter of its own organism, has had for its development only atmospheric air, carbonic acid gas, water, and mineral substances.

In my researches, I have constantly obtained a number of plants equal to that (which was very limited) of the seeds I had sown. I have found in this the advantage that the soil contained only very little organic remains, because, having only one or two plants to act on, I stopped the vegetation when I found the vigor of the plant diminishing, before the leaves began to fall. The crops, once dried, were of a weight which admitted of their being analysed entire; in oneor two operations, an essential condition, and which I regard as most favorable to the clearness of the results.—*Comptes Rendus*, March 29, 1854.

(*To be continued.*)

Adulteration of Guano. By J. Thomas Way.

In a lecture recently delivered before the members of the Royal Agricultural Society of England, I had occasion to remark that the adulteration of guano had this year reached a height to which it had in no previous season attained. You will understand that I did not speak without knowledge on this subject, for the number of analyses of manure necessarily passing through my hands enables me to form a very fair opinion of the state of the manure market at any particular time.

Several circumstances have conspired to offer this spring to the adulterators of guano a more promising field of exertion than usual. In the first place, from causes into a consideration of which I need not enter, the supply of guano from Peru was, especially at the early part of the season, very limited. The importers could not meet the demand, and were obliged to refuse orders. Even at this moment I find that guano is selling in many places in the country at 13*l* to 14*l* per ton. In ordinary years the demand for one kind of manure bears some relation to the supply of manures in general. Thus, if one article upon which the farmer is wont to rely is not forthcoming, he turns to another. If he cannot get guano for his green crops he will try superphosphate of lime or *vice versâ*—there is, in fact, a certain equivalency in different manures which regulates the demand for them. After guano, superphosphate of lime is, perhaps, the artificial manure of greatest consumption. It has happened, however, this year, that there has been a particular scarcity of the raw materials of this manufacture. Bones have been scarce—guanos of the phosphoric kind, which at other times have been most valuable to the superphosphate maker, have all but disappeared; whilst the supplies of coprolites, which form really our natural source of phosphoric acid, have so fallen off, that the price has been more than doubled; and they were at one time, from their excessive cost, almost out of reach of the manure maker. The dealer in superphosphate of lime, as well as the guano merchant, has found himself this year in the enviable or unenviable position, as it may be, of refusing orders. Here, then, was a glorious opportunity of making money, which the dealers in adulterated manure were not likely to let slip; farmers rushing to the manure market, only too glad to get supplies of manure, and not overcareful, if the truth must be told, to ascertain whether it was good or bad; and, on the other hand, the nefarious dealer, not even driven to the necessity of adopting a low price to obtain a sale, but revelling in a ready market and enormous profits. In this way many a ton of good guano becomes three or four tons of a manufactured article. For my own part, I feel convinced that the demand for artificial manures is increasing, and will increase to an enormous extent. No one can well form a notion from the demand this year, what it may next year become; but it is precisely this circumstance, coupled with the fact that the supply does not increase in anything like proportion with the growing requirements of agriculture, that should point out to us the necessity

of looking very closely after those who are entrusted with the trade in manures.

As I have before had occasion to shew, the dealer in manufactured guano has three or four principal raw articles, to one or more of which he has recourse in his trade. Undoubtedly his favorite ingredient is gypsum—more especially that which in several manufacturing processes is artificially produced—this variety of gypsum, being precipitated, is in fine powder, so that it mixes readily with the guano, and is not liable to the awkward exposure of its presence in the mixture by an accidental crystal, which has escaped the vigilance of the manufacturer. Besides, it does not want crushing, and herein is an extra economy. The chief recommendations of gypsum, however, are, first, that it *burns white;* secondly, that, even when quite dry, it contains water, which burns off at a red heat. Now, good guano burns white when heated sufficiently long; gypsum does not interfere with this whiteness. Good guano loses much weight by burning, and the farmer might perchance have resort to this mode of learning whether or not he has been honestly served; but as gypsum accommodates itself to these circumstances by losing weight when burned, the proof is not so easy.

These are quite sufficient reasons why the guano adulterator should cherish gypsum as his most useful and indispensable ally. Common salt is another resource of the refined adulterator; it also burns white, although it does not, as gypsum, lose weight on burning. Sulphate of soda has lately, I find, been used, and it has, unfortunately, some advantages to recommend it. Coprolites finely powdered form another source of adulteration, and inasmuch as they contain phosphate of lime, one of the natural ingredients of guano, the dishonest dealer may, perhaps, soothe his conscience by persuading himself that he is not, after all, doing his customers so great an injury as we would have them believe. I need not occupy the time of your readers by answering such sophistry as this (which I have, in fact, heard put forth on more than one occasion), for, independently of the fact that coprolites in their natural state are of no earthly use to vegetation, and that they can at all times be bought for less than 1–3rd of the cost of guano, I say, quite apart from these circumstances, adulteration is adulteration all the world over, and nothing can make those who practise it other than dishonest and disreputable persons.

As matters stand now, coprolites are not likely to be used largely in adulterating guano, on account of their price; neither are they so much esteemed in the adulterating trade, because they burn red, and their presence is more likely to be discovered.

But if the dealer is of an ambitious go-a-head character—if, scouting the refinements of his competitors, and relying, as he frequently may do with perfect safety, on the blind confidence and penny-wise policy of the farmer, he is determined to make the largest amount of profit in the shortest time, he straightway betakes himself to the nearest sand or loam pit.

Sand, I believe, unless very fine, is not well suited to the adulterating

dealer's purpose; he prefers a clay, provided that it be so mixed with sand that it will dry and work well. Your readers will remember that two years ago an organised factory, with drying-stoves, reverberatory furnaces, and grinding mills, and all the requisites of a large manufacture, was in full work at, or near, Bow Common, for the sole purpose of preparing clay or loam for the use of the guano manufacturer. Here you might have bought every variety of dry loam, with shades of color to suit the adulterator and the market, whose tastes he had to study. Has this manufacture been discontinued? Have we any reason to suppose that what was profitable then is less so now? I cannot say; but this I can speak with certainty about, that large quantities of guano are still adulterated with loam, wherever it may come from.

I do not propose to discuss the question of the loss which the agriculturist individually, and the community at large, suffer by these frauds. If this truth is not present to every one's mind without explanation, I feel that I do not possess powers of argument to make it so. I will, therefore, simply give you a few results of the different forms of adulteration that have come before me this Spring. I shall, however, first of all give the analyses of some half dozen good specimens examined this year, to shew that the average composition of Peruvian guano has suffered no alteration, and that no diminution of value has, in the last few years, occurred with this valuable manure.

The following are the analyses of six samples of guano received from different parts of the country and taken at random from my laboratory-book.

ANALYSIS OF PURE PERUVIAN GUANO, EXAMINED IN THE SPRING OF 1854.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Moisture . . .	12·02	14·70	15·37	16·75	22·26	8.29
Organic matter and salts of ammonia .	51.82	52·09	49·69	50·66	46·99	57·31
Sand, &c. . . .	2·45	1·19	1·25	2·29	1·28	1·12
Phosphate of lime and magnesia . .	27·69	21·72	22·38	18·50	21·52	21·93
*Alkaline salts .	6·02	10·30	11·31	11·80	7·95	11·35
	100·00	100·00	100·00	100·00	100·00	10·000
*These alkaline salts contain phosphoric acid, making the total phosphate of lime . . .	33·21	31·39	30·00	26·11	27·21	33·28
Nitrogen . . .	13·93	14·61	14·34	13·68	12·69	14·17
Equal to Ammonia . .	16·91	17·74	17.41	16·61	15·41	17·20

The sample No. 5, is certainly below the average composition of best guano, but this arises evidently from the dampness of the sample; it was obviously a cargo, or a portion of a cargo slightly injured by sea-water.

I am justified, therefore, by the result of recent experience in the composition of genuine Peruvian guano, in saying, as I did several years ago, that it *averages* fully 17 per cent. of ammonia. From this composition it has not varied in the course of several years—a circumstance which is of the utmost importance, as furnishing us with a standard of comparison wherewith to try sophisticated samples.

I will now give you the analyses of a few adulterated specimens in which the chief or only addition has been gypsum.

ANALYSES OF GUANO ADULTERATED WITH GYPSUM.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Moisture . . .	8·97	11·30	11·67	11·57	11·22	8·41
Organic matter and salts of ammonia .	29·49	30·65	23·66	18·65	22·36	18·58
Sand, &c. . .	1·97	2·00	2·01	2·97	1·64	1·65
Phosphate of lime and magnesia . .	19·51	26·48	23·02	18·09	24·24	14·20
Hydrated sulphate of lime (gypsum) .	35·81	20·91	31·97	38·42	37·19	53·53
Alkaline salts . .	4·25	8·66	7·67	7·02	3·35	3·63
Carbonate of lime .	...	...	...	3·28	...	...
	100·00	100·00	100·00	100·00	100·00	100·00
Nitrogen . . .	7·70	7·54	8·85	5·06	6·23	5·83
Equal to Ammonia . .	9·35	9·15	10·74	6·14	7·56	7·08

Here you will observe that the adulteration has ranged from 20 to more that 50 per cent. of gypsum; and it is difficult to say why the whole composition of the guano is not altered in precise relation to the adulteration—that is to say, if 50 per cent. of gypsum be added, the ordinary substances—phosphates, ammonia, &c., should be present in just half the usual proportion. This is not exactly the case, although there is an obvious relation between the ammonia and the gypsum—the one going down as the other increases in per-centage. I am inclined to think that, in many cases, the adulterators get hold of damaged guano, which would account partly for the discrepancy; or that they, perhaps, add a portion of a naturally inferior guano, not of the Peruvian type. But the facts, as they stand, are near enough for all practical purposes; these analyses indicate a value in the case of Nos. 4, 5, and 6, of about one-half, and in the other samples of six-tenths of real Peruvian guano.

The following are analyses of adulterated guano, in which sand and clay have been employed in addition to sulphate of lime:—

SAMPLES ADULTERATED WITH GYPSUM AND SAND OR CLAY.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Moisture . .	5·50	9·12	8·53	19·45	7.45	9·93
Organic matter and salts of ammonia .	16·42	14·19	18·89	7·10	8·62	20·87
Sand and clay, &c. .	27·80	48·87	26·80	18·67	35·53	13·89
Phosphates of lime and magnesia .	12·24	16·10	16·19	24·17	6·04	19·68
Hydrated sulphate of lime (gypsum) .	11·86	7·97	25·00	20·66	29·12	29·08
Alkaline salts &c. .	11·29	3·75	4·59	9·95	13·24	6·55
Carbonate of lime .	14·89	...	...	...	...	...
	100·00	100·00	100·00	100·00	100·00	100·00
Nitrogen . . .	2·91	3·14	4·48	1·82	1·11	4·83
Equal to . . Ammonia . .	3·53	3·81	5·44	2·21	1·34	5·68

You will see by this table that you are far better off with the gentlemen who only use gypsum, than when you come to those who use clay or sand as well; for the character of the manufactured article in these last instances is infamous. The sample, No. 5, is not worth, at the highest estimate, 2*l.* a ton; whilst no doubt, if the truth could be got at, it was sold *a bargain* at 11*l.* per ton.

I could add very many to the number of these analyses, but your readers will be already sufficiently disgusted with the detail. I shall simply state that although for obvious reasons I give no names or particulars in respect to these analyses, they could, in each case, be authenticated if necessary. The samples have come to me for analysis in the ordinary way of business, and without any seeking or solicitation on my part.

If you should be able to afford me the space, I will, in a future number of your Journal, point out how far the agricultural public possesses a remedy for this most crying evil.

Agricultural Gazette.

PRESERVATION *of* POTATOES *from* DISEASES.

MR. T. J. HERAPATH has recently addressed to the editor of the "*Bristol Mercury*" a communication, giving the results of trials made of his plan of preserving potatoes, by various persons.

The following is the summary of these results:—

1. Twelve sackfuls of potatoes, lifted October 25th ult., were stored with lime—the lime being placed "*in small bundles in the*

middle of each sack." Tubers "*all preserved;*" whereas "*some of the same potatoes stored without lime were much affected.*"

2. Fifty bushels of potatoes, dug up towards the end of October, were pitted with three bushels of quick lime; the latter being placed at the bottom of the pit, and covered over with a thick layer of gorse. On opening the pit, it was found that the diseased tubers "*did not amount to more than two dozen.*" Tubers stored in the ordinary way, in pits without lime, were "*almost entirely destroyed by disease.*"
3, 4, 5. Potatoes stored with and without lime, "*quite untouched by the disease.*"
6. Roots stored in a large chest or box with lime; the latter being placed in a small clothes-basket, and covered over with fagots. On opening the box, the tubers "*were found quite healthy;*" whilst some that had been kept in a cellar, without lime, "*were much* diseased."
7. Twenty bushels of the tubers were placed in a large bin, with three bushels of lime; the latter forming a stratum at the bottom, and covered over with a thick layer of coarse cinders. At the end of three months, "*the roots were found to be quite sound; whilst another lot which had been put into another bin, without lime, were very much diseased.*"
8. Potatoes housed in sacks, one or two large lumps of lime being put into each sack, "*tubers quite dry and all preserved.*"
9. Potatoes first dried by exposure, on a gravel-walk, to the heat of the sun, and then stored away in large boxes, with lime; "*all healthy.*" Tubers housed in their natural condition "*became diseased.*"
10. Potatoes housed with lime, all healthy.
11. Same result.
12. Several bushels of potatoes were pitted with lime, and when examined, at the expiration of several months, were found "*to remain untouched by the disease.*" Roots pitted without lime "*became quite rotten.*"
13. About ninety or a hundred sackfuls of potatoes were divided into four lots, and pitted in the following manner:—The first lot was stored in the ordinary way; the second, third, and fourth lots, with lime. In the second lot, the lime was placed at the bottom of the pit, and covered over with a thick layer of coal-ashes; in the third, it was placed in a heap, nearly in the centre of the pit; and in the fourth, it was thrown in on top of the tubers, being prevented from coming in contact with the roots by the intervention of a thick layer of brushwood and coarse cinders; between three and four bushels of lime being used to each lot. On examining the tubers, at the end of some months, those contained in the first pit were found to be much diseased—"*perhaps about a quarter of their number were, more or less, affected; whereas the roots in the other pits were also entirely preserved.*" * * "*The best plan of using the*

lime, therefore, seems to be to place it on top of the tubers, in the way described, or perhaps it would be better to put some at the top, and some at the bottom of the pit."

A description of this process will be found on page 15 of this volume.

LECTURES *on* PEAT CHARCOAL. By PROFESSOR WAY.

PROFESSOR WAY remarked that, independently of the noxious gases resulting from the putrefaction of animal matter generally, and which consisted principally of sulphuretted hydrogen and sulphuret of ammonia, each particular animal substance, excretionary or otherwise, had its *peculiar* odor, which, although abundantly perceptible by the senses, and, in many cases, as in musk, almost inexhaustible, was inappreciable in weight; therefore, by deodorising a large amount of odor, it was not to be inferred that a large amount of manuring matter was thereby secured. He then enumerated the various single and double deodorisers that had been employed.

He referred to Sir William Burnett's excellent application of chloride of zinc, and to the ordinary chloride of lime; to gypsum (sulphate of lime), and its conversion in ammoniacal atmospheres into sulphate of ammonia and carbonate of lime; to the agreeable odor of pure ammonia, and its power of giving intensity to odors of a disagreeable character, which intensity was lost when the ammonia was withdrawn; to sulphate of iron (green copperas), which when powdered and thrown into tanks turned black, on account of the sulphuret of iron formed on the decomposition of the sulphuretted hydrogen present.

He then proceeded to the consideration of charcoal as a deodoriser. He gave an interesting statement of the peculiar action of charcoals in general, arising, he believed, from the great amount of surface their spherical interstices presented, and of the particular action and superior value of animal charcoal over all others. He referred to the theory he had been led to form of this peculiar difference, and to a very successful imitation of animal charcoal, which he and Mr. Paine had made, in reference both to deodorising and decolorising properties, from the light porous silica-rock, found on Mr. Paine's estate in Surrey, which when broken up and steeped in heated tar, was put into a gas-retort, where the tar was burnt off in the state of very pure gas, and a residuum left of the new silicated charcoal in question.

He explained that in charcoals it was not the amount of carbon they contained that constituted their value, but the mode in which the carbon was distributed; that animal charcoal contained only 10 per cent. of real carbon, while wood charcoal contained 90 per cent. He referred to the large amount of water, 50 or 60 per cent., which peat charcoal took up, and to the fallacious dry state of the manures with which this water-carrier was mixed. He feared this mode of introducing water in a latent state into manures, in many cases, gave a turn of the scale more in favour of the manufacturer than of the

farmer. He doubted whether peat-charcoal could be used economically for the purpose of soaking up tank-water; if not, he feared it would prove of no advantage, in other respects, as a remunerative agent to the farmer. It had been long before the public, but had not progressed in market value, as it would have done had its application been successful. He considered it to lead to much error in practice, that the exact nature of the action of charcoal on ammonia was not better understood by the public. Fresh-burnt charcoal would absorb a large quantity of ammoniacal gas, but it was a mistake to suppose that it would consequently abstract ammonia from a liquid impregnated with it; on the contrary, water had the power of displacing from charcoal the whole of the ammonia it had received in a gaseous state within its pores. Peat charcoal did not either make manure or separate it from sewage; it simply rendered manure portable. He exhibited a striking experiment, showing the power of *dry* peat charcoal to arrest odors. Two open tumblers were half-filled with the most offensive sewage-matter Professor Way could obtain, and the surface of each mass covered with a film of thin paper and a thin bed of powdered peat charcoal resting upon it. These tumblers were in this state handed round to the members, who ascertained the perfect manner in which the sewage-matter was thus rendered no longer offensive to the smell. He then gave an interesting account of the process of Mr. Stothert, by which sewage-matter was reduced, by a double action of purification, into clear water and inodorous precipitate—a process admirable adapted for sanitary purposes, although not for those of agriculture, as the more valuable manuring matters were held in solution and carried off in the pellucid liquid, while the precipitate was comparatively an inert mass.

On a Manure *prepared with* Fish, Dried *and* Pulverised.

By M. de Molon.

I have for many years past employed fish as manure. The excellent results which I obtained by its use, led me to the idea of reducing it to the state of a dry powder, in order to preserve all its richness under the smallest possible bulk, and to render its transportation and preservation easy.

After numerous experiments, the means which I now employ consist:—1, In disaggregating the fish in double boilers, without the addition of water, and solely by the action of steam at a pressure of several atmospheres introduced between the sides of the boiler; 2, in subjecting the fish, after leaving the boiler, to pressure, for the purpose of removing as much as possible of the oil and liquid; 3, in dividing, by rasping the cakes formed by the pressing; 4, in spreading the fish thus divided, on frames covered with canvas, and than submitting it, in appropriate drying-rooms, to a current of hot air, which speedily completes its dessiccation; 5, finally, in grinding the dried fish as it leaves the drying-room, so as to completely pulverise it.

In this state, it may be immediately sold for agricultural purposes, and it may be preserved for any length of time.

This manure has been analysed at the *Conservatoire des Arts et Métiers*; the author makes known the results obtained, which assign to this powder the character of a good manure.

"Experience, moreover," says M. Molon, "has confirmed the anticipations, and its considerable quantities, which I have already sold to agriculturists during the last two years, have uniformly given results superior to those obtained with Peruvian guano."

The extraction of the fish-oil diminishes its cost, and allows of its being sold to manufacturers at a moderate price.

Comptes Rendus, No. 23, June 5, 1854.

FORENSIC CHEMISTRY.

Detection *of* Blood Stains *on* Garments.

Dr. Alfred Taylor has informed the writer, that he has employed pure glycerine in medico-legal inquiries as a means of separating blood corpuscules, and has found it serviceable in that respect from its not drying: a property of that fluid of considerable practical importance in the examination of blood globules under the microscope, or by the help of a lens, as the globules are in nowise altered in size or form thereby.—*British and Foreign Medico-Chirurgical Review.*

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

On the Changes *produced in the* Blood *by the administration of* Cod-liver Oil, *and* Cocoa-nut Oil.* By Theophilus Thompson, M.D., F.R.S.

The author has found, that during the administration of cod-liver oil to pthisical patients their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. F. Simon to the same effect. The use of almond-oil and of olive-oil was not followed by any remedial effect; but from cocoa-nut oil results were obtained almost as decided as from the oil of the liver of the cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleïne, obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the Copperah or dried cocoa-nut kernel, and refined by being treated with an alkali, and then repeatedly washed with distilled water. It burns with a faint blue flame, shewing a comparatively small proportion of carbon, and is undrying. The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed, and divided into two portions. One portion was weighed, and then dried in a water-oven, to determine the water. The other was macerated in cold water until it became colorless, then moderately dried, and digested with ether and alcohol, to remove fat; and, finally, dried completely, and weighed as fibrin. From the respective weights of the fibrin, and the dry clot,

* The substance of a Paper read to the Royal Society.

that of the corpuscles was calculated. The following were the results observed in seven different individuals affected with phthisis in different stages of advancement:—

		Red corpusules.	Fibrin.
First stage, before the use of cod-liver oil . . .	Female	129·26	4·52
	Male	116·53	13·57
First stage, after the use of cod-liver oil . . .	Female	136·47	5·00
	Male	141·53	4·70
Third stage, after the use of cod-liver oil . . .	Male	138·74	2·23
Third stage, after the use of cocoa-nut oil . . .	Male	139·95	2·31
	Male	144·94	4·61

PHOTOGRAPHY.

On a Silver Bath, *with which* Collodion Proofs *may be developed by* Gallic Acid. By the Abbé Laborde.

(From the "Cosmos," March 31, 1854*.)

Much is said about a silver bath, the composition of which is kept secret, which has the property of allowing the image to be developed instantaneously under the influence of *gallic acid*.

I remembered, in thinking of this, some old experiments in which I obtained similar results on paper, by mixing nitrate of lead with the silver bath in the gallic acid, or nitrate of potass with the iodide.

I tried in vain to apply the effect of nitrate of lead to collodion; the image indeed appears quickly enough, but it is dull and without force.

I succeeded perfectly, on the contrary, by adding to the silver bath a less oxygenised salt of lead, the nitrite. The preparation of this exciting bath is very easy; it is merely requisite to allow the solution of nitrate of silver to digest for about 24 hours on the nitrite of lead. The proportion of the salt of silver is 1-10th. By double decomposition a little nitrite of silver is formed, which very probably plays an important part; but as the nitrite of silver is transformed, by slow degrees, into the nitrate, it is good to leave the undissolved nitrites of lead and silver at the bottom of the liquid.

Very nearly the same results are obtained by adding to the collodion the smallest proportion of *nitrite* of *potass* it will dissolve. This second process will perhaps be preferable to the first, for ordinary solution of silver may then be employed, and we thus avoid the changes which the bath containing nitrite of lead exhibits, which require a little watching.

I had already observed in albuminised glasses a rather curious effect, connected with the preceding facts; solution of silver is placed in a bottle, and the rutilant vapors evolved from a heated mixture of nitric acid and amydon, introduced; in this way we obtain a bath which

* As given in the "Journal of the Photographic Society."

gives the albumen coating great sensibility; but its efficiency disappears in the course of a few hours, because the nitrous or hyponitrous acid introduced into the liquid, gradually changes into nitric acid, injurious to the development of the image. I have thought it right to quote this observation, because it is so desirable to bring all materials towards establishing the exact and positive theory of the different photographic operations.

Whenever a substance has a tendency to oxidation, so as to form a compound not injurious, there are good reasons for trying the use of it in photography, as an auxiliary afforded to the luminous agent.

TOXICOLOGY.

On Pure Oxide *of* Carbon, *considered as a* Poison.

By M. Adrien Chenol.

The facts we are about to communicate relative to the poisonous action of pure oxide of carbon, lead to the following theory:—

As carbonic acid only plays the part of an *obturator*, and as, being a gas undecomposable at a low temperature, it is incapable of furnishing the oxygen necessary for the combustions which sustain life; it causes death by pure and simple asphyxia; it is quite otherwise with pure oxide of carbon.

This oxide of carbon, indeed, in contact with the marvellous organs of combustion with which we are endowed, speedily gives rise to the following effects:—It passes to the state of carbonic acid, whence result:—1. Substraction of oxygen and its consequences; 2. combustion of this oxygen and its consequences; 3. formation of carbonic acid and its consequences.

These three effects are inseparable, and the last occasions immediate asphyxia by arresting the action of the lungs and every act of motion. But at the same time, the oxygen has been condensed; whence has resulted an action of compression and of tearing by the vacuum produced; but, moreover, the conversion of oxide of carbon into carbonic acid has given rise to a disengagement of about 6000 calores per litre of oxygen. burned in the spongiosities of our organisation containing this oxygen, which should have served for enriching the blood.

These 6000 calores, developed in intimate contact and cellules of atoms or small spheres of oxygen, occasion, then, virtually and infallibly, a disorganisation by cauterisation, which causes that violent pain accompanying poisoning with *oxide of carbon*, which, differs in this respect from that produced by *carbonic acid*, which, as we have frequently experienced in the mines of Pont-Gebaud, causes an agreeable intoxication, passing by degrees to a sweet lethargy, rather than a painful sensation. Consequently, the study of oxide of carbon as a poison is a subject of much complexity, and of the highest interest in a toxicological point of view.

Facts in support of this Theory.—Contrary to what has been written, the oxide of iron of ordinary combustion is an insufficient reducer, and incapable of removing oxygen from metals of the class of iron. Thus, the theories based on this action are erroneous. This is not the occasion for a discussion of this order. We may here say that this oxide of carbon presents dangers as a poison.

But if the oxide of carbon of combustion, that is to say, that which contains 4. 5, and even 6 volumes of nitrogen, is not a powerful reducer, pure oxide of carbon is not only a reducer of the greatest energy, but a violent poison, even in very feeble dose.

My health has suffered seriously, in consequence of several poisonings by this gas, of which I have made very frequent use, on account of its energetic and peculiar properties as a reducer. I wish, therefore, to obviate, for those who shall follow up my works, the dangers attached to their practice, by pointing out to them this danger in an especial manner.

In 1846, I was at the Marquis of Sassenay's manufactory, at Stolberg, in Prussia, for the purpose of studying the ores of zinc, and their treatment in various points of view, particularly as regards improving the quality of very poor ores, by a system which I had invented, and which consisted in fusing these poor ores in a *cubilat*, closed at the upper part, and collecting the zinc at different heights in various states of oxidation. In the course of my experiments, I desired to make some investigations concerning the oxide of carbon resulting from the reaction of the oxides mixed with an excess of charcoal. With this view, having no instrument suitable for collecting this gas, I drew it by means of a pipette from the apparatus, and conveyed it under a bell-glass. While I was occupied with this operation, the engineer of the establishment spoke to me, and struck me on the shoulder, without attracting my attention; I then, as it appears, inhaled some of the gas contained in my mouth, and immediately fell on my back, as if stunned.

The following were the external and internal effects of this sudden annihilation of all external faculties described, according to a *procès-verbal*, drawn up by the director, the engineer, and myself. After I had somewhat recovered; and this extract is perfectly in accordance with another accident, of which I will speak presently, and which occurred to me two months ago.

Externally:—1, I fell as if struck by lightning; 2, the eyes were turned back in their orbits; 3, the limbs were contracted; 4, the skin was discolored; 5, the veins were swelled, and presented a black tint under the skin. *Internally*:—1, The sensibility was extreme; life was, so to speak, exalted; all the ideas and principal matters of interest, and all the predominent affections were reproduced to the mind, as in an instantaneous mirror. 2. Violent pains, as if of tearing, were experienced in the thorax. This inward pain was most vivid; the brain felt powerfully compressed, either as a principal action, or as a nervous action, induced by the pain.

Being in this position, I was carried into the air without knowing

it, nor did I feel the lotions of water and of vinegar, or the inhalations of ammonia, &c. After a quarter of an hour, external sensation slowly and progressively returned, accompanied by the internal pains already mentioned; but then changing into a sensation of suffocation, attended with cold, and with drops of perspiration all over the body, but especially on the head.

For several days the lassitude was general and continuous, and digestion very bad. Moreover, there was a general disgust for everything. Sleep, from being light, became obstinate and heavy; it was frequently disturbed by cramps in the knees and toes.

For several months after, these effects, although they diminished very much, maintained a marked influence on my health. I was inclined to sadness, faintness, and disgust:—I was alarmed at the noise of any unexpected shock; it produced a nervous shock analogous to an electrical discharge. This state gradually became modified, passing to that of a kind of insensibility which was fixed more particularly at the ends of my fingers, in degrees of intensity varying with the state of the atmosphere. To sum up, it is evident that poisoning with pure oxide of carbon is most terrible in itself, and involves a series of disorganisation.

I am now under the influence of the second poisoning to which I have alluded, and which happened through the sudden breaking of a manometer tube, against which I knocked my head in a narrow place. All the internal symptoms were exactly the same, but I was not knocked down. I remained in a state of half consciousness and capable of taking care of myself, according to what I hoped to do, either for others or for myself.

Feeling convinced that the internal derangement resulted from a lesion produced either by the effect which I have just mentioned as regards the theory of the poisonous action of oxide of carbon, I drank for several days, and in as large quantity as I could, gum water and mallowa water. I expected to attain my object as soon as possible. Nevertheless, the disgust, feebleness, and insurmountable inertness continually prevailed; the insensibility of the ends of the fingers became extreme; and, by a singular contrariety, not only did shocks cause me to tremble electrically, but a drop of water which fell on my skin, or the slightest touch, even my own, produced a sense of irritation. Bathing seemed to do me much good; for several hours after, the *calm* was much ameliorated; but some times in the baths, I experienced a kind of general shuddering.

I have addressed these details to the Academy for the sake of general utility, and in order that the medical profession may study the dangers of so terrible a poison, and the curative means to be employed.

Comptes Rendus, No. 16, April 17, 1854.

BIBLIOGRAPHY.

THE "DRUGGISTS' GENERAL RECEIPT BOOK," COMPRISING A COMPLETE VETERINARY FORMULARY; NUMEROUS RECIPES IN PATENT AND PROPRIETARY MEDICINES; DRUGGISTS' NOSTRUMS, ETC; PERFUMERY AND COSMETICS; BEVERAGES, DIETETIC ARTICLES AND CONDIMENTS; TRADE CHEMICALS, ETC. WITH AN APPENDIX OF USEFUL TABLES. By HENRY BEASLEY. Third Edition. London: Churchill, 1854.

THE former editions of this work have been for many years before the public, and the present can only add to the reputation which the others have earned and so long maintained. The book amply realises the expectations formed from the above copious title. Its utility to the Pharmaceutical Chemist and the general reader is so manifest as to require no demonstration.

In the Preface, Mr. Beasley informs us that the whole work has undergone careful revision, and that "upwards of two hundred new forms, receipts, and processes, collected from the best authorities," have been added.

NOTES AND QUERIES.

TO THE EDITORS OF "THE CHEMIST."

GENTLEMEN,—In the No. of "THE CHEMIST" for the present month, you have an article, extracted from the "*Journal de Pharmacie et de Chimie*," upon the preparation and uses of glycerine. In reference to the process therein ascribed for the purification of this substance, permit me to state, as the result of long experience, that the difficulty of getting rid of the butyric and other fatty acids *by boiling* is very great. The heat required is sufficient to decompose the glycerine, and the extremest care will be found requisite to prevent it from becoming tainted with an empyreumatic odor. This difficulty may be avoided by adding a sufficient quantity of alcohol to the concentrated glycerine in the presence of sulphuric acid, and thus converting the fatty acids into their corresponding ethers, which latter may be distilled off at a moderate heat.

The final purification will be accomplished according to the fancy of the operator; but I venture to draw your attention to the above modification, in the hope that it may preserve some experimenters from the disappointment which would almost certainly ensue, from following the *modus operandi* described by your French correspondent.

I am, Gentlemen, yours &c.,

300, Holborn. GEORGE STACEY..

CENTIGRADE TESTING.

To the Editors of "THE CHEMIST."

GENTLEMEN,—In the last Number of "THE CHEMIST," you have inserted an attack made on me by Mr. Griffin; I now claim a space in your columns for my reply.

In order to convey a clear idea of this matter, I must state that about six years ago I entered Mr. Griffin's employ, and a part of my duty was the graduation of apparatus for Centigrade Testing. At that time I found the process of calibration of the crudest description, nearly all the standard instruments in use inaccurate, and the process for the production of the necessary articles exceedingly tedious

and liable to error. I immediately took up the subject, worked early and late, spending my hard earned funds in perfecting the machinery, (without a helping hand from my employer, but with occasional jeers at my first failures), until I had perfected all the standards that required it, and had completed the Dividing Engine, which gained for the instruments divided by it a European fame. During the two years these experiments were in progress, I turned my attention to the process of Centigrade Testing, and finding the difficulties of acquiring a clear comprehension of the subject very great, I made frequent applications to Mr. Griffin to simplify his process, or allow me to do so, in order that our manufacturers might derive advantage from his system. This he would not do, much to my annoyance, as I felt how impossible it was to induce the workmen in a laboratory or manufactory to understand or use a system of testing, which required an extensive series of tables to be referred to, and frequent calculations to be made; and I much wished the subject could be brought out in a simpler form, so as to be easily understood.

In introducing the process which I have given to the public, and which the "*Annales de Chêmie*" has translated and published in the last Number, and for which the Academy of Sciences has voted me a sum of money, I did not certainly deem it necessary to mark every line or paragraph as original that was so, but I stood on the broad basis of the fact, that my communication contained a sufficiency of new matter to warrant me in bringing the subject before the public; and, lest there should be any doubt as to what part is my own, I may state that I claim the method of ascertaining the quantity of an impure or commercial oxalic acid necessary to form a decem of test acid of the proper strength. I also claim as original the plan of ascertaining the per-centages of potassa, and of soda and their carbonates, *in a solution*. I claim the plan of obtaining the per-centages of sulphuric acid, of muriatic acid, of nitric acid, of acetic acid, and of ammonia, by a direct centrigrade process, *without the use of tables or calculations*. I claim the invention of a machine for dividing, with mathematical exactness, the chlorimeter I have recommended for use in testing bleaching powder.

Mr. Griffin states that this is a copy of Gay Lussac's burette; this I admit, but Mr. Griffin forgets to mention that he placed one of those burettes, made by Collardiere, of Paris, in my hands, for the purpose of its being copied, and that I tried every means I could then devise, or he suggest, without success; but since leaving his employ I have succeeded in accomplishing this object; and I think I am entitled to claim whatever merit may be due as being the first in England who has accomplished the graduation of a piece of apparatus so irregular in the divisions.

My reason for altering the names of Mr. Griffin's division of the gallon was, that I considered a decimal division required decimal names. The word "Septem," conveys no idea of its proportion to the "UNIT" or gallon, whereas the word "decimillem" furnishes the required information.

My modification of his plan for making test solutions was introduced for the purpose of throwing overboard two useless terms,—the "TEST ATOM" and the "DEGREE." The unnecessary use of new terms being the reason that few men ever studied, and none of our writers noticed, Mr. Griffin's plan of operating.

Mr. Griffin's assertion that the pictures of the apparatus you have published are copies of his woodcuts, is false, as any one may see in a moment by comparison. There does not exist that amount of similarity which may be supposed to occur between two drawings, by different artists, of the same pieces of apparatus. Those I have used were drawn from the articles by Mr. Williams, of 144, Fleet Street.

In closing this communication, I assert that Mr. Griffin has not published or ever invented a process for obtaining the per-centages of acids and of alkaline solutions without the use of tables, and that he knew nothing of the process until the publication of my paper is evident; for the last edition of his "*Chemical Recreations*," dated 1854, and written eighteen months after I left him, although containing a chapter on "Centigrade Testing," does not contain a single line that can be construed to indicate his knowledge of the process I have introduced.

I am, Gentlemen,

Your obedient Servant,

123, *Newgate Street.* WM. ACKLAND.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

New Test *for* Sugar *in* Diabetes. By John Horsley.

If a freely alkaline solution of chromate of potassa be mixed with urine suspected to contain sugar, and boiled, the liquor will assume a deep sap green color, arising from the decomposition of the chromic acid, the reduced oxide of chromium being held in suspension by the potassa.

Such is the sensitiveness of this test that five or six drops only of saccharine urine diffused through water is sufficient to shew the effect, which is infinitely more striking than even Moore's potassa or Trommer's Test.

I would therefore recommend a mixture, in equal parts, of a solution of the neutral chromate of potassa and liquor potassæ, to be kept in the Chemical Cabinet of every medical practitioner, labelled "Test for Sugar."

The following two experiments beautifully illustrate the value of this process in the detection of sugar under any circumstances.

First Experiment.—Take a small test tube, and having put into it ten or twelve drops of simple syrup (cane sugar) and diluted it with water, add a few drops of the test mentioned, and apply the heat of a spirit lamp. No effect will be produced.

Second Experiment.—Take another test tube, and having put the same quantity of simple syrup diluted with water, add two or three drops of acid, sulph. dil. and boil for a few minutes; this will convert the cane into grape sugar. If we now add a few drops of the test and apply heat, the effect becomes striking, developed in the change of the color of the liquid to an intense green.

When the quantity of sugar is very small, a piece of white paper at the back of the test tube will shew the color more distinctly.

CHEMICAL TABLES.

Compiled by Thornton J. Herapath.

No. II.

Thermometrical Equivalents *and* Effects *of* Temperature.

(Continued from page 590.)

158° F.; 70° C. *Liq. ammoniæ* 0·96 or 8·3 boils.
158·5° F. Nitrate of methyl boils.

160° F. Mercury and iron inflame in chlorine, 100° to 160°.
Chloride of nitrogen may be distilled without exploding.
Temp. at which pigs are scalded.
Blood coagulates when dropped into water.
Hatchettine fuses.
Aldehyde-ammonia, oxide of benzule, and benstearic acid fuse.
Thorinum, glucinum, and yttrium take fire in oxygen and chlorine at about this temp.
Colophony or rosin becomes soft.

162° F. Starch granules swell in water at this temp., and "*boiled starch*" is formed.
Borate of methyl-oxide boils.
Iodide of æthyl boils, 161° to 162°.
Helenine and benzoyle fuse.

163° F. Chloronaphthalin fuses.

164° F. Dammara resin fuses.

165° F. Stearic acid and cedar-camphor fuse.
Acetic æther and chloride of carbon boil.

167°F; 75° C. Camphrone, 3-chloride of acetyle, and acetal (Liebig) boil.

168° F. Brewing or mashing should be effected at about this temp.

168·5° F. Absolute alcohol boils.—*Ure.*

170° F. Mineral tallow fuses.—*Conybeare.*
Margarone fuses, 170° to 170·5°.

172° F. Mechanical equivalent of heat = 172° = 1 lb. raised one foot in a second.—*Joule.* Alcohol, 0·8 boils.—*Henry.*

173° F. *Liq. ammoniæ* of 0.97 or 6·2 ⅌ boils.

173·5° F. Anhydrous alcohol boils (*Most Authors*).
Athamantine fuses.

174° F. 3-chloride of phosphorus boils.—*Andrews.*
Octohedral borax is formed in a solution of 1·26 between 174° and 145°.

175° F. Alcohol of 0·825, *i. e.* common spirits of wine boils. [Lat. Heat = 457°.—*Ure.*]

176° F.; 80° C. Naphthalin and campholic acid fuse.
Acetal boils.—*Gluckelberger.*

177° F. Cerotine fuses.

178·6° F. Proof spirit boils.—*Ure.*

179·75° F. Spirit of 10 U.P. boils.—*Ure.*

180° F. Carnauba wax, Chinese wax, and cerosine fuse.
Cyanide of æthyle boils.

180·4° F. Spirit of 20 U.P. boils.—*Ure.*

182° F. Spirit of 30 U.P. boils.—*Ure.*

183° F. Propione and metacetone boil.

183·4° F. Spirit of 40 U.P. boils.—*Ure.*

185° F.; 85° C. Nitrate of æthyle boils.

185·6° F. Spirit of 50 U.P. boils.

186° F. Naphtha ($C^{14}H^{12}$) boils—186° to 194°.

187° F. Water boils on the summit of Mont Blanc.
Liq. Ammoniæ of 0·98 or 4·1 % boils.
Benzole boils. Idrialine fuses.
188° F. Lactone boils.
189° F. Spirit of 60 U.P. boils.
190° F. Sodium and melissic acid fuse.
191·8° F. Spirit of 70 U.P. boils.
192° F. Erucic acid fuses.
193° F. Stearoptene of oil of turps. fuses.
194° F.; 90° C. Atropine fuses, 190° to 104°.
3-basic citrate of methyle boils.
F.195° Magnas resin (fused) solidifies.
Meconine fuses.
196° F. *Liq. ammoniæ* 0·99 or 2 % boils.
Myrrhine fuses.
196·4° F. Spirit 80 U.P. boils.
197·5° F. Scheererite boils.—*Macaire Princeps.*
198° F. Narceia fuses.
199·5° F. Amylamine boils.
200° F. 5-chloride of phosphorus sub.
Oxybromide of phosphorus boils.
The so-called nitro-sulphuric acid dissolves silver below 200, but scarcely acts upon copper, lead, or tin.
A medium heat, *Dub. Phar.* 100° to 200°.
201° F. Rose's fusible alloy melts.
201·5° F. Chloral and oxichloro-carbonic æther boil.
202° F. Spirit, 90 U.P. boils.
203′ F; 95° C. Butyral boils.—*Chancel.*
Acetal boils.—*Döbereiner.*
205° F. Many natural silicates are decomposed by HS air, and steam between 212° and 205°.
Citraconanile fuses.
206° F. Brazilian wax and protobromide of antimony fuse.
207° F. Water boils (bar. = 27 in.).
207·9 F. Water boils (bar. = 27·5 in.).
Chlorobenzoïc acid fuses.
208·7 F. Water boils (bar. = 28·3 in.).
209·5 F. Chryssamic æther fuses.
209·6 F. Water boils (bar. = 28·5 in.).
210° F. Monohyd. formic acid boils.
210·4 F. Water boils (bar. = 29 in.).
211·2 F. Anilo-cyanic acid fuses.
Water boils (bar. = 29·5 in.).
212° F.; 100° C.; 80° R. Water boils (bar. = 30 in. or 1 atm). Superior heat, *Dub. Phar.*, 200° to 212°.
Temp. of the water of the Geysers (Iceland).
Zinc becomes very ductile and malleable between 212° and 300°.
Best temp. for making boiled starch, arrow-root, &c.

Best temp. for tempering hard steel for the knife-edges of pendula and chemical balances; also for razors.

Paste varnish becomes softened.

Pitch fuses. Selenium conducts electricity, and becomes semifluid.

Glucose or grape-sugar, pyrotartaric acid, crys. oxalic acid, and potassiamide fuse.

Dentist's copper amalgam swells up and becomes soft.

Albumen, in a very dilute condition, requires a temp. of 212° for coagulation.

The vitality of the spores of *Oïdium aurantiacum* is not destroyed by a heat of 212°.

Blue hydrated oxides of copper and of cobalt become respectively black and red when boiled in alkali.

Zinc becomes ductile and malleable; 212° to 300°.

212°·8′ F. Water boils (Bar. = 30·5 inches).

213° F. Chl. of zinc fuses and conducts electricity.
Liq. potassæ and soda-ley of 1·06 or 4·7% boil.

213°·5 F. A sat. solu. of sulphate of soda boils.

213°·6 F. Water boils (Bar. = about 31 in.).

214° F. *Liq. potassæ* of 1·11 or 9·5% boils.

215° F. Butyrate of methyle and chloride of amyl boil.
Deutoxide of phosphorus fuses.
Liq. potassæ of 1·15 or 13% boils.

217° F. Soda-ley of 1·18 or 13% boils.
Vitriol of 1·1 or 10% boils.

217°·4 F. *Liq. potassæ* of 1·19 or 16·2% boils.

218° F. Solu. of oxalate of ammonia boils.

219° F. Phosphorus distils.—*Pelletier*.
Acetal boils; 219° to 223°.

220° F. Potash-ley of 1·23 or 19·5%, soda-ley of 1·23 or 16%, and sat. solu. of chloride barium, alum, chlorate potash, carb. soda, acetate potash, and sulp. zinc boil.
Iodine fuses, 220° to 224°, and then conducts electricity.

221° F.; 105° C. Bihyd. formic acid boils.

222°·5 F. Maynas resin fuses.

222°·8 F. Piperidine distils.

224° F. Potash-ley of 1·28 or 23·4% and soda-ley of 1·29 or 19% boils; also a sat. solu. of 3—basic phosphate soda (*Legrand*) and solus. of common salt of 30% and of nitrate strontia of 53%.

224°·6′ F Lactide fuses. Vitriol of 1·2 or 20% boils.

226° F. Sulphovinate of ammonia is decomp. Valyl boils.
Toluol boils; 226° to 230°.

227° F. Benzoate of methyl boils.
Nitrate of ammonia and benzidine fuse.

228° F. Sulphur fuses 228° to 233° [L.H. = 145°.—*Irving*].
Soda-ley of 1·32 or 23% boils; also oxichl. of kakodyle.

Phloridzine fuses.

229° F. Potash-ley of 1·33 or 26·3§, and a sat. solu. of common salt boil.

230° F.; 110° C. Gutta-percha undergoes a sort of doughy fusion between 212° and 230°.

Butyric æther, oil of lemons, valeral, valerone, and amyle-oxide (*Balard*) boil.

Nitraniline, biniodide of phosphorus, and hydrobenzide fuse.

232° F. Oxychloride of phosphorus boils; also, bisulp. methyle, 232° to 234°.

233°·5 F. Oil of sassafras begins to boil.

234° F. Water boils (1½ atmos.) *Dulong and Arago*; also potash-ley of 1·36 or 29·4§, and bichloride of tin (*Andrews*).

Chloralide fuses; 234° to 237°.

235° F. Soda-ley of 1·36 or 26§ boils.

235°·5 F. Camphor of oil of turps. fuses.

238° F. Copperas fuses.

Solu. of nitre of 74%, and sat. solus. of sal-ammoniac, and tartrate of potash boil.

239° F.; 115° C. Napthene and vitriol of 1·3 or 30§ boil.

240° F. Peroxide of silver evolves oxygen with a kind of deflagration.

Cholestrine (fused) solidif.

Chloride of camphine, pyrogallic acid, benzamide, and valeranilide fuse and boil.

Oil of gurjun, *liquor potassæ* of 1·39 or 32·4§, and sat. solus. of nitre and Rochelle salt boil.

242° F. Soda-ley of 1·40 or 29§ boils.

244° F. Chl. strontium, sat. solu., boils.

244°·5 F. Sulphochloride of phosphorus (PS^4Cl^3) boils.

245° F. Crystallised olivile fuses.

246° F. *Liq. potassæ* of 1·42 or 34·4§ boils; also 3-basic boracic æther, and chlorochromic acid.

246°·5 F. Nitrate of soda, sol. of 60§ boils.

248° F.; 120° C. Alum kept for about 12 hours at from 248° to 320° is changed into a porous mass (*Roche alum*).

1-hyd. acetic acid, iodide of amyl, oxide of allyle, caproic æther, bromide of carbon, soda-ley of 1·44 or 31§, and a sat. sol. of iodide of potassium boil; also nitric acid of 1·42 (*Graham*).

Salicine, formiate of ammonia, pure coumarine, and caoutchouc or India-rubber fuse.

Benzoïc acid fuses; 248° to 250°.

249°·8 F. Nitrate soda, sat. sol., boils.—*Legrand.*

250° F. Acetate of amyl-oxide boils; 250° to 257°.

Bichloride of tin and amylic mercaptan boil.

250°·5 F. Water boils (pressure = 2 atmos.).

253° F. Cinnamic acid, urea, and menispermine fuse.

254° F. Lichestearic acid fuses.
255° F. Soda-ley of 1·47 or 34⅜, and *liquor potassæ* of 1·44 or 36·8⅜, boils.
256° F. Citric acid fuses.
257° F.; 125° C. Selenium passes into the crystalline state most quickly; 257° to 356°.
Hydriodic acid, concen. solu., boils; 257° to 263°.
Amorp. amygdaline fuses; 257° to 266°.
Sebacic acid (*St. Evre*), fuses; valeronitril, and sulphochloride of phosphorus (PS^2Cl^2) boil.
258° F. Mesitic chloral boils.
259° F. Carbonic æther boils.
260° F. Pimaric acid fuses.
260°·6 F. Vitriol of 1·408 or 40⅜ boils.
261° F. Sebacic acid and nitrobenzoic acid fuse.
262° F. A sat. solu. of acetate soda boils.
263·9 F. Water boils (pressure = 2½ atmos.).
265° F. Soda-ley of 1·5 or 36·8⅝, and *liquor potassæ* of 1·47 or 39·6⅜ boil; also capnomor.
266° F.; 130° C. Uroxalic acid is converted into uroxile. Iodide of æthyle is decomp. by potassium. Hydrate of laurel turpentine sublimes. Hyd. periodic acid, malæic acid, pyromucic acid, and Paste varnish fuse. Pyromucamide fuses; 266° to 269°.
268° F. Narcotine fuses.
270° F. Fousel-oil boils.—*Most Authors.*
Dammarone and chlorocinnamic acid fuse.
273° F. Dichloride sulphur boils.—*Marchand;* also terebiline.
275° F.; 135° C. Campholen, and a sat. sol. of carb. potash (*Legrand*) boil. Colophony (Rosin), and carb. of bichlor. of sulphur fuse.
275·2 F. Water boils (3 atmos.)
276° F. Potash-ley of 1·52 or 42·9⅜ boils.
277° F. Nitrocinnamic æther fuses; mesityle boils.
278·5 F. Anhyd. acetic acid boils.
280° F. Morson's commercial "creasote," or oil of tar, begins to boil; temp. gradually rises to 420°.
Temp. most conducive to the formation of æther, by the ordinary process.
Paramylene boils.—(*De Claubry*).
281° F. Dichloride of sulphur boils.
284° F.; 140° C. The germinating power of the spores of *Oïdium aurantiacum* is completely destroyed.
Hyd. perchloric acid is volat.
Oil of amber, propionic acid, cyanarsin, and nitrate of amyl boil.
A sat. solu. of carb. potash boils.
Caoutchouc may be vulcanised by immersion in a bath of the polysulphide of calcium, of 25° Baumé.

285° F. Glucose is caramelised.
β-hydrate of succinic acid is sublimed.
285·1° F. Water boils (pressure = 3½ at.)
289·4° F. Vitriol of 1·52 or 50% boils.
Oil of hyssop boils.
290° F. Mineral tallow boils.
Fulminating gold explodes; and sometimes even below 212°.
Potash-ley of 1·6 or 46·7% boils.
291° F. Butyrone boils.
Pyroxanthine fuses.
293° F.; 145° C. Chloride of tantalium and cholesterine fuse.
Benzoïc acid sublimes; 293° to 295°.
293°·75 F. Tereben *boils.*
Water boils (pressure = 4 atmos.)
295° F. Anhyd. succinic acid fuses.
Sulphocyanide of æthyl boils, also iodide of amyl.
297° F. East Indian grass-oil boils.
297°·5 F. Binacetate of potash fuses.
298° F. Nitrate of amyl-oxide boils.
Cumene boils; 298° to 307°.
300° F. Soda-ley of 1·63 or 46·6% boils.
Fulminating mercury explodes.
Potassium and sodium decomp. carbonic acid and liberate carbon.
Hatchettine boils above 300.
Bromide of silicon boils.
Zinc becomes very ductile and malleable between 212° and 300°.
300°·25 F. Laurel turpentine boils.
Water boils (pressure = 4½ atmos.).
Citric acid is decomp.
301° F. Bisulphide of soda, lactose, and salicylic acid fuse.
302° F.; 150° C. Oxide of cacodyl, oil of garlic, and retinyl boil.
Anisole boils; 302° to 305°.
Coriander oil boils. *Kawalier.*
Iodide of æthyl is decomp. by zinc.
Aloine fuses.
303° F. Coniine boils.—*Geiger.*
304° F. A sat. solu. of nitrate of lime boils.
306° F. Bromoform boils.
307° F. Lactucone boils; 307° to 309°.
Œnanthole boils; 307° to 316°.
307°·5 F. Water boils (pressure = 5 atmos.)
309° F. Tolene boils; 309° to 320°.
310° F. Kinic acid and masopine *fuse.*
311° F.; 155° C. Amyl boils.
313° F. Idrialine fuses above 313°.
314°·5 F. Camphine boils. [Latent heat of vapor = 184°.—*Ure.*]

315° F. Water boils (5½ atmos.)
316° F. Potash-ley, sat. at 60°, boils.—*Some Authors.*
Tersulphide of nitrogen explodes.
317° F. Nitrate of urea is decomp.
318° F. Ethylomeconic acid fuses; 316° to 318°.
320° F.; 160° C. Persian naptha boils; 320° to 365°. [Latent heat = 184°.]
Malæic acid boils; 320° to 350°.
Sesquichl. of carbon, crys. succinic acid, and trichloroquinone fuse.
Paramylene, and chlorobutyrene boil.
320°·4 F. Water boils (pressure = 6 atmos.)
323° F. Furfurole, oxalate of methyl, and sulphurous æther boil.
324° F. Silicic æther, 3-basic, is volat.
Cinnamic æther boils.—*Kopp.*
325° F. 3-hyd. of sulphuric acid boils; 325° to 338°.
326°·25 F. Water boils (pressure = 6½ atmos.)
327° F. Butyric acid boils.
329° F.; 165° C. Potash-ley of 1·68 or 51·2% boils.
330° F. "*Pulvis fulminans*" explodes.
331° F. Mannite fuses.
331°·7 F. Water boils [pressure = 7 atmos.]
333° F. Oxybromide of phosphorus boils; 333° to 392°.
334° F. Terpinole boils. Carotine fuses.
335° F. Coniine boils; 335° to 360°.—(*Blyth*).
336° F. A sat. solu. acetate of potash boils.
336°·9 F. Water boils [pressure = 7½ atmos.]
338° F.; 170° C. Tartaric acid is changed into tartralic acid.
Diamylamine and cacodyl boil.
340° F. Cymene boils.
341°·8 F. Water boils [pressure = 8 atmos.]
343°·5 F. Carven boils.
345° F. Oil of elemi boils.
346° F. 3-bromide of phosphorus boils.
347° F.; 175° C. Camphor fuses.
Valeric acid boils; 347° to 387°; amyl-oxide, 347° to 361°; and iodine, 347° to 356°.
350° F. Oil of bitter almonds boils.
Fulminating zinc explodes.
Oxalic acid is decomp.; 350° to 450°.
350°·5 F. Urethylane boils.
350°·6 F. Vitriol boils (sp. gr. 1·65 or 58·6%.)
350°·8 F. Water boils (pressure = 9 atmos.)
352° F. Caprylone boils.
356° F.; 180° C. Arsenic sublimes (without fusing).—*Donovan.*
Gun-cotton inflames.—*Pelouze.*
Chl. of aluminium boils; 356° to 365°.
Crys. chromic acid fuses; 356° to 374°.
a-oil of thyme, oil of orange peel, and urethane boil.

358°·8 F. Water boils (pressure = 10 atmos.)
359°6 F. Vitriol of 1·67 or 60$\frac{0}{0}$ boils.
360° F. Arsenic sublimes in closed vessels. Aniline boils.—*Hofmann.*
A sat. solu. of nitrate of ammonia; and sesquichloride of carbon, and oxalic æther boil.
Cantharidin slowly sublimes; 300° to 360°.
365° F.; 185° C. Itaconanilide fuses.
366°·8 F. Vitriol of 1·684 or 61$\frac{8}{8}$ boils.
366°·9 F. Water boils (pressure = 11 atmos.)
367°·5 F. Fulminating mercury explodes, but often at much lower temps.
370° F. Periodic acid is rapidly decomp.; 370° to 374°, and is changed into iodic acid.
Commercial "*creasote*" (rich in napthalin ?) boils between 370° and 380°.
374° F.; 190° C. Water boils (pressure = 12 atmos.)
Vitriol of 1·699 or 62$\frac{0}{0}$ boils.
Fused sulphur, when dropped into water, becomes soft and plastic at all temps. between 374° and 500°.
376° F. Coniine boils; 376° to 377°.—(*Christison*).
Kyanethine fuses.
380°·7 F. Arsenious acid sublimes.—*Piria.*
Water boils (pressure = 14 atmos.)
382° F. Carbonate of silver is changed into the oxide.
383° F.; 195° C. Vitriol of 1·715 or 63$\frac{8}{8}$ boils.
384° F. Crys. cane-sugar fuses.
385° F. Salicylous acid fuses; 385° to 400°.
Chloracetic acid boils; 385° to 390°.
386° F. Crys. bisulphate of potash; 386° to 392°.
3-chloride of antimony boils.
Sulphocyanide of amyl boils.—*Medlock.*
387° F. Water boils (pressure = 14 atmos.)
388° F. Fibrine and coagulated albumen dissolve in water in Papin's digester.
Toluidine boils.
390° F. Caproic acid boils; 390° to 394°.
Binacetate of potash becomes the neutral acetate.
391°·5 F. Chloralide boils.
392° F.; 200° C. Vitriol boils (sp. gr. 1·73 or 64$\frac{8}{8}$.)
Starch is converted into torrefied starch or dextrine.
392°·9 F. Water boils (pressure = 15 atmos.)
393° F. Xylite-oil boils above 393°.
394° F. Coniine boils.—*Ortigoza.*
394°·5 F. Terebilic acid fuses.
396° F. Caproyle boils.
397° F. Pure creasote boils ?

(*To be continued.*)

TRANSLATIONS AND ABSTRACTS.

CHEMICAL PHILOSOPHY.

On Osmotic Force. By Professor Graham.

This name was applied to the power by which liquids are impelled through moist membrane and other porous septa in experiments of endosmose and exosmose. It was shewn, that with a solution of salt on one side of the porous septum and pure water on the other side (the condition of the osmometer of Dutrochet when filled with a saline solution and immersed in water), the passage of the salt outward is entirely by diffusion, and that a thin membrane does not sensibly impede that molecular process. The movement is confined to the liquid salt particles, and does not influence the water holding them in solution, which is entirely passive; it requires no farther explanation. The flow of water inwards, on the other hand, affects sensible masses of fluid, and is the only one of the movements which can be correctly described as a current. It is osmose and the work of the osmotic force to be discussed.

As diffusion is always a double movement, while salt diffuses out, a certain quantity of water necessarily diffusing in at the same time in exchange, diffusibility might be imagined to be the osmotic force. But the water introduced into the osmometer in this way has always a definite relation to the quantity of salt which escapes, and can scarcely rise in any case above four or six times the weight of salt, while the water entering the osmometer often exceeds the salt, leaving it at least one hundred times. Diffusion, therefore, is quite insufficient to account for the water current.

The theory which refers osmose to capillarity appears to have no better foundation. The great inequality of ascension assumed among aqueous fluids is found not to exist when their capillarity is correctly observed; and many of the saline solutions which give rise to the highest osmose are indistinguishable in ascension from pure water itself.

Two series of experiments on osmose were described, the first series made with the use of porous mineral septa, and the second series with animal membrane. The earthenware osmometer consisted of the porous cylinder employed in voltaic batteries, about five inches in depth, surmounted by an open glass tube 0·6 inch in diameter, attached to the mouth of the cylinder by means of a cup of gutta percha. In conducting an experiment the cylinder was filled with any saline solution to the base of the glass tube, and immediately placed in a large jar of distilled water; and as the fluid within the instrument rose in the tube, during the experiment, water was added to the jar, so as to prevent inequality of hydrostatic pressure. The rise (or fall) of liquid in the tube was highly uniform, as observed from hour to hour, and the experiment was generally terminated in five hours. From experiments made on solutions of every variety of soluble substances, it appeared that the rise or osmose is quite insignificant with neutral organic substances in general, such as sugar, alcohol, urea, tannin, &c.;

so also with neutral salts of the earths and ordinary metals, and with chlorides of sodium and potassium, nitrates of potassa and soda, and chloride of mercury. A more sensible, but still very moderate osmose, is exhibited by hydrochloric, nitric, acetic, sulphurous, citric and tartaric acids. These are surpassed by the stronger mineral acids, such as sulphuric and phosphoric acid, and sulphate of potassa, which are again exceeded by salts of potassa and soda possessing either a decided acid or alkaline reaction, such as binoxalate of potassa, phosphate of soda and carbonates of potassa and soda. The highly osmotic substances were also found to act with most advantage, in small proportions, producing in general the largest osmose in the proportion of one quarter per cent. of salt dissolved. Osmose is, indeed, eminently the the phenomenon of weak solutions. The same substances are likewise always chemically active bodies, and possess affinities which enable them to act upon the material of the earthenware septum. Lime and alumina were accordingly always found in solution after osmose, and the corrosion of the septum appeared to be a necessary condition of the flow. Septa of other materials, such as pure carbonate of lime, gypsum, compressed charcoal, and tanned sole-leather, although not deficient in porosity, gave no osmose, apparently because they are not acted upon chemically by the saline solutions, capillarity alone was manifestly insufficient to produce the liquid movement, while the *vis motrix* appeared to be chemical action.

The electrical endosmose of Porrett, which has lately been defined with great clearness by Weidemann, was believed to indicate the possession of a peculiar chemical constitution by water, while liquid, or at least the capacity to assume that constitution when water is polarised and acting chemically upon other substances. A large but variable number of atoms of water are associated together to form a liquid molecule of water, of which an individual atom of oxygen stands apart, forming a negative or chlorous radical, while the whole remaining atoms together are constituted into a positive or basylous radical, which last will contain an unbalanced equivalent of hydrogen, giving the molecule basicity, as in the great proportion of organic radicals. Now, it is this voluminous basylous radical which travels in the electrical decomposition of pure water, and resolves itself into hydrogen gas and water at the negative pole, causing the accumulation of water observed there, while the oxygen alone proceeds in the opposite direction to the positive pole. Attention was also called to the fact that acids and alkalies, when in solution, are chemically combined with much water of hydration; sulphuric acid, for instance, evolving heat when the fiftieth equivalent of water is added to it. In the combination of such bodies, the disposal of the water is generally overlooked. Osmose was considered as depending upon such secondary results of combination, that is, upon the large number of voluminous proportions of the water molecules involved in such combinations. The porous septum is the means of bringing out and rendering visible, both in electrical and ordinary osmose, this liquid movement attending chemical combinations and decompositions.

Although the nature and *modus operandi* of the chemical action producing osmose remains still very obscure, considerable light is thrown upon it in the application of septa of animal membrane. Ox bladder was found to acquire greatly increased activity, and also to act with much greater regularity when first divested of its outer muscular coat. Cotton calico also impregnated with liquid albumen, and afterwards exposed to heat so as to coagulate that substance, was sufficiently impervious, and formed an excellent septum, resembling membrane in every respect. The osmometer was of the usual bulb-form, but the membrane was supported by a plate of perforated zinc, and the instrument provided with a tube of considerable diameter. The diameter of the tube being 1–10th of that of the mouth of the bulb or the disc of membrane exposed to the fluids, a rise of liquid in the tube, amounting to 100 millemetres, indicated that as much water had permeated the membrane and entered the osmometer, as would cover the whole surface of the membrane to a depth of one millimetre, or 1–25th part of an inch. Such millimetre divisions of the tube become degrees of osmose, which are of the same value in all instruments.

Osmose in membrane presented many points of similarity to that in earthenware. The membrane is constantly undergoing decomposition, and its osmotic action is exhaustible. Further, salts and other substances, capable of determining a large osmose, are all chemically active substances, while the great mass of neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action, or are wholly inert. The active substances are also relatively most efficient in small proportions.

When a solution of the proper kind is used, the osmose or passage of fluid proceeds with a velocity wholly unprecedented in such experiments. The rise of liquid in the tube with a solution containing 1–10th per cent. of carbonate of potassa in the osmometer, was 167 degrees or millimetres, and with 1 per cent. of the same salt, 206 degrees in five hours. With another membrane and stronger solution, the rise was 863 millimetres, or upwards of 30 inches in the same time, and as much water, therefore, was impelled through the membrane as would cover its whole surface to a depth of 8·6 millimetres, or 1–3rd of an inch. The chemical action must be different on the substance of the membrane at its inner and outer surfaces to induce osmose; and according to the hypothetic view which accords best with the phenomenon, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane, at the inner surface, and as an acid action on the albumen at the outer surface. The most general empyrical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt, such as carbonate or phosphate of soda in the osmometer, and water outside, the flow is inwards; but with an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube. In the last

case the water outside is basic when compared with the acid within, and the flow is, therefore, still towards the base.

The chloride of sodium, chloride of barium, chloride of magnesium, and similar neutral salts, are wholly indifferent, or appear only to act in a subordinate manner to some other active acid or basic substance, which last may be present in the solution or membrane in the most minute quantity. Salts which admit of dividing into a basic subsalt and free acid exhibit an osmotic activity of the highest order. Such are the acetate and various other salts of alumina, iron, and chromium, the protochloride of iron, chloride of copper and tin, chloride of copper, nitrate of lead, &c. The acid travels outward by diffusion, superinducing a basic condition of the inner surface of the membrane, and an acid condition of the outer surface, the favorable condition of a high positive osmose. The bibasic salts of potassa and soda again, such as the sulphate and tartrate of potassa, although strictly neutral in properties, begin to exhibit a positive osmose, in consequence, it may be presumed, of their resolution into an acid supersalt and free alkaline base.

The following table exhibits the osmose of substances of all classes :—

Osmose of 1 *per cent. Solutions in Membrane.*

	deg.		deg.
Oxalic acid	—148.	Chloride of zinc . .	54.
Hydrochloric acid . .	— 92.	Chloride of nickel . .	88.
Terchloride of gold . .	— 54.	Nitrate of lead .	125 to 211.
Bichloride of tin . .	— 46.	Nitrate of cadmium . .	137.
Bichloride of platinum .	— 30.	Nitrate of uranium	234 to 458.
Chloride of magnesium .	— 3.	Nitrate of copper . .	204.
Chloride of sodium . .	+ 2.	Chloride of copper . .	351.
Chloride of potassium .	18.	Protochloride of tin . .	289.
Nitrate of soda . . .	2.	Protochloride of iron .	435.
Nitrate of silver . . .	34.	Chloride of mercury . .	121.
Sulphate of potassa .	21 to 60.	Protonitrate of mercury.	356.
Sulphate of magnesia .	14.	Pernitrate of mercury .	476.
Chloride of calcium . .	20.	Acet. of sesquioxide of iron	194.
Chloride of barium . .	21.	Acetate of alumina	280 to 393.
Chloride of strontium .	26.	Chloride of aluminum .	540.
Chloride of cobalt . .	26.	Phosphate of soda . .	311.
Chloride of manganese .	34.	Carbonate of potassa . .	439.

It may appear to some that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to be incompatible with vital conditions, and that osmotic movements must therefore be confined to dead matter; but such apprehensions are, it is believed, groundless, or, at all events, premature. All parts of living structures are allowed to be in a state of incessant change, of decomposition and renewal. The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly, therefore, be of a reparable kind. In other respects chemical osmose appears to

be an agency particularly adapted to take part in the animal economy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess, is another most favorable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems, therefore, almost inevitable.

In osmose there is further a remarkably direct substitution of one of the great forces of nature by its equivalent in another force—the conversion as it may be said, of chemical affinity into mechanical power. Now what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues. In minute microscopic cells the osmotic movements being entirely dependent upon extent of surface, may attain the highest conceivable velocity. May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link which certainly intervenes between muscular movement and chemical decomposition.

Abstract of the Bakerian Lectures; Proceedings of the Royal Society, June 15, 1854; *and Pharmaceutical Journal.*.

ORGANIC CHEMISTRY.

Observations *on* Milk. By M. Morin, of Geneva.

(Concluded from page 604.*)*

The presence of *gelatiniform matter* is a fact which I had recognised in studying the passage of milk through membranes, but I was far from expecting to find nearly as much of it as of albumen, about one-tenth of the weight of the caseum. So large a quantity of this substance in the liquid which forms the first aliment of mammiferous animals, supports the views published in 1843 by M. Prévost and myself, on the general presence of this substance in the organism and on the part which it performs in the functions of nutrition.*

Fourcroy and Vauquelin detected in the mother-waters of sugar of milk the presence of a matter quite distinct from caseum, not coagulable by the acids, precipitating with tannin and chlorine, and very analogous to fermented gluten. It is evident from these characters that they had acted on the gelatiniform matter or gelatine.

With regard to the gelatiniform matter, I may remark that the recent ingenious researches of M. Corvisart on the composition of white of egg,† completely confirm that which we have said of its presence in the white and yolk of the egg. M. Corvisart ascertained that the albumen of the egg cannot be converted into albuminose under the influence of water, but that this substance pre-exists in the egg. By employing gastric juice for its extraction, he obtained more of it, without, however, being able to produce nearly the entire transformation of the albumen. These results appeared to him sufficiently

* *See* The Chemist, vol. iv., 1843, p. 389.

† *Comptes Rendus de l'Académie des Sciences*, t. xxxv., p. 244.

conclusive for doing away with the name of *albuminose*, which rested on a false idea, and for substituting for it that of *ex-albumen*, indicating only that it has been extracted from albumen.

If I have rightly understood the note in the *Comptes Rendus* relative to M. Corvisart's work, there are some contradictions in these data. Indeed, if the greater part of the white of egg cannot be transformed into ex-albumen, under the influence of the gastric juice, and if, however, a portion undergoes this transformation, there should be two kinds of albumen in this same white of egg, which the author does not appear to admit. But if the gastric juice itself contains ex-albumen, the facts noticed by M. Corvisart are easily explained; for by employing this liquid instead of water for the treatment of white of egg, we should obtain a greater quantity of ex-albumen, comprising that of the egg and that of the gastric juice.

The researches on the digestion of the herbivora, which I have already quoted,§ had precisely the result of shewing the presence, in the gastric juice, of the substance to which we have provisionally given the name of gelatiniform matter, and which possesses the properties attributed by M. Corvisart to ex-albumen. Consequently, this new designation, based on an erroneous idea, does not appear more happily chosen than that of albuminose, for the gelatiniform matter.

As to the *gelatine*, which exists ready formed in milk, or which is produced during the evaporation of the serum from which the cheese and second cheese have been extracted, it is easy to render its presence palpable. It is sufficient, after having carried the operation far enough to determine the crystallisation of the sugar of milk, to mix the mother-liquors with a small quantity of alcohol, which may prevent the putrefaction of that liquid, without producing the precipitation of the substances which it still contains. The mass, after a certain time, assumes the form of a jelly, as to the nature of which the ordinary reactions of gelatine leave no doubt. Fifteen litres of serum treated in this manner produce about half a litre of a jelly, which retains the gelatiniform matter and the substances soluble in alcohol.

The caseum possesses the property of carrying with it, in coagulating, almost all the butter. The manufacture of cheese reposes more or less on this fact.

This affinity of caseum for the fatty body, or the faculty which it possesses of removing, as if the coagulated net-work acted as a filter, appeared to me to be connected with a corresponding property in caseum in solution, that of forming the fatty body into an emulsion.

To ascertain this, I evaporated skimmed milk to dryness at the temperature of 176° F. I freed it by means of ether from all the butter which it still contained, and I made the following experiments.

First Experiment.—One part bruised with thirty of water produced a slightly milky liquid, which became clear in half-an-hour by depositing the pulverulent albumen to which it owed its opacity.

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‡ "*Nutrition dans l'œuf*," *Journal de Pharmacie*, 3e Série, t. ix., p. 323.

§ *See* THE CHEMIST, vol. iv., 1843, p. 389.

Second Experiment.—One part treated by the same process furnished, with 2 of oil of almonds and 15 of water, an emulsion as stable as milk, but from which the pulverulent albumen separated as promptly as in the foregoing case. In coagulating the liquor with acetic acid, the oil was almost entirely carried away by the caseum.

Third Experiment.—The same result with lard at the temperature of 95° F; but the fatty body separated by cooling, removing with it a portion of the caseum.

From these experiments it results, that *the caseum and the other soluble substances of milk possess, independently of the albumen, the property of forming an emulsion with oil and lard*, but the latter in smaller quantity and incompletely.

In order to ascertain whether the caseum alone possessed the property of forming an emulsion with the oil, the same experiments were repeated separately with the gelatiniform matter, containing a little gelatine, and with the substances soluble in alcohol.

The latter substances do not possess this property at all; whilst the gelatiniform matter, reduced with water to a syrupy consistence, perfectly maintained its own weight of oil in suspension for many days. It did not emulsify a larger proportion. Gelatine is also endowed with the emulsive property, but in a less degree than the gelatiniform matter.

Consequently, *if this matter does not emulsify the fatty bodies in as large quantity as caseum, the result is more stable*, which is owing, doubtless, to the solution not being so promptly altered as that of caseum.

If it were urgent to give to the gelatiniform matter a more concise designation, it might be derived from this property of rendering aqueous liquids lactescent by emulsifying oil, a property which it partakes with caseum in a remarkable degree, and which is perhaps connected with the part which it plays in the animal organism. The name of *galactine*, if adopted after verification of the facts which I have mentioned, would have the additional advantage of indicating that this substance is found in considerable quantity in milk, and in other lactescent secretions, such as the liquid of the cotyledons of the cow, a kind of milk which serves for the nourishment of the fœtus.*

I will conclude with a few remarks on the nutritive quality of the elements of cow's milk.

1. If we admit the division of aliments into combustible or respiratory, and nitrogenous or assimilable, we find that in the entire milk, the amount of the former—butter and sugar—is nearly double that of the latter—caseum, albumen, galactine, and substances soluble in alcohol; and that thus the 14 or 15 per cent. of organic substances which milk contains are divided into :—

9 or 10 of combustible aliments.
4 or 5 of nitrogenous aliments.

* From the properties of the pancreatic juice, I am persuaded that it contains a considerable proportion of galactine, and, perhaps, of caseum.

2. In skimmed milk, retaining one-third of the total quantity of butter, like that which I have analysed, these two kinds of aliments exist in nearly equal weight, and form together not more than 10 or 11 per 100 of milk. This liquid loses then about one-fourth of its nutritive value by being skimmed, and probably more in relation to its commercial value.

3. After the extraction of the butter, cheese, and second-cheese, there remains in the whey nearly one-third of the organic substances of the milk, namely, 4 or 5 per cent., and the sugar and the combustible aliment exist in it in four times as large quantity as the nitrogenous matter.

4. Finally, in the countries where sugar of milk is made, the mother-liquors still contain rather more than a twelfth of the solid substances of the milk, or from 1 to 1½ per cent. of the milk originally employed. This remainder consists only of nitrogenous aliments, and a little chloride of sodium; it is, I believe, used for feeding pigs and other animals.

The experiments above mentioned shew:—

That the caseum exists in milk in the state of combination with a fixed base, oxide of sodium, from which it separates in coagulating.

That milk contains a considerable proportion of gelatiniform matter, and that it produces by boiling, or contains ready formed, a small quantity of gelatine.

That caseum and the gelatiniform matter in solution possess, independently of each other, the property of emulsifying oils—caseum in the largest quantity, and the gelatiniform matter in a more stable manner.

This property, and the circumstance that the gelatiniform matter exists in considerable quantity in the lactescent secretions, justify the name of *galactine*, by which I propose to designate it.

Journal de Pharmacie, June, 1854.

On a New Series *of* Sulphuretted Acids.

By Dr. August Kekulé.

Adopting the idea that the series of organic compounds of which sulphuretted hydrogen is the type, corresponds in every respect with the series of which water is the type, I concluded that not only mercaptans and neutral sulphides which correspond to the alcohols and ethers, but also compounds corresponding to the acids, anhydrous acids and ethers of acids might be produced; I, therefore, endeavored to obtain reactions which would enable me to replace oxygen in the compounds of the latter series by sulphur.

Such reactions are produced by the compounds of sulphur with phosphorus—the tersulphide (P^2S^3) and the pentasulphide (P^2S^5)—which are easily obtained by fusing together amorphous phosphorus

and sulphur in an atmosphere of carbonic acid; no explosion takes place, although the combination is attended with a very violent action.

Experiment has proved that these combinations of sulphur and phosphorus act on the members of the series of water in the same manner (although less violently) as the corresponding compounds of chlorine and phosphorus;—however, with this difference, that by using the chlorine compounds the product is resolved into *two* groups of atoms, while by using the sulphur compounds there is obtained only *one* group; a peculiarity, which, according to the bibasic nature of sulphur, must have been expected. By acting on these compounds of sulphur and phosphorus with water, one atom of sulphuretted hydrogen is obtained, while the chlorides give two atoms of hydrochloric acid.

$$6\left.\begin{matrix}H\\H\end{matrix}\right\}O + P^2S = 3\left.\begin{matrix}H\\H\end{matrix}\right\}S + 2PO^3H^3$$

$$6\left.\begin{matrix}H\\H\end{matrix}\right\}O + 2PCl^3 = 6HCl + 2PO^3H^3.$$

Similar reactions are observed with organic compounds belonging to the series of water with the formation of phosphorous and phosphoric acids respectively, or a conjugate acid. By acting in this way, the following series of sulphuretted organic compounds is obtained, by the side of which are placed for comparison the products formed by the action of the chlorides of phosphorus on the same substances.

Sulphuretted Hydrogen.	Hydrochloric Acid.
$\left.\begin{matrix}H\\H\end{matrix}\right\}S.$	$2HCl.$
Mercaptan.	**Chloride of Ethyle + Hydrochloric Acid.**
$\left.\begin{matrix}C^2H^5\\H\end{matrix}\right\}S.$	$C^2H^5Cl + HCl.$
Sulphide of Ethyle.	**Chloride of Ethyle.**
$\left.\begin{matrix}C^2H^5\\C^2H^5\end{matrix}\right\}S.$	$2C^2H^5Cl.$
Othyl-Hydrosulphuric Acid.	**Chloride of Othyle + Hydrochloric Acid.**
$\left.\begin{matrix}C^2H^3O\\H\end{matrix}\right\}S.$	$C^2H^3O, Cl + HCl.$
Othyl-Sulphide of Othyle.	**Chloride of Othyle.**
$\left.\begin{matrix}C^2H^3O\\C^2H^3O\end{matrix}\right\}S.$	$2C^2H^3O, Cl.$
Othyl-Sulphide of Ethyle.	**Chloride of Othyle × Chloride of Ethyle.**
$\left.\begin{matrix}C^2H^3O\\C^2H^5\end{matrix}\right\}S.$	$C^2H^3O, Cl + C^2H^5Cl.$

Mercaptan is obtained by the action of tersulphide or pentasulphide of phosphorus on alcohol with extreme facility. Sulphide of ethyle may also be prepared by acting on ether in a similar manner.

Thiacetic Acid,—Sulphuretted Acetic Acid,—has been obtained by

me by acting on monohydrated acetic acid with tersulphide of phosphorus. It is a colorless liquid, boiling at about 199° F., and has a peculiar odor resembling sulphuretted hydrogen and acetic acid. It dissolves potassium in the cold and zinc on heating with the evolution of hydrogen, and gives with lead a salt less soluble than the ordinary acetate, so that it gives a precipitate with acetate of lead. By recrystallisation from water or alcohol, the lead salt is obtained in fine silky needles, which, though quite colorless at first, are rapidly decomposed (whether in solution or in the solid form) with the formation of sulphuret of lead.

By analysis I found the lead salt contained—

Lead 58·8 per cent. Theory requires 58·0 per cent.

The acid contained—

Sulphur 41·3 per cent. Theory requires 42·1 per cent.

Thiacetic acid is also formed in small quantity and by secondary action, by distilling pentasulphide of phosphorus with fused acetate of soda. Pentachloride of phosphorus gives a violent reaction with thiacetic acid, yielding sulphochloride of phosphorus, chloride of othyle, and hydrochloric acid,

$$\left.\begin{matrix} C^2H^3O \\ H \end{matrix}\right\} S + PCl^5 = C^2H^3OCl + HCl + PSCl^3$$

Thiacetate of Othyle.—Sulphide of Othyle.—Anhydrous Sulphuretted Acetic Acid.—Pentasulphide of phosphorus acts but very feebly upon anhydrous acetic acid in the cold, but on heating a violent reaction takes place. By distilling the product, the anhydrous acid is obtained in the form of a colorless liquid, boiling at about 249°F., and having an odor greatly resembling sulphuretted acetic acid. On mixing with water it falls to the bottom, without, at first, suffering any change; on standing, however, it is slowly dissolved and decomposed into sulphuretted acetic acid and ordinary acetic acid. This change takes place much more rapidly on heating.

$$\left.\begin{matrix} C^2H^3O \\ C^2H^3O \end{matrix}\right\} S + \left.\begin{matrix} H \\ H \end{matrix}\right\} O = \left.\begin{matrix} C^2H^3O \\ H \end{matrix}\right\} S + \left.\begin{matrix} C^2H^3O \\ H \end{matrix}\right\} O.$$

It appears that anhydrous sulphuretted acetic acid is also produced by acting on the othyle-sulphide of lead with chloride of othyle, at all events chloride of lead is formed. Chloride of benzoyle gives with the lead salt a similar reaction, and it is probable that an intermediate sulphuretted acid is formed, having the formula

$$\left.\begin{matrix} C^2H^3O \\ C^7H^5O \end{matrix}\right\} S.$$

Thiacetate of Ethyle.—Sulphuretted Acetic Ether.—This compound may be prepared by the action of pentasulphide of phosphorus on acetic ether. It is a liquid lighter than water, and possesses an odor resembling acetic ether and sulphuretted hydrogen. It boils at about 176° F.

It will be seen that the action of tersulphide and pentasulphide of phosphorus above described produces sulphuretted organic compounds

by substituting sulphur for oxygen. The compounds obtained in this way may also be formed by replacing one or two atoms of hydrogen in sulphuretted hydrogen (H^2S), or one or two atoms of metal in sulphide of potassium (K^2S), or in sulphide of hydrogen and potassium (KHS), by organic radicals. Mercaptan, and the sulphides of alcohol radicals have, in fact, been long obtained in this manner.

The formation of a sulphuretted compound containing an acid radical has been observed by Gerhardt by acting on sulphide of lead with chloride of othyle. I have not made many experiments of this kind, but I have observed that chloride of benzoyle is not decomposed by sulphuretted hydrogen, while it (as well as chloride of othyle) gives a reaction with sulphide of hydrogen and potassium yielding chloride of potassium.

I am continuing these researches, and believe the above reactions will furnish many new compounds, and will tend to complete our knowledge of some of those organic and inorganic compounds now known.—*Royal Society.*

On a CLASS *of* COMPOUNDS *homologous to* QUINOILE *and its derivatives.*

By M. LALLEMAND.

THE stearoptene of essence of thyme, or thymol, whose composition and properties I have already described (*see* "THE CHEMIST," Nov., 1853, p. 97), is capable of producing, under the influence of oxidising agents, such as chromic acid or a mixture of sulphuric acid and binoxide of manganese, a solid crystallised substance, which, in its properties and metamorphoses, resembles the quinoïle or quinone obtained by M. Wöhler, by oxidising quinic acid.

The most simple process for preparing this compound, which I shall call *thymoïle*, consists in combining thymol with an excess of sulphuric acid. The sulphothymic acid thus obtained, having been diluted with five or six times its volume of water, is introduced into a retort, and mixed with an excess of binoxide of manganese; the mass becomes heated, and distillation commences; the application of a moderate artificial heat causes an aqueous liquid to pass into the receiver, mixed with small oily drops, of an orange-yellow color, which soon become solid. The aqueous portion is simply very dilute formic acid. The residue is a solid brownish resin, soluble in water, to which it communicates a red color. The solid matter condensed in the receiver, dissolved in hot alcohol, or, still better, in etherised alcohol, is readily crystallised.

Thymoïle has a powerful aromatic odor, somewhat resembling that of iodine. It is very sparingly soluble in water, more soluble in alcohol, and very soluble in ether, prolonged contact with which, however, alters it. It crystallises in quadrangular plates of a beautiful orange yellow color, and of great brilliancy. It fuses at 118°F., forming a dark yellow liquid, and emits abundant vapors at 212°F. However, on attempting to distil it, the temperature rises rapidly to 455°F.,

and, at the same time that a large proportion sublimes, unaltered, in the neck of the retort, it decomposes and leaves a dark red oily residue, which solidifies, assuming a beautiful violet tint with an iridescent reflection.

Three concordant analyses gave as its formula $C^{24}H^{16}O^4$, the homologue of $C^{12}H^4O^4$, which represents quinoïle, of which it possesses the characteristic properties and reactions. When it is subjected to the action of reducing agents, when, for instance, it is put in contact with a solution of sulphurous acid, it immediately becomes of a deep violet color; prolonged contact with the sulphurous solution transforms it, in the course of a few days, into a white crystallised body, sparingly soluble in hot water, readily soluble in alcohol and ether. Fuming nitric acid and concentrated sulphuric acid dissolve it readily without heat; water separates it from them unaltered. With time, or at a higher temperature, these acids change it into fresh products. The action of chlorine is likewise very slow, and has no influence except with the aid of heat, when chloruretted bodies with the same formula are obtained, liquid ammonia dissolves it slowly, assuming a dark red tint; the alkalies act in the same manner.

The final product obtained with sulphurous acid is a homologue of the colorless pyroquinol or hydroquinone of Wöhler; when cooled in dilute alcohol it crystallises in small, colorless four-sided prisms. Its analysis after dessiccation *in vacuo*, leads to the formula $C^{24}H^{18}O^4$. Its reaction on thymoïle is similar to that of hydroquinone on quinoïle; it is a beautiful phenomenon of crystallisation, which is obtained by mixing equal weights of these substances in boiling alcohol. The mixture becomes immediately of a deep red color, and, on cooling, deposits beautiful prismatic crystals of a violet tint by transmitted light, giving metallic bronze reflections, similar to those observed on the elytra of many Coleoptera. Dilute formic acid distilled in sufficiently powerful proportion in the preparation of thymoïle, likewise acts on it as a reducing agent, and transforms it into these two new products.

Colorless *thymoïlol*, when subjected to the action of oxidising agents, reproduces the two bodies from which it was formed. Perchloride of iron, dilute nitric acid, and chloruretted water, cause the instantaneous formation of violet crystals, and a larger quantity of the reagent reproduces thymoïle. The new compounds which I mention, lead me also to consider quinone and its derivatives as the first terms of a numerous series having peculiar chemical qualities and very interesting physical properties. We have, in fact,

$C^{12}H^4O^4$ quinoïle (quinone)	homologue of,	$C^{24}H^{16}O^4$ thymoïle
$C^{12}H^6O^4$ quineïde (green hydroquinone)	„	$C^{24}H^{17}O^4$ thymeïde
$C^{12}H^8O^4$ quinoïnol (pyroquinol)	„	$C^{24}H^{18}O^4$ thymoïlol

Between the two terms of these three series, five others might be introduced, whose preparation might be imagined with a great degree of probability. Chemists adopt a double equivalent of the preceding for quinoïle; but the existence of thymoïle and its volatility, appear

to me to justify the above formula, for, as far as I can see, no reaction, no metamorphosis, appear to necessitate the employment of so high an equivalent. The three bodies which I have thus described undergo numerous transformations, which I have partly studied; but my work is not yet in a sufficiently advanced state to be laid before the Academy.

Comptes Rendus, No. 23, June 5th, 1854.

On the Combinations *of* Oxide *of* Mercury *with* Allantoin.

By M. H. Limpricht.

When a solution of allantoïn is boiled with oxide of mercury, a certain quantity of this oxide is dissolved, and two combinations are principally formed. The first is deposited by the cooling of the liquor, in the pulverulent and amorphous state; it contains $3(C^8H^5N^4O^6)$, 5HgO. The second, containing less oxide of mercury, is deposited by the evaporation in a water-bath of the filtered liquor, and occurs under the form of an amorphous and transparent mass; treated with water, this matter is converted into a white powder, which contains $3(C^8H^6N^4O^6)$, 4HgO.

An aqueous solution of allantoïn is not precipitated by corrosive sublimate. Nitrate of mercury forms in it a voluminous precipitate, containing $2(C^8H^6N^4O^6)$, 5HgO.

These properties of allantoïn remind us of the manner in which urea acts in analogous conditions, and shew that the remarkable process which Liebig employs for estimating urea, is not applicable in the cases in which urea contains allantoïn.

Annalen der Chemie und Pharmacie.

On the Formation *of* Amarine, Furfurine, *and a new base*, Anisine.

By C. Bertagnini.

Under the influence of caustic potassa, the hydramides are transformed, as is known, into alkaloïds. The same transformation may be accomplished under the influence of heat alone.

When pure hydrobenzamide is exposed for three or four hours, to a temperature of 248° to 266° F., we obtain by cooling, a vitreous mass, which is no other than amarine.

Furfuramide, heated for half-an-hour to 230° or 248° F., is converted into furfurine.

Pure anishydramide fuses at 248° F. into a yellowish liquid, which, maintained for two hours at a temperature of from 329° to 338° F. is converted, without change of aspect, into a new base. This new product, which is isomeric with anishydramide, is *anisine*. In the state of purity, anisine crystallises in transparent prisms, sparingly soluble in boiling water and in ether, and soluble in alcohol; its solutions are strongly alkaline, and have a bitter taste. Its composition is represented by the formula

$$C^{16}H^{24}N^2O^4.$$

With the acids, it forms definite salts. The hydrochlorate crystallises in white needles containing $C^{16}H^{24}N^2O^4$, $HCl + 2\frac{1}{2}HO$; at 212° F. this salt loses its water. It forms, with chloride of platinum, a double salt, crystallisable in orange-yellow lamillæ, containing $C^{16}H^{24}N^2O^4$, $HCl + PtCl^2$.

Annalen der Chemie und Pharmacie.

On the Formation *of* Benzonitrile *from* Hippuric Acid.

By MM. Limpricht and Von Uslar.

It is known that Fehling obtained benzonitrile or nitrobenzile by the distillation of benzoate of ammonia. This body is also formed by the dry distillation of hippuric acid.

At 266° F. this acid fuses, and at 464° F. this fused mass enters into ebullition, disengaging vapors of benzoïc acid, traces of prussic acid, and benzonitrile, whose vapors condense into an oleaginous liquid.

Purified by distillation with water, and rectification over caustic lime, this liquid is colorless, powerfully refringent, and possesses the odor of essence of bitter almonds. Its boiling point is at 377° F., and its composition is represented by the formula $C^{14}H^5N$.

Annalen der Chemie und Pharmacie.

On Fraxinine, *the crystallisable principle in the* Bark *of the* Fraxinus excelsior *or* Common Ash. By John Stenhouse, LL.D., F.R.S.

It is stated in most systems of chemistry, such for instance as Löwig's, Liebig's, &c., on the authority of Messrs. Keller, Herberger, and Buchner, that the bark of the *Fraxinus excelsior* contains a neutral, crystallisable, bitter principle, to which they have given the name of fraxinine.

Their mode of extracting this principle was to treat an infusion of ash-bark, so long as a precipitate fell, first with neutral, and then with basic acetate of lead. The whole was then thrown upon a filter, and the clear liquid which passed through was treated with a current of sulphuretted hydrogen till all excess of lead was removed. The aqueous solution, when free from sulphide of lead, on being sufficiently concentrated, yielded crystals of fraxinine on standing for a few days in a cool place. I operated on 3lbs. of ash-bark exactly in the way just described. The impure crystals obtained from the aqueous solution were dried with blotting-paper, and when crystallised out of spirits of wine, were colorless. They had lost their bitter, and had acquired a sweetish taste. They had all the characteristics of mannite; and when they were subjected to analysis the following were the results:—

0·273 grm. substance, dried at 212° F., gave 0·194 grm. of water, and 0·389 grm. of carbonic acid.

	Calculated.		Found.
$C^{12} = 72$		39·5	38·897
$H^{14} = 14$		7·7	7·882
$O^{12} = 96$		52·8	53·221
182		100·0	100·000

The so-called fraxinine, therefore, is merely mannite. The reason why previous experimenters had mistaken it for a new bitter principle was simply this, that they had not freed it entirely from adhering impurities.

Philosophical Magazine, July, 1854.

INDUSTRIAL CHEMISTRY.

Method *of* Representing Objects *by* Printing Directly *from them*.

By Felix Abate, of Naples.

This invention constitutes a new art, by means of which natural and artificial objects can be represented and imitated by printing directly from the objects themselves upon any suitable substance. The specimens submitted to the inspection of the Society at its last meeting, are imitations of veneering wood, some simple, and some ornamented with inlaid work, made upon wood, calico, and paper.

Before entering into the details of this invention, I may, perhaps, be allowed to state, in order to prevent mistakes, that it is essentially different from the well-known invention under the name of *Phytoglyphy*, or Nature-printing, patented in England by Messrs. Bradbury and Evans, and practised at the Imperial Printing-office at Vienna, and which consists in taking impressions in lead or other metals, or gutta percha, from natural objects, making electro-plates from such impressions, and then printing with these plates in the usual way. The principle of my invention dates from an epoch anterior to the Great Exhbition of 1851, as I exhibited on that occasion the first specimens of a particular application of it, called *Metallography*. For this branch of the art I was rewarded with the prize medal. An idea of this art will be obtained from the following notice of the principles and processes upon which it rests :—

The art of *metallography* consists in printing from engraved wood blocks upon *metallic surfaces*, so as to produce imitations of figures and ornaments inlaid in wood. This effect is obtained by using, as a printing menstruum to wet the block with, solutions of such metallic or earthy salts as are decomposed when brought into contact with certain metals, and produce, through an electro-chemical action, an adhesive precipitate of a colored metallic oxide, or any other chemical change upon the metal. Such are the salts of copper, antimony, &c., upon zinc, tin, silver, &c. ; the hydrosulphuret of ammonia upon copper and brass.

There are two principles at work in this branch of the art: the one is the chemical action just referred to; the other, which is the foundation and the key-stone of the invention in its most general sense, rests on the porousness of the printing object, which causes the absorption of the wetting fluid, and yields it, under the action of pressure, in quantity for each point proportionate to the capacity of the pores; so that if any chemical change is wrought upon the impression, to produce a coloring of it, this coloring, by its different shades, makes a true representation of the printing object.

The application of the invention to printing upon vegetable substances instead of metallic surfaces, required the introduction into the process of some new principle to produce that chemical change which, in metallography, is spontaneous. I devised, for that purpose, two principles, which, by different means, lead to the same results. One of these principles I borrowed from the art of dyeing. It consists in the peculiar actions that the salts, acids, and alkalies have upon each other, and upon vegetable coloring matters. It is upon these actions the processes of mordant and discharge-printing on textile manufactures rest. The surface of the printing object is slightly wetted with the acting fluid, which is then well wiped off from the surface; the impression is then taken, which, by combining with a previous or a subsequent dyeing of the printed surface, instantaneously appears. The other principle I found in heat, that is, in the coloring action that this most powerful agent of Nature has upon vegetable substances when acted on by acids, which coloring, I believe, is the effect of an accelerated carbonisation of the surfaces of these substances produced by the acid. I think I may properly call this art THERMOGRAPHY, or the art of printing by heat.

From the following description of the process, it will be remarked—perhaps with some degree of surprise—the excessive sensitiveness of vegetable substances under the joint action of acids and heat, so that an infinitesimal dose of the former, and an instantaneous application of the latter, are sufficient to produce the most striking effects. The process is as follows:—

Suppose a sheet of veneering-wood be the object from which impressions are to be taken; I expose the wood for a few minutes to the cold evaporation of hydrochloric or sulphuric acid, or I slightly wet it with either of these acids diluted, and then well wipe the acid off from the surface. Afterwards it is laid upon a piece of calico or paper, or common wood, and by a stroke of the press an impression is taken, which is, of course, quite invisible; but by exposing this impression, immediately after, to the action of a strong heat, a most perfect and beautiful representation of the printing wood instantaneously appears. In the same way, with the same plate of wood, without any other acid preparation, a number of impressions, about twenty or more, are taken; then, as the acid begins to be exhausted and the impressions faint, the acidification of the plate must be repeated as above, and so on progressively, as the wood is not in the least injured by the working of the process for any number of impressions. All these impressions shew a

general wood-like tint, most natural for the light-colored woods, such as oak, walnut, maple, &c.; but for other woods that have a peculiar color, such as mahogany, rosewood, &c., the impression must be taken, if a true imitation be required, on a stuff dyed of the light color of the wood.

It must be here remarked, that the impressions, as above made, shew an inversion of tints in reference to the original wood, so that the light are dark, and *vice versâ*, which, however, does not interfere with the effect. The reason of it is, that all the varieties of tints which appear in the same wood are the effect of the varying closeness of its fibres in its different parts, so that where the fibres are close, the color is dark, and light where they are loose; but in the above process, as the absorption of the acid is greater in proportion to the looseness of its fibres, the effect must necessarily be the reverse of the above. However, when I wish to produce the true effect of the printing wood, I alter the process as follows:—I wet the surface upon which the impression is to be taken with dilute acid, and then I print with the veneering-wood previously wetted with diluted liquid ammonia; it is evident that in this case the alkali neutralising the acid, the effect resulting from the subsequent action of heat will be a true representation of the printing surface.

Such is thermography, or the art of printing by means of heat. Now it is nothing but natural to anticipate, in regard to this art, as well as to the other above described processes for printing directly from objects, that they will afford most important services to the natural, botanical, mineralogical, and anatomical sciences; as it is by their means that the internal structure of bodies is unveiled to the eyes of the philosopher, and the wonders of nature in its inexhaustible varieties are indefinitely multiplied, to be subjected to the investigation and to serve the gratification of mankind.

But the new art will prove not less useful to the decorative arts, particularly in its application to produce imitations of rare and costly woods, as well as of works of art, mosaic and inlaid work applicable for paper-hangings, or for furniture in the place of veneering, these imitations being produced at an exceedingly low cost, while they rival in perfection the original objects, enabling those whose means are limited to obtain decoration at once cheap and in good taste.

Journal of the Society of Arts.

AGRICULTURAL CHEMISTRY.

RESEARCHES *on* VEGETATION. By M. BOUSSINGAULT.

(*Continued from page 625.*)

§ 3. FIRST SERIES, 1851.

Estimation of the Nitrogen of the Seeds, in the state in which they were experimented on.—Dwarf beans collected in 1850.

Ten cubic centimetres of normal sulphuric acid being equal to 0·0875 grs. of nitrogen.

I. Bean weighing 0·780 gr.

		cc	
Amount of acid,	Before the experiment .	32·7	
	After „	19·7	
	Difference	13	{ equiv. to nitrogen 0·0348; 4·46 pr ct.

II. Bean weighing 0·798 gr.

Amount of acid,	Before the experiment	32·7	
	After „	19·3	
	Difference	13·4	{ equiv. to nitrogen 0·0358; 4 485 pr ct

III. Two beans weighing 1·040 gr. Estimation with oxide of copper. Nitrogen gas measured over water, 39·4 cubic centimetres; temperature 44° 6′ F.

Barometer	0·742
Tension	0·007
Pressure	0·735

Gas at 32° F. and pressure $0{\cdot}76^{m} = 37$ cubic centimetres, in weight 0·0466; 4·480 per cent.

	Exp. I.	II.	III.	Mean.
Nitrogen per cent. . .	4·460	4·485	4·480	4.475

Cultivation of a dwarf bean during two months.

First experiment.—A bean, weighing 0.780, which should contain, according to the foregoing analysis, 0·0349 grs., was sowed on the 20th of August, in suitably prepared pummice soil, containing ashes of dung.

Sept. 1.—The seminal leaves are developed.

Oct. 4.—Besides the seminal leaves there are six leaves of a very pale green.

Oct. 20.—The seminal leaves are decolored, and the cotyledons decayed, but still adhering to the stem.

Oct. 21.—The experiment was concluded. The plant had twenty-six well-formed leaves, but pale and small. The surface of the largest did not exceed 2 square centimetres. Some flowers had begun to be developed. The height of the stem, from the neck of the root, was 14 centimetres. Dried in a stove, the plant weighed 1·87 grs.

Estimation of Nitrogen in the Plant.—The entire crop was analysed; 10 cubic centimetres of normal acid were equivalent to 0·0875 grs. of nitrogen.

		cc	
Amount of acid,	Before the experiment	32·0	
	After . . .	21·4	
	Difference . . .	10·6	{ equivalent to nitrogen 0·0290 grs.

Estimation of Nitrogen in the Pummice Soil.—The dried pummice weighed 24·5 gr.; it was analysed in a single operation. Ten cubic centimetres of normal acid were equivalent to 0·0875 of nitrogen.

		cc	
Amount of acid,	Before the experiment	32·0	
	After . . .	30·8	
	Difference . .	1·2	equivalent to nitrogen 0·0033 grs.

Estimation of the Nitrogen in the matter of the Pot.—The pot dried and pulverised weighed 120 grs. Two operations were made, 40 grs. of matter being employed each time. The same normal acid was used.

		cc	
Amount of acid,	Before the experiment	32·0	
	After . . .	31·6	
	Difference . .	00·4	equivalent to nitrogen 0·0011 grs.
For the 40 grs. of matter remaining, . . .			0·0006 „
In the 120 grs. of matter			0·0017 „

Summary of the first experiment.

In the plant	nitrogen .	0·0290 grs.
In the soil	„ .	0·0033 „
In the vessel	„ .	0·0017 „
In the crop		0·0340 „
In the seed, weighing 0·780 grs. . . .		0·0349 „
Loss during the culture		0·0009 „

Conclusion.—No nitrogen was fixed during the vegetation.

§ 4.—Second Series, 1852.

Estimation of the Nitrogen of the Seeds.—Flageolet beans gathered in 1851.

Summary of the analyses.

	Exp. I.	II.	III.	Mean.
Nitrogen per cent. . .	3·943	3·664	4·290	3·97

Vegetation of a bean during three months.

First experiment.—A flageolet bean weighing 0·530 grs., which should contain 0·0210 of nitrogen, was planted on the 10th of May in pummice soil, containing ashes of dung, and the ash resulting from the seeds. The pot was placed in the apparatus A.

June 6.—The plant was vigorous.

„ 12.—The vegetation was beautiful, although the leaves were paler and shorter than those of beans grown in the open air. It was proved that the confined air contained 5 per cent. of carbonic acid.

June 28.—The stem was strong. Independently of the seminal leaves, which had attained to great development, there were six normal leaves.

July 11.—The heat having become very great, the screen which covered the bell-glass was removed only at five o'clock P.M. The plant bore twelve leaves in good condition, although rather pale, and many nascent leaves.

Aug. 6.—The experiment was concluded. The dried plant weighed 0·90 grs.

Estimation of Nitrogen in the Crop.—Ten cubic centimetres of normal acid equal 0·0875 grs.

		cc.	
Amount of acid,	Before the experiment	33·6	
	After . . .	26·85	
	Difference . .	6·75	equivalent to nitrogen 0·0176 grs.

Estimation of the Nitrogen of the Soil.—The entire soil, which, when dried, weighed 39 grs.

		cc.	
Amount of acid,	Before the experiment	33·4	
	After . . .	33·3	
	Difference . .	·1	equivalent to nitrogen 0·0003 grs.

Estimation of Nitrogen in the material of the Pot.—The pot weighed 140 grs. Two separate portions of 35 grs. each were analysed.

		cc.	
Amount of acid,	Before the experiment	33·4	
	After . . .	33·2	
	Difference . . .	·2	equivalent to nitrogen 0·0005 grs.
For the 70 grs. of matter not analysed . .			0·0005 „
Total nitrogen in the pot			0·0010 „

Summary of the first experiment.

In the plant gathered . . .	nitrogen .	0·0176 grs.
In the soil	„ .	0·0003 „
In the pot	„ .	0·0010 „
Total nitrogen in the crop		0·0189 „
In the seed, weighing 0·530 gr. . . .		0·0210 „
Loss of nitrogen during the growth . . .		0·0021 „

Conclusion.—There was no nitrogen fixed during the growth.

Vegetation of a bean for three months: flowering.

Second experiment.—A flageolet bean, weighing 0·618 grs., and

which should contain 0·0245 grs. of nitrogen, was placed in the condition described in the first experiment. The pot containing the seed was confined in an apparatus on the 11th of May.

Aug. 6.—The flowers had blown: they were of scarcely one-third the size of the flowers of beans grown in a fully manured soil. As they must have speedily fallen, I terminated the experiment.

The plant, dried at a gentle temperature, weighed 1·13 grs.

Summary of the second experiment.

In the gathered plant . . .	nitrogen .	0·0191 grs.
In the soil	„ .	0·0029 „
In the pot	„ .	0·0006 „
Total nitrogen in the crop		0·0226 „
In the seed, weighing 0·618		0·0245 „
Loss of nitrogen during the culture . . .		0·0019 grs.

Conclusion.—No nitrogen had been fixed during the vegetation.

§ 5.—Third Series, 1853.

In this new series of experiments, I modified the apparatus in which the plants were developed. A fortunate circumstance having enabled me to arrange flasks of white glass, of the capacity of 70 to 80 litres, I proceeded as follows:—

The bruised pummice, freed from the finer particles, washed, heated to redness, and cooled under a large bell-glass, in presence of sulphuric acid, received an addition of ashes of dung and ashes resulting from seeds similar to those experimented on. It was then moistened with water, *free from ammonia*, and the mixture was introduced into a large flask, B.

The moistened pummice, in falling, arranged itself in a heap.

The aperture of the flask was immediately closed with a cork, which was covered with a cap of caoutchouc. Forty-eight hours after, the cork was removed for the purpose of adding pure water, so as to bathe the base of the pummice. The seed was now planted by means of a glass-tube, in which it slipped to the spot where it was desired to place it. The seed being introduced, the flask was again closed, and when the germination was sufficiently advanced, the confined atmosphere was charged with carbonic acid gas. For this purpose, there was substituted for the cork a flask of scarcely one-tenth the capacity of the large flask: this flask was full of pure carbonic acid gas. Its neck, turned back, passed through a cork cemented with sealing-wax on its upper and lower surfaces; the luting was effected with the same wax, and, for greater security, a conical sheath of caoutchouc was applied, which effectually connected the necks of the large and small flasks. The caoutchouc was surrounded by a long strip of white linen, in order to give it resistance, and to protect it from the action of the sun.

Supposing the large flask to be of the capacity of 80 litres, the small

one should be of that of 6 or 7 litres; we should then have an atmosphere of 86 or 87 litres, in which there would be from 7 to 8 per cent. in volume of carbonic acid gas, or from 12 to 14 grs. In order to give to the apparatus a stability which would enable it to resist the action of the wind, the apparatus was sunk in the ground of the garden to the depth of 1½ decimetre; this is also very favorable to the vegetation, because the roots are not nearly so much heated by the sun as when the apparatus is entirely out of the earth.

The advantages of the new arrangements adopted in this third series of researches are evident; for supposing, as is probable, that it is impossible to completely free from ammonia, or dust of organic nature, the water, soil, and air employed, the causes of error remain limited to what they are at the commencement of the experiment, since no further quantities of these agents are introduced. It is no longer necessary to replace the water which would have been dissipated by evaporation, the vegetation is accomplished in the same atmosphere in which the seed germinated, and in a permeable soil, which is always moist, although it is in the condition of a drained soil.

When an experiment is terminated, the plant is withdrawn from the flask, by means of a thick brass wire, having at the extremity a fork, the teeth of which are fixed under the joints of the petioles. The pummice is then turned into a large porcelain capsule, and, after having removed as quickly as possible the debris of the plant which are often found mixed with it, it is dried preparatory to the estimation of the nitrogen.

I have arranged several apparatus in conformity with the directions which I have just given: the larger were from 7 to 90 litres, the smaller from 10 to 30 litres in capacity.

In the experiments made in 1853, I devoted my attention, except in two special cases, to examining the plants when they were in their full vigor, that is to say, before any of the normal leaves were detached; the fall always happens at a certain period, although the vegetation continues with activity, since the leaves which fall are very soon replaced by new ones. I acted thus, in order to avoid the action which must necessarily be exerted by vegetable remains in contact with a humid soil and the atmosphere, an action resembling that of manures, and which I thought I ought to study separately. It is true that by remaining within this limit, the experiment is of shorter duration, but the vegetation is nevertheless sufficiently long in duration for the assimilation of the nitrogen to be clearly manifested, in the case in which it would have taken place.

(To be continued.)

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

On the BEHAVIOR *of* ALCOHOL *in the* ANIMAL ORGANISM.

By Dr. DUCHECK.

THERE are now six kinds of alcohol well known, the æthyle, methyle, amyle, and acryle alcohols, the cetyle and the glyceryle. To each of

these alcohols, possessing the general formula ($C_nH_x + O + HO$), there is a corresponding aldehyde (($C_nH_x—H^2$) + O + HO) which differs from the alcohol by possessing two equiv. less of hydrogen; and also a group of characteristic acids having the formula ($C_nH_x—H^2 + O^2$ + HO). Each of these acids contains two equiv. more of oxygen than its corresponding aldehyde. To the æthyle-alcohol belong the acetal-aldehyde and the acetic acid; to the methyle-alcohol, the formic aldehyde and the formic acid; to the amyle-alcohol, the amyle aldehyde and the valerianic acid, &c., &c.

In order to change the alcohol into aldehyde, two equivalents of oxygen are absorbed, forming two equivalents of water, then the aldehyd again absorbs two equivalents more, forming water and acetic acid.

Alcohol $C^4H^6O^2 + O^2 = 2HO + C^4H^4O^2$ aldehyde.
Aldehyde $C^4H^4O^2 + O^2 = 2HO + C^4H^2O^2$ acetic acid.

The process of oxidation, however, does not end here:

Acetic acid $C^4H^2O^2 + O = HO + 2C^2HO^2$ formic acid.
Formic acid $C^2HO^2 + O = HO + C^2O^2$ oxalic acid.
Oxalic acid $C^2O^2 + O = 2C\,O^2$, carbonic acid.

It is apparent from this, that the process of the transformations of alcohol takes place by the continued absorption of oxygen, whereby finally water and carbonic acid are formed. The process of oxidation in the other kinds of alcohol is the same as in the æthyle alcohol.

The author, by means of a stomach pump, introduced into the stomachs of three dogs large quantities of absolute alcohol (from half ℥ to 60 grms), partly pure and partly diluted; he also injected into the rectum of a fourth dog 1½ ℥ of fusel oil diluted with water. The phenomena exhibited in all the four cases were:—

1. Intoxication more or less strong, and death sooner or later, according to the quantity of alcohol introduced, and to the slow or rapid manner of administration. 2. These phenomena occurred in the same manner whether the alcohol had been introduced into the stomach or the rectum. 3. The blood appeared somewhat darker, was of an alkaline reaction, and was distinguished by an odor like aldehyde, that was diffused through all the organs. 4. In two cases aldehyde was detected in the blood; while 5th, no further product of oxidation of the same was found. 6. Alcohol was not once detected in the blood. 7. The stomach, even a short time after the introduction of alcohol, contained only a very small amount of it. 8. The urine, as well as the fluid in the cerebral ventricles, exhibited a peculiar but evanescent odor of ether. 9. No important anatomical change existed in any of the organs. 10. The action of amyle-alcohol appeared far more decided than that of the æthyle-alcohol. From these facts it follows: that the alcohol becomes absorbed and chemically changed, and that these two actions occur so rapidly, that even after a very short time we can only detect the aldehyde. The impossibility of detecting alcohol in the blood is opposed to the prevailing view, that alcohol in an unchanged atomic combination remains for a short time in the blood. This view rests upon the observation that,

after the use of a large quantity of spirituous liquors, the breath is charged with a kind of alcoholic odor, or rather that peculiar to the drink that has been taken; and also that in the opening of the bodies of those who have suddenly died, an odor of alcohol is diffused from the inner organs. These cases, the author considers, are all founded upon incorrect observation. In dissection the odor of alcohol is confounded with that of aldehyde, and as for the breath, it carries along with it, besides the peculiar pulmonary exhalations, that portion of the liquor that remains in the form of vapor in the cavities of the mouth and throat. Other substances besides aldehyde are contained in the breath, and exhaled undecomposed at the same time, such as the peculiar aromas of the various liquors, which can be distinguished from the pulmonary exhalations. They originate from the various substances associated with the alcohol, viz., the different kinds of ether; the œnanthic ether, (from wine and cognac), the butyric ether, from rum, and also ethereal oils, various acids, &c. The odors of these substances are generally characteristic of the specific drink, and they are identified with that of alcohol, and it is then assumed that this is exhaled, and therefore remains undecomposed in the blood. If it be granted that alcohol is oxydised in the system into aldehyde, then it must be admitted that this oxidation takes place either in the stomach or in the blood. Oxygen is, in every case, necessary to the transformation of the alcohol; in this respect, therefore, the formation of aldehyde meets with no impediment in these places. The change, however, cannot take place in the stomach, since, 1st, we can never detect the presence of aldehyde in it; and 2nd, since the conditions requisite to the formation of aldehyde in the stomach are the same as in atmospheric air, the change of the alcohol in the stomach taking place slowly, as when exposing a large amount of it to the atmosphere, so that intoxication would occur much later. This view will finally appear untenable, if we but consider that intoxication, consequently the formation of aldehyde, occurs equally if the alcohol be introduced into the rectum, whose cavity, as is well known, contains no oxygen. If now the alcohol remains unchanged in the stomach, its transformation must necessarily take place in the blood. The question arises, how does the alcohol enter the blood? Many experiments which the author made of the penetrability of the membranes (hog's bladder) to alcohol and aldehyde, place it beyond doubt that the walls of the stomach can be penetrated by the alcohol, and thus be taken up by the vessels. Another question now arises; does the transformation of the alcohol take place after it has been carried unchanged to a remote part of the body, or immediately after its absorption by the blood vessels? Several facts are opposed to the first supposition. Whenever alcohol in great amount comes in contact with blood, coagulation necessarily occurs. This is proved by the daily experience and unsuccessful attempts of earlier experimenters, who have studied its action whilst injecting alcohol into the veins. Such coagulation will then occur if, as in the first three experiments of the author, a great quantity of alcohol penetrates through the walls of the stomach with-

out oxidising, that is to say, without becoming changed into aldehyde. And yet in the bodies of men or animals that have suddenly died from intoxication, there has never been found any trace of coagulation, that had taken place during life. Experiments shew that only a very large amount of aldehyde, introduced rapidly and in an undiluted condition into the veins, can produce coagulation; that this, therefore, never takes place when introduced slowly in small quantities and tolerably diluted. From this, therefore, we conclude, that alcohol only after it has been changed into aldehyde can remain any length of time in the blood. Besides the rapid penetration of aldehyde into all the tissues even in one or two minutes, can be more readily explained by its greater capacity of imbibition, than by assuming the same of alcohol, and supposing the change into aldehyde to take place only after the imbibition of the former into the organs.

These reasons and the impossibility of chemically detecting alcohol in the blood, make it probable that the alcohol enters only the absorbent vessels, and that on the appearance of even the smallest amount in the interior of these, oxidation immediately follows. This view of the rapid formation of aldehyde is supported by the large surface and fine porous condition of the walls of the stomach. Since then it is assumed that intoxication is not produced by the action of alcohol upon the stomach, it must, therefore, commence with the formation of aldehyde, and this consequently must be the intoxicating principle. The author to prove this, injected into the veins of three, and into the stomach of two dogs, aldehyde partly diluted and partly undiluted. He obtained these results: 1. That aldehyde introduced into the venous blood or stomach, produces the same violent symptoms of intoxication as if alcohol were used. 2. The sudden introduction of a large amount of aldehyde into a blood-vessel, produced mechanical coagulation of the blood. 3. After the passing off of the narcotic effect, acetic and oxalic acids were found in the blood; from this it appears that aldehyde is fitted for excretion by the absorption of oxygen. 4. If there is at any one moment a large amount of aldehyde in the blood, a portion of it can be thrown off unchanged, with the exhalations from the lungs. Hence arises the odor of the breath, that has been confounded with that of alcohol. This symptom only appeared when a great amount of aldehyde had been rapidly injected. The lengthened respiration indicated the necessity for oxygen, the animal heat appeared to keep parallel with the increased process of combustion.

If it now be granted that intoxication commences with the formation of aldehyde, it may be asked whether it ends with its disappearance from the blood, or whether the symptoms of intoxication are connected with the presence of the further process of oxidation, acetic acid, &c. The author's experiments shew that acetic acid is never present in the blood of animals that have died during intoxication, although it has been found after this has passed off. Besides, the author made the direct experiment of injecting dilute acetic acid into the veins of a dog—no narcotic effects followed. These facts lead to

the conclusion that the period of intoxication is limited to the disappearance of the aldehyde, that is to say, to its conversion into acetic acid. Consequently the above-stated view, that intoxication is due properly only to the aldehyde receives further confirmation.

In two experiments oxalic acid was found in the blood, as a further product of the oxidation of alcohol. The ultimate products of this continued oxidation must of course be carbonic acid and water. For the determination of carbonic acid and water, in the pulmonary exhalations, after the use of alcohol, the author made three experiments upon dogs, in which at the same time, the amount of expired air, the animal heat, the number of respirations, and arterial pulsations were taken into account. He obtained the following results:—1. After the use of alcohol more air is inhaled, indicating an increased necessity for oxygen. 2. In the expired air is found (with equal amount and equal number of inspirations) *less carbonic acid and less water* in the pulmonary exhalations. (A result which Vierordt and Böcker also obtained). 3. The number of respirations and arterial pulsations, together with the animal heat, and, consequently, the process of oxidation, were greatly augmented.

When the process of alcoholic combustion by the continued absorption of oxygen takes place, the blood is in this way deprived of a great amount of oxygen, which would otherwise be applied to the combustion of other substances. The blood therefore must undergo a change, and the normal course of metamorphoses be impeded; and the quantity of the final products—carbonic acid and water—also must vary. In the normal condition (besides other substances) grape sugar is consumed in the blood, and its transformations into carbonic acid and water depend upon oxidation, a process of great importance to respiration and the generation of heat. If a substance, though, like aldehyde, enters the blood, making energetic demands upon oxygen, and generally having a greater affinity for it than sugar has, then the *latter must in greater or less amount remain unconsumed*, at least for a time. That this actually takes place, the author proves by his investigations of cases of sudden death by alcohol and aldehyde, as well as by long-continued use of alcohol, in which cases, he always found a large quantity of sugar in the blood, and, indeed, larger than in the blood of two dogs drawn in the course of the experiment, *before* the first intoxication. If alcohol ($C^4H^6O^2$) is burnt, the proportion of carbonic acid to water is as 4 : 6 or 1 : 1½; in the combustion of sugar ($C^{12}H^{12}O^6$) as 1 : 1. In the first case, therefore, there is more water and less carbonic acid formed. If now, whilst using alcohol, it alone is consumed, the sugar remaining unchanged in the blood, then the proportion of carbonic acid to water that is expired, is changed, and this is the reason why *after the use of alcohol* less carbonic acid is expired. Concerning the second product of combustion—water—in spite of its outweighing the carbonic acid, its amounts are very inconsiderable. Since now the kidneys are especially designed for the secretion of water, then it is evident why its amount is greatly decreased in the exhalations from the lungs. That a great part of the water passes off into the urine is

shewn by the great increase of the latter after the use of spirituous liquors, as well as by the peculiar kind of ethereal odor in it, which can be produced, though only by the secretion of a yet unknown secondary product of the alcoholic combustion, which with the water is carried off also by the kidneys. The question now arises :—During the general combustion of aldehyde, what is the behavior of the remaining blood in the body, whose metamorphosis into carbonic acid and water are so retarded? The retarding of oxidation affects principally the sugar. If this is not consumed, it must finally either be accumulated in the blood, or be ejected as such (diabetes) with the excrement, or it must undergo some other kind of transformation. We cannot imagine an accumulation of sugar to remain for any length of time in the blood ; the author never found it in the urine of any animals experimented upon; it must undergo consequently some other transformation. In relation to the appearance of milituria, the author begged attention to the fact, that many had declared that diabetes was frequently found among drunkards, and considered it possible that, in such cases, it was induced by the above mentioned causes.

The further metamorphosis of sugar is, however, most important. If it is not transformed into carbonic acid and water, it must change into fat, to which probably the excess of oxygen breathed, is applied. Herein doubtless lies the cause of the excessive formation of fat among drunkards.

To what extent the transformation of the nitrogenous substances suffers from the superior oxidation of aldehyde in the blood is unknown ; it may, however, be analogous to what has been previously stated in regard to sugar—the retarding of the metamorphosis, or what is more probable, a change in the nature of the constituents. The principal action of aldehyde upon the blood is generally, therefore, the withdrawal of its oxygen. Its existence is, however, not always indicated by this, but by the rapid action of the processes ; the alcohol rapidly oxidising into aldehyde, and this more readily than other substances, withdrawing rapidly a large amount of oxygen. This probably soon becomes balanced by the increased respiration from the necessity for oxygen ; as was observed by the author in his experiments, and as is known in life. During intoxication purer air is requisite ; therefore in cold climates, or in winter, a larger amount of alcohol can be taken than in warm climates, or in summer. It seems therefore that the rapid absorption of oxygen performs here another part. Those bodies which act in the same or somewhat similar manner, which either absorb oxygen rapidly, or impede its access to other substances, exhibit similar actions. The breathing of carbonic acid and of hydrogen gas produces insensibility and sleep. The intoxicating effect of protoxide of nitrogen is known. The action of opium, on the contrary, can not be explained in the same manner. In opposition to these subatances, bodies though of like or similar combination, which are incapable of being oxidised, shew no action analogous to that of alcohol.

Of the alcohols, the æthyle, methyle, and amyle-alcohols alone act si-

milarly, possessing a like affinity for oxygen. The æthal, a kind of fatty substance, which oxidises slowly, acts on the contrary, indifferently on the organism, as the author proves by experiments. Acetic acid, as above shewn, is also wanting in that intoxicating principle, though it must finally be removed from the system by further oxidation. It is therefore not improbable that the rapid withdrawal or the great want of oxygen is essentially the fundamental cause of *most* if not all the phenomena of intoxication. The trembling, the transient paralysis of the lower extremities —which we know to be the case with men, and which were not wanting in any of the author's experiments, cannot therefore depend immediately upon the chemical action of alcohol nor their disappearance upon the inhalation of much oxygen during intoxication, which was observed by the author in his experiments upon birds and dogs. Death, in consequence of the so-called alcoholic poison, must occur, agreeably to the above, when with the presence of a great amount of aldehyde in the blood, there exists not at the same time a sufficient amount of oxygen to transform it into acetic acid.

To establish the phenomena that occur after *long continued use of alcohol*, the author instituted several experiments upon dogs. In order to obviate the difficult application of the stomach-pump, and to prevent the fluid from getting into the windpipe from the restlessness of the animal, he used gelatine capsules, which contained each 5 grms. of absolute alcohol. To a dog two years old, rather thin, and of small species, were daily given 15 grms. of absolute alcohol, which was followed regularly by intoxication. Emaciation and weakness in the hinder limbs, occurred. Death in seventeen days and fourteen hours after the last dose. The blood contained many carbonates and much sugar. To a setter one year old, and well fed, first small, then large doses of alcohol (from 5 to 15 grms.) until intoxication occurred, were given. Here also appeared weakness in the hinder limbs—emaciation not very great. Death in forty-three days and eight hours after the last dose. A weak odor of aldehyde from the inner organs and blood. The latter contained much sugar. To a small dog, corn brandy was daily given for seventy-two days, until intoxication occurred. The animal became rapidly emaciated, to which a cancerous tumor in the glands of the breast probably contributed. The symptoms of weakness in the hinder limbs were also here apparent. The blood contained no sugar. To another young dog, corn brandy was given for thirty-two days. The same symptoms occurred. The blood contained much sugar. To a large setter, corn brandy was given daily for ninety-three days, from two to three spoonfuls. Upon this he became very fat, and was well, when on the above named day he was accidentally killed. The blood contained much sugar. There was much fat on the under skin of the cellular tissue, otherwise no anatomical change.

The anatomical dissection in the cases of sudden death by alcohol presents no change of any organ, of the brain or its membranes—in the lungs only were found a spurious œdema. Emaciation was shewn in long continued habits of intoxication ; in the brain though, no change, not even that thickening of the membranes and dropsy in the

ventricles, quoted by Huss. In two cases there was blennorrhœa of the external auditory canals. The stomach and its membranes were, in opposition to others' account, always normal. Only in one case where but little amount of brandy had been taken was much fat found. In conclusion, the author abridged his obtained results, as follows :—

1. Alcohol undergoes in the system continued combustion, of which the intermediate products are found in the blood.

2. Intoxication is dependent upon the existence of aldehyde in the blood.

3. The action of aldehyde upon the blood is the rapid withdrawal of its oxygen.

4. Therefore the combustion of other substances, consequently their metamorphoses, are prevented or retarded.—*Schmidt's Jahrbuch, Oct., and Med. Ex., (Philadelphia).*

On the SECRETION *of* UREA *considered as giving the* MEASURE *of the* METAMORPHOSES *of the* TISSUES. By M. BISCHOFF.

M. BISCHOFF, availing himself of the process of estimating urea recently proposed by Liebig, undertook a series of researches on the secretion of this principle. The following are the deductions from his numerous experiments :—

1. Urea should be regarded as the product of the metamorphoses of the tissues, and of the decomposition of the nitrogenous matters of the organism. It is never formed directly in the vascular system at the expense of the albumen of the blood ; gelatine, alone, when it enters accidentally into the torrent of the circulation, which is rare, can give rise to the formation of urea in the blood itself.

2. Although the urea results from the transformation of materials already elaborated and forming the organs, the aliments exert, however, on the secretion of this principle a more considerable influence than is at present supposed. Indeed, the secretion of urea continues during abstinence, but the quantity of urea secreted diminishes considerably in these conditions. Thus, a dog, which had devoured in twenty-four hours 4,000 grms. of beef, without fat or bone, secreted during that period 190 grms. of urea, whilst he eliminated only from 6 to 8 grms. under the influence of a poorer diet (500 grms. of potatoes and 950 grms. of fat).

3. The nitrogen contained in the urea eliminated during a certain time, never represents the total quantity of nitrogen which the aliments contained ; a portion of this principle is therefore eliminated from the organism under another form. In the dog submitted to these experiments, the aliments contained an excess of nitrogen. His urine did not contain uric acid, and only traces of nitrogenous matter besides urea ; and his excrements contained only very small quantities of nitrogen. It is then difficult to point out under what form the excess of nitrogen has been eliminated. It is very probable that a portion of the urea was transformed in the blood, or perhaps in the bladder, into carbonate of ammonia, which was eliminated either by the skin and lungs, or by the urine.

The proportion of nitrogen which is not found in the urine is variable; it attained its maximum in the case of insufficient nitrogenous nourishment (250 grms. of meat), in which it exceeded two-thirds of the total quantity of nitrogen contained in the aliments. In normal conditions, when the nitrogenous alimentation is sufficient for maintaining the weight of the body constant (500 grms. of meat), the excess of nitrogen, which is not found in the urine, is only one-third of the total quantity of nitrogen. It diminishes considerably, and is reduced almost to nothing with a superabundant nitrogenous diet; in this case, consequently, the nitrogen contained in the urea represents very nearly all that was contained in the food.

4. The quantity of urea eliminated is in more or less direct ratio with the quantity of urine excreted. Although the density of the urine may vary, and although a dense urine is generally rich in urea, it is no less true that a large quantity of urine carries away a considerable quantity of urea.

5. Common salt increases the proportion of urea excreted. A dog, which consumed daily a pound of meat, and which secreted on the average, under the influence of this regimen, 22·50 grs. of urea, eliminated 28·34 grs. when about 12 grms. of salt were added to his ordinary ration.

Annalen der Chemie und Pharmacie.

VEGETABLE CHEMISTRY.

NOTE *on the* CARBONATE *of* LIME PRE-EXISTING *in the* NORMAL STATE *in* PLANTS, *and on its* ESTIMATION. By M. PAYEN.

IN their researches on the mineral substances which vegetables contain, Fourcroy and Vauquelin having detected the presence of various salts decomposable by heat, thought that they might draw from this general fact the following conclusion:—

"The lime, or carbonate of lime found in vegetable ashes, is never contained in this state in the plants, but in that of salts which are decomposed by burning and reduced to their more or less carbonated base."*

From the period at which these illustrious men of science wrote, to the present time, almost all chemists have admitted the rule thus laid down, even in its absolute terms.

We may readily understand how it has been thus, when we consider, as many have said, that plants present in their entire state, juices with an acid reaction: now these liquids having the property of attacking and decomposing the carbonates, it was denied that the latter could pre-exist in such conditions.

This view, moreover, was not at variance with the results either of the analyses of vegetable ashes made by Theodore de Saussure, or with the numerous comparative analyses by means of which M. Berthier

* *Annales de Muséum d'Histoire Naturelle*, t. xiii, p. 1.

demonstrated the especial aptitudes of certain species and the localization of mineral substances in larger proportions in certain organs of vegetables.*

However, an attentive examination under the microscope aided by chemical reactions, enabled me to recognize and to publish, in 1840, a great number of facts antagonistic to the principles laid down in conformity with that which occurs in the chemical reactions between bodies in contact; and to prove, on many occasions, the different or opposite state of the liquids in two contiguous cellules of vegetable tissues—neutral or alkaline in one, and acid in the other, although their delicate membranes were perceptibly permeable.

Thus all the glands surrounding the leaves and branches of the Ice-plant (*Mesembrianthemum cristallinum*) are filled with a white, limpid solution of oxalate of potassa or soda, with a distinct alkaline reaction, whilst the juices secreted in the subjacent tissues present a manifest acidity.†

That several species of *Chara* secrete in light, peripheric tissues arranged in the form of a helix around their tubular organs, abundant concretions of carbonate of lime; whilst other species, as the *Chara translucens*, vegetating in the same waters, do not shew such secretions.

That in the leaves of four species of Ficus,‡ in which Meyen had perceived globular bodies developed around a pedicle, and which he regarded as formed of a gummy mass, superficially covered with carbonate of lime, I detected the more complex structure and the composition of those small organs, presenting a slight tissue, formed of minute cellules, quite filled with carbonate of lime.

We understand that the mineral secretion, being covered with a thin membrane, these bags being swelled up with mineral particles, form prominent papillæ in the last range of the cellules of these small globular bodies, which present a profile surrounded by denticulations.

I have found, in such conditions, concretions of carbonate of lime filling a special tissue developed around a pedicle of cellulose, with some variations of forms in the leaves of all the species of Ficus, to the number of eighteen, which I have examined.§

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* I have myself proved that the salt most diffused in plants is the oxalate of lime, giving the carbonate by incineration. We observe, under the microscope, this salt under all the different crystalline forms of *raphides*, short and longer prisms, rhombohedra, agglomerations of needles arranged in a parallel manner among themselves and in great number in the same cellule, and, finally, other agglomerations of rhombohedra or prisms radiating from a common centre, truncated or terminated by pyramids. Potassa and lime are found between the cellules of various tissues.

† See the fifth Memoir on the Developments of Vegetables (mineral concretions and incrustations); by M. Payen, t. ix of the "*Savants étrangers*," p. 77; and "*Annales des Sciences Naturelles*," t. xvi (Botanique); 1841, p. 321.

Guy-Lussac indicated the presence of soluble oxalates in the ice-plant and De Candolle noticed the alkaline character of the liquid of the glands without proving the acidity of the juices in the subjacent tissues.

‡ *Ficus elastica, Bengalensis, pisiformis and clusiæfolia.*

§ Especially, besides the above-mentioned *Ficus*, the *F. ferruginea, F. nymphæifolia, F. carica, F. laurifolia, F. Neumani rigida, F. religiosa, F. montana*, (the con-

I have proved the presence of analagous concretions of carbonate of lime in the leaves of other vegetables of the various tribes of the family of *Urticeæ;* especially in the *Celtis,* the *Conocephalus nauclei florus;* in mulberry trees, Pareitaria (*P. officinalis*), the Orties (*Urtica nivæa*), *Broussonetia papyrifera,* the hop, hemp, &c.

Lime is also met with in the state of carbonate in the fruits of several Celtis (*C. orientalis, C. occidentalis, C. australis, C. cordata*).

Here, the calcareous secretion is found contained in the cellules of the tissue of the stones: these receive from the mineral substance so hard a consistence, that we cannot cut them without notching the knife.

After the solution of the carbonate of lime with acetic acid, or dilute hydrochloric acid, the stones, which retain their consistency, may be cut without difficulty.

Then, the special tissue in which the calcareous substance was deposited, shews, by thin layers seen through the microscope, a very fine net-work fixed in the interior of each cellule, the sides of which it thickens very much: iodine tinges this special tissue of an orange-color.

The tint, as well as the forms, remains on many points when the addition of a drop of concentrated sulphuric acid, disaggregating the sides of the cellules themselves, manifests in the latter the distinctive character of pure cellulose by a rich violet coloration, which, by degrees, is effaced, leaving only orange or brown particles, the last vestiges of the nitrogenous organic substances. In this example also, the calcareous secretions is effected and persists in the tissue of the kernels in the presence of the juices, with an acid reaction contained in the fleshy pericarp of the fruit.

The globular kernel which occupies almost the entire cavity of the stone, is abundant in oily secretion; I extracted from it with ether 0·48 of its weight in the dry state. This oil, which is yellowish, remains fluid at the temperature of 68° F.

From these various observations, which may easily be repeated by causing some chemical agents to react under the microscope, it might still be thought that the presence of carbonates pre-existing in vegetables could only be proved with the assistance of the microscope, and that, besides, their ponderal proportions were so small that it would be impossible to determine them experimentally; that thus, to a certain extent, we were freed from the necessity of taking them into account in chemical analyses.

It was with the desire of removing this doubt that I undertook to determine directly the ponderal quantities of carbonate of lime contained in some organisms of plants, and to define more completely the structure of these organisms belonging to the fruits of the Celtis, as well as the proximate composition of their various parts.

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cretions of this last were found in the short hairs of the inferior face), *F. scandens, F. citrifolia, F. glaucescens,* and three other kinds, not named, in the hot-houses of the *Muséum d'Histoire Naturelle.*

At the period at which I commenced these experiments most of the leaves had fallen; however, I was able to collect in a garden of Grenelle, leaves of *Broussonetia papyrifera*, and of black mulberry, partially dried on the trees. The desiccation was completed with care; that is to say, slowly, and at a gradually increased temperature, so as to avoid all infiltration of acid juices which might have come in contact with the calcareous concretions; I found, moreover, leaves dried in these suitable conditions, such as M. Peligot had prepared for his beautiful work on the alimentation and products of the silk-worm.

These various leaves, completely dried, were finely powdered and sifted. Each of the samples weighed was put into a flask communicating with an apparatus for collecting and estimating carbonic acid; it was easy to deduce the equivalent weight of the carbonate pre-existing in the leaves.

The following are the results of this experiment on the leaves of the black mulberry, of the *Broussonetia papyrifera*, and of several white mulberries. The results would have been different if young leaves had been employed, for then the calcareous substance might not yet be secreted in the tissues intended for its reception, and even these tissues might not be completely formed.

ESTIMATION OF CARBONATE OF LIME PRE-EXISTING IN PLANTS.*

	Dried leaves employed.	Weight.	Carbonic acid obtained.	Carbonic acid per cent.	Equivalent in carbonate of lime.
		gr.	gr.		
1	*Broussonetia papyrifera*, autumn leaves	6·21	0·025	0·40	0·90
2	Black mulberry, autumn leaves .	9·750	0·100	1·01	2·27
	Id *Id* .	37·800	0·415	1·09	2·30
3	White mulberry, summer leaves .	25·000	0·045	0·18	0 41
	Id *Id* .	25·000	0·050	0·20	0·45

It will be remarked, in glancing at this table, that the proportion of normal carbonate of lime, equivalent to the carbonic acid disengaged, amounts to from 4 to 23 per 1000 of the weight of dry leaves employed, or from 4 to 20 per cent. of the total quantity of calcareous salts contained in the same leaves. This ponderal determination

* MM. Poinsot and Wood have assisted me in these determinations. We wished to verify, by an approximative estimation, the results relative to the white mulberry, by collecting into tris-acetate of lead the carbonic acid from 50 grms of leaves. The carbonic acid obtained in this form weighed 0·07, representing 0·14 of carbonic acid, or 0·32 of carbonate of lime, which agrees with the above results, taking into account the loss of carbonic acid in this last assay.

agrees, then, with the observation under the microscope in demonstrating the presence of normal carbonate of lime in the tissues of several vegetables.

A still more easy demonstration of this principle may be given, by submitting to the same mode of testing the stones of the fruits of several kinds of *Celtis*, entirely freed from the acidulous fleshy pulp which surrounds them, either entire, or after the removal of the oleaginous kernel which they contain by carefully breaking them in a vice.

Being then almost entirely composed of a vegetable tissue filled with calcareous particles, the fragments of these stones, placed under the influence of hydrochloric acid, diluted with from 5 to 10 volumes of water, disengaged without the aid of heat the whole of the carbonic acid.

100 parts of these fragments of the *Celtis orientalis*, dried, gave 27 of carbonic acid, equivalent to 60 of carbonate of lime ;* 100 of similar fragments, resulting from the *Celtis cordata*, disengaged, under the same influence, 28·1 of carbonic acid, equal to 63 per cent. of carbonate of lime.

The tissues of the stones thus freed from carbonate of lime still yielded to pure hydrochloric acid (HCl, 6HO) 1 hundredth of its weight of carbonate, or 2·75 per cent. of the total weight of encrusted shells.

In order to determine the state of the silica in these tissues, I incinerated slices of the stones previously freed from carbonate and phosphate of lime, and I was able to discover then, under the microscope, that the mineral substance (silica or silicic-acid), undissolved, belonged to the fine tissue developed in the interior of each cellule, the free cavity of which was thus reduced to about one-third of the total diameter, the sides being thickened by this slight tissue, the minute cells of which are filled with calcareous particles.

It is perhaps worthy of remark, that this tissue, intended to contain the mineral secretion, is composed, like the epidermis of vegetables or its external pellicle, the epidermic cuticle, of cellulose impregnated with both silica and nitrogenous substance.

The following are the results of the anatomy and of the proximate analyses of the fruits of the Celtis :—

In the Celtis Orientalis.

The fleshy pericarp forms per cent.		71·70
The stone 28·5, of which	the shell	17·81
	the kernel	10·49
		100·00

The proportions of water were, in the pericarp, 0·583 ; in the shell of the stone, 0·0616 ; and in the kernel, 0·166.

* The kernels of the same fruits gave only 0.0364 of ashes formed in great part of soluble salts. The ashes of these kernels, put in contact with water, communicated to it an acid reaction.

The stones of 100 fruit weighed 10·444 gr., namely :—

Shells	7·160
Kernels	3·284

Analysis and Anatomy made comparatively on 100 *parts by weight of these stones.*

Shells, — 67·30 =	Organic tissue (cellulose and nitrogenous matter)	22·90
	Silica (in the special tissue) . . .	4·40
	Carbonate and traces of phosphate of lime and magnesia	40·00
Kernels,—32·70 =	Organic tissue	16·30
	Fluid oil	15·20
	Mineral substances	1·20
		100·00

In the proximate analysis of the stones (dried shells) of *Celtis cordata*, rather more carbonate of lime was obtained; there were only very slight differences as to the other results. The following are the numbers of this analysis :—

Organic substances (cellulose and nitrogenous matter)	28·723
Carbonate of lime	64·234
Phosphate of lime and magnesia	traces
Silica	7·043
	100·000

The quantity of carbonic acid produced by the carbonate of lime secreted in the internal tissue of the stones of the *Celtis* is so abundant, that the experiment may be repeated at a public lecture, and a numerous audience may witness the demonstration of this curious phenomenon.

Indeed, if we place in a test-tube twenty of these small stones freed from pulp and well-washed,* and if we pour on them two or three times their weight of water, acidulated with 0·1 of hydrochloric acid, a brisk effervescence is very soon produced; and if the tube has been closed with a cork, the latter will very soon be expelled with a slight explosion, by the carbonic acid gas : the effervescence continues for eight or ten hours.

The demonstration will be completed by manifesting, with the appropriate reagents, the presence of chloride of calcium in the liquid, which contains only traces of other mineral substances, especially phosphate of lime.†

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* This is easily done by rubbing these fruits strongly, in water, with a rough cloth, and renewing the water several times. The cleansing will be more complete if some sand be placed with the fruits, and likewise renewed several times.

† The *Chara flexilis*, the only one which I have been able to procure at this season, also gave directly a ponderable quantity of carbonic acid. In the fresh state it contained 80 per cent. of water; 100 parts of the dried substance gave 7·9 of carbonic acid, equivalent to 17·95 of carbonate of lime.

Every one, I think, after having witnessed these experiments, which are so clear and so easy to repeat, will admit carbonate of lime to the number of the mineral salts which exist in certain vegetable species.

Annales de Chimie et de Physique, June, 1854.

ELECTRO-METALLURGY

On the EXTRACTION *of* METALS *by Means of the* BATTERY.

By Professor BUNSEN.

IN the course of his researches on the electrolysis of metallic combinations, Professor Bunsen was led to determine the causes which most influence the separation of the metal. These causes are two in number, the principal of which is owing to what he calls the density of the current; the other cause dwells in the greater or less concentration of the electrolyte or liquid to be decomposed. The maximum of effect is obtained with the most dense fluid, and the most concentrated solution.

The word *density*, applied to a force, necessarily excludes the idea of weight and volume; Professor Bunsen means, by this word, the concentration to a single point of the electrical undulations, in a manner analogous to the concentration of luminous or calorific rays in the focus of a concave mirror.

Let us take, for example, a charcoal crucible in communication with the positive pole of the battery, and place in it a small capsule of glazed porcelain containing the liquid to be decomposed; the space between the crucible and the capsule is filled with hydrochloric acid, and the liquid of the small capsule is put in communication with the battery by means of a thin sheet or wire of platinum.* The current is then established between a large surface, the charcoal crucible and a fine platinum wire, in which it is concentrated; the effects are added in this direction, and the fluid becomes capable of overcoming affinities which have hitherto resisted powerful batteries.

The apparatus which we have just described is placed in a porcelain crucible, which is kept warm in a sand-bath.

Chromium and manganium are thus separated with the greatest facility from their chloruretted solutions, provided that the negative pole is small, and the saline solution very concentrated; if not, we may, at will, obtain hydrogen, peroxide or protoxide of chromium, or *chromoso-chromic* oxide. When the galvanic deposit is formed only of these oxides, it is sufficient to add solid protochloride of chromium to obtain metallic chromium.

In this state the chrome is chemically pure; it presents the appearance of iron, but it is less alterable by humid air. Heated in the air, it is converted into sesquioxide. It resists nitric acid even when

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* For this purpose, the platinum wire must be exactly in the centre of the crucible; if not, by virtue of its tendency to take the shortest road, the current is established in preference between the nearest points.

boiling; hydrochloric and aqueous sulphuric acids act on it, forming a protosalt. The density of galvanic chromium coincides with the density deduced from the atomic volumes, and does not differ much from the known density. These facts regard only the metal prepared with one of the modifications of chloride of chromium; they leave completely undecided the question as to whether the chromium of the green chloride is identical with that of the blue. Professor Bunsen intends to study this point.

On diminishing the current, the metal ceases to be deposited, and in its place appears a black powder, not crystalline, anhydrous, formed of protoxide and sesquioxide of chromium; the composition of this powder varies between the formula $2CrO + Cr^2O^3$ and $3CrO + Cr^2O^3$; it is insoluble in the acids; it is purified by boiling it in *aqua regia;* it burns vividly in the air.

Professor Bunsen obtained sheets of chromium of more than fifty square millimetres surface; these sheets were friable, and presented a perfect polish on the side which had been in contact with the platinum.

Manganium was obtained in the same manner. The learned chemist of Heidelberg thus prepared very friable plates, of more than 100 square millimetres surface; these plates were oxidised in humid air almost as readily as potassium.*

When we diminish the density of the current, we obtain black manganoso-manganic oxide, soluble in hydrochloric acid, which converts it into protochloride.

To reduce barium and calcium, a greater density of current is required. These metals are taken in the state of chloride reduced to concentrated solution, and acidulated with hydrochloric acid; the boiling liquid is poured into the polished porcelain capsule, and an amalgamated platinum wire, communicating with the battery, is introduced; calcium is deposited on the platinum wire in a grey layer, which is easily detached, and contains a little mercury. In presence of water or humid air, this amalgam of calcium oxidises rapidly with disengagement of hydrogen; it burns with brilliancy when heated.

The precipitation of the calcium is effected only with difficulty; in consequence of its oxidisability, this metal is converted into lime, which covers the electrode, and interrupts the current. To obtain an appreciable quantity of this product, we can do nothing better than frequently remove the quite dry grey layer, and amalgamate again the platinum wire before returning it into the chloride.

Barium is more easily extracted: chloride of barium, in powder, is reduced into a paste by means of water acidulated with hydrochloric acid; it is heated to 212° F. in a water-bath, and the current is established. The amalgam of barium, which is thus produced, is solid, of a silvery white, and very crystalline; exposed to humid air it becomes

* This great alterability of galvanic manganium is owing doubtless to the texture of the metal. We remember to have seen, in the laboratory of M. H. Deville, a button of metallic manganium, prepared by the igneous way, and which was perfectly preserved in an imperfectly corked flask: we may add, however, that the metal was slightly oxidised on the surface.

heated, and is converted into hydrate of baryta.* Placed in a charcoal boat, and heated in a current of hydrogen, it abandons the mercury, and the residue is composed of porous barium, presenting here and there brilliant metallic particles.

Poggendorff's Annalen.

REDUCTION *to the* METALLIC STATE *of the* ALUMINIUM *in a Piece of Disthene melted in the Electric Flame.* By M. DUVIVIER.

HAVING by chance the disposal of a reversed Bunsen's battery of 80 elements, I subjected to the electric flame disengaged from the charcoal point of one of the poles a small piece of laminæ disthene, which is not naturally very fusible; for if heated to whiteness for half-an-hour, it merely cracks and whitens, and only the very smallest asperities become fused.

This small piece of disthene, when exposed to the electric flame disengaged from a charcoal of the size of a drawing-pencil, melted entirely after two or three minutes; moreover, the elements which composed it were partly disunited and were dispersed by the power of the electric current, and the aluminium freed from its oxygen, rose to the surface of the water in a state of fusion. One small globule fixed itself quite out of the mass, becoming flattened by cooling; other globules remained in the midst of the melted matter. I removed the one which was uncovered, with a needle; it was as white as silver, and its hardness appeared to me comparable to that of pure silver.

Comptes Rendus, No. 24, June 12th, 1854.

PHYSICS.

On the BATTERY *with* TWO LIQUIDS: *on the* CHEMICAL ACTION. By M. C. DESPRETZ.

THE perusal of several Notes presented to the Academy or published in various scientific journals† might lead one to fear that the results of my experiments on the comparison of the internal with the external

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* It may be asked, and not without reason, how metals so oxidisable can be reduced in a *hot aqueous* solution, acidulated with *hydrochloric acid*. Extraordinary as these facts appear, they are not without precedent. We may remark, besides, that the reduction in question is operated under the influence of the battery, and that the metal is deposited on the pole at which the hydrogen is disengaged. Although this deposition is effected in acidulated water, the metal may be nevertheless protected against the action of the liquid by a sort of passiveness analogous to that which protects iron, nickel, and cobalt against the nascent oxygen in the galvanic circuit, and against acidulated water, in which this oxygen is produced.

† M. Meidinger ("*Annales der Chemie und Pharmacie*," t. lxxxviii., Oct., 1853).
M. Soret "*Bibliotheque Universelle de Genéve*," February, 1854.
M. Foucault "*Cosmos*," 3e vol., p. 553; 4e vol., p. 248.
M. Jamin "*Comptes Rendus*," t. xxxviii, p. 390.
M. Lablanc "*Comptes Rendus*," t. xxxviii, p. 444.
M. Matteucci "*Cosmos*," t. vi., p. 390.

working of the battery with two liquids were tainted with inaccuracy. (*See* "THE CHEMIST," 1851-2, p. 1.)

It might be thought, for example, that I have not directed my attention either to the possible combination of the two gases, or to the solubility of these gases in the acidulated water, or to ineffectual passage of a certain quantity of electricity.

The first two causes, had they had not been avoided, would alone have been sufficient to render our experiments quite defective, and, consequently, at least useless.

To shew that I did pay attention to these causes of error, and indeed to others which have not been noticed, I may detail some experiments made in 1850, when I was occupied with the work, an abstract of which I had the honor of reading to the Academy in 1851, and mentioned therein. These experiments are not, I venture to think, quite devoid of interest.

In the experiments in which I employed platinum electrodes of 95 millimetres in height, 43 or 82 millimetres broad, and placed at the distance of 9·5 millimetres from each other, I perceived that very small and very numerous bubbles of gas, even with 4 Bunsén's elements, rose only slowly in the graduated bell-glass.

In the experiments made with the electrodes of 82 millimetres in breadth, the water loses its transparency to such an extent that the flame of a taper can no longer be seen through the thickness of the column of water acidulated to 1–10th in volume of at least 60 centimetres in height, except in the extent of 1 centimetre at the upper part. The diameter of the graduated bell-glasses was about 90 millimetres.

If we replace the electrodes of 82 millimetres by electrodes of 43 millimetres, the water is still rather cloudy; but it admits of the form of the flame of a taper being seen through it.

In both cases we must wait some minutes for the water to resume its ordinary limpidness. In proportion as the gases rise in the bell-glass, the water which goes out takes with it a very considerable quantity of gas.*

I knew at that time, as did all chemists and physicists, from the old and important experiments of M. Thenard on oxygenised water, that the presence of an acid facilitates the combination of oxygen with water. I was constrained to believe that the two gases disengaged in these experiments were not strictly in the proportion in which they exist in water; but I do not dwell on this subject. The experiments which I have made on this point would have no value now, since several of the young men of science mentioned at the commencement of this note have recently treated of it.

The observation of the facts which I have just mentioned has led me to reject the large electrodes, and to substitute for them wires, likewise of platinum, of the diameter of $1{\cdot}2^{mm}$, of the height of 87^{mm}, and fixed at the distance of $6{\cdot}7^{mm}$.

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* M. Govi, a young Italian man of science, witnessed these experiments, as well as almost all those which I made at this period.

I covered these wires with mastic at their lower part, to the height of nearly 2 centimetres, in order to avoid the error occasioned by the presence of gaseous bubbles on the lower part of the graduated tubes. These bubbles, carried away by the water, are found minus in the volume measured at the end of the experiment.

The bubbles of gas, which were now larger, immediately rose in the graduated tube.

Being warned by the facts above remarked, I examined many times with the naked eye and a lens the water which left the tube, and I never perceived the smallest bubble carried away.

In all the experiments detailed in my Memoir I collected only the hydrogen gas, and always on the same wire; I wished thus to obviate the error caused, either by the recomposition of the water, or by the greater solubility of oxygen in the acidulated water.

Let us now look into the error arising from the quantity of electricity which may traverse the liquid of the voltameter, without decomposing it. It is very inconsiderable: it had not escaped me. In my eighth communication (*See* THE CHEMIST, 1851–2, p. 436), I say:—

"The slight difference in favor of the interior working, which frequently amounted to only 1–150th, or 1–200th, or even only 1–250th, must be neglected in such a subject; it may, moreover, be explained by slight derivations, by the solution of a small quantity of gas, and by the inefficacious passage of a very small portion of electricity through the voltameter."

I might have added the influence of a very small quantity which penetrates into the porous vessels.

If the results obtained by Faraday ("Experimental Researches") on fused bodies, and if the experiment in which Buff saw that the two poles of an isolated battery lose a portion of their intensity when they are connected by a column of pure water without *the liquid being decomposed* ("Archives de l'Electricité," t. i., p. 274), had not sufficiently forewarned us, we should have been by our own experiments (1849) on the electrical effects of muscular contraction, when we were only very imperfectly acquainted with the very extended work of M. du Bois-Reymond on this delicate matter. We proved at that time that electrodes of gold or platinum, give deviations in pure water with the long wire galvanometers of M. Ruhmkorff. And in this case no water is decomposed, that is to say, not the least disengagement of gas is perceived.

With regard to the inefficacious passage of electricity through water, either pure or containing a foreign substance in solution, several points present themselves to the mind for decision.

For example:—

Does electricity pass into water or into an aqueous liquid without decomposition taking place?

Does a sufficient quantity pass to affect the results of observations when the effects are rather considerable?

Will an electric current, traversing several voltameters, presenting

to this current a peculiar resistance, decompose in these several voltameters equal or unequal quantities of water?

The reply to the first question is affirmative: the above-mentioned experiment of Buff, our experiments in 1849, and others which we have made, leave no doubt on this point.

We will detail an experiment made with three voltameters: the first, into which the current entered, was full of acidulated water, composed of nine parts of pure water, and one volume of pure monohydrated sulphuric acid; the second was full of distilled water; the third full of acidulated water, containing only 1–2000th of its volume of the same acid.

The space between the platinum wires was about 12 millimetres; the current furnished by 4 Bunsen's elements charged for intensity, as we have said in our various communications, and marking, at the moment of the closing of the current, 9° and 7° 5′ at the end, on a galvanometer of 120 turns, traversed the three voltameters for more than two hours without producing the least indication of decomposition.

In another experiment, with four elements charged and arranged in the same manner, the intensity of the current was indicated by 30° of the same galvanometer, about 1–3rd of a cubic centimetre of hydrogen was disengaged in each of the voltameters in the space of two hours.

The first experiment shews clearly that electricity may traverse pure water and acidulated water without decomposing it.

The second proves only, that a current of feeble intensity is sufficient for decomposing both pure and acidulated water.

It must not be concluded from these results that a very great quantity of electricity passes through the voltameters without separating the elements of that liquid. These currents, which deviate the needle of even a not very delicate galvanometer, have no action on the compass of the smallest radius.

The means which appeared to us the most logical for appreciating the quantity of electricity which passed through the voltameters in these and other analogous experiments, without producing effect on the force lost, is to weigh the zincs before and after the experiment.

It would be imprudent, however, to give oneself up to this idea without restriction; we should be liable to greatly exaggerate the value of the quantity which we proposed to appreciate. Nothing proves that all the zinc dissolved in the battery is connected with the current. In 1850, 1851, and this year, I ascertained that in these circumstances, where the intensity is very feeble, the weight of the zinc dissolved in an isolated battery, the circuit of which was broken, was very little less than the weight of the zinc dissolved in the battery when the circuit was closed; even the weight of zinc dissolved in sulphuric acid of 1–10th, is only very little less than the weight of zinc dissolved in the open or closed battery. It is evident from this that there remains very little zinc for representing the quantity of inefficacious electricity.

It was in reliance on experiments of this kind, repeated several times in 1850, that I said in my eighth communication (*See* THE CHEMIST, 1851-2, p. 436), that the portion of electricity which is inefficacious is very small.

I postpone to another occasion the details which I should be in a condition to give now on this subject. I wish to use in these appreciations the proportional rheoscopes which I proposed in 1851.

I must say, however, that these weighings made with Grove's or Bunsen's battery, or with Daniell's battery, may give rise to errors, if we do not guard against certain perturbative causes.

With regard to the third point,—Do very variable quantities of electricity pass through several voltameters, each of which contain a liquid of a peculiar resistance? We shall examine this.

If these quantities were very different, it would be serious for the work which I now defend, but, which would be more serious for science, the law of chemical decompositions (Faraday, "Experimental Researches") would be attacked at its foundation; there would no longer be any certain measure for appreciating the interior working of a battery, &c. Very fortunately it is not so.

In an experiment made with 12 Bunsen's elements and three voltameters, there were disengaged in eight minutes $18^{cc}\cdot2$ in each voltameter. The first voltameter was filled with pure water containing 1–10th in volume of sulphuric acid, the second contained 1–20th of acid, and the third 1–100th.

A second experiment gave the same result in the same time; only the volume disengaged was $18^{cc}\cdot24$: the temperature was 48° 2′ F. in both experiments.

1. These graduated tubes, employed in these first experiments, were cylindrical throughout their length. We did not make the slight correction required for the slight difference of the elastic force of the vapor of water in each of the tubes; this correction would have changed the results very little; besides, these cylindrical tubes do not give sufficiently accurate measures for making corrections.

Thus, these liquids differing very much in their conductibility, or their resistances, traversed by the same current, disengaged sensibly the same quantity of gas.

The water, containing 1–100th of sulphuric acid in volume, was replaced by distilled water. Here the resistance of the distilled water weakened the current so much, that it required 300 elements for obtaining a very slight disengagement.

Electricity passed, even with 2 elements, sufficiently to deviate the needle of the galvanometer of a thermomultiplier.

Fifty elements produced, in several hours, a bubble of gas, of the size of a small pea.

With 300 elements, the intensity measured with a compass with tangents of 44 centimetres, was about 1–8th of a degree.

14 cc. 8 were disengaged in the voltameter with 1–10th, and in that with 1–20th; in the distilled water, the volume appeared rather greater, which was owing to a peculiar phenomenon produced in this experiment.

Each platinum wire of the voltameter passed into a glass tube, but these tubes and the voltameter were covered with mastic; each wire was also covered with mastic to the height of nearly 2 centimetres. I remarked in the voltameter containing distilled water, that the hydrogen carried away a whitish pearly matter, which was deposited on the sides of the tube. This matter tended to increase a little the apparent volume of gas disengaged. It was insoluble in water, soluble in alcohol and ether; it had a slight alkaline reaction, and left a little lime on being calcined.

In order to avoid the presence of this matter, I soldered to the wire the glass tube which passed through it; in this way there was no longer any contact between the current and the mastic, and the production of the white matter did not again occur.

It is also indispensable that the glass tube be raised at least 2 centimetres, and that the platinum wire be not very long (2 or 3 centimetres), without which, the bubbles of gas, which are very small, have not the power of ascending necessary for overcoming the slight current of water from above to below. Even with these arrangements, the distilled water during the decomposition is less clear, and the bubbles are more divided than in the acidulated water.

An experiment made with this new arrangement gave, in two hours and twenty minutes:—

	cc.
Water acidulated to 1–10th, . . .	13·57;
Water acidulated to 1–20th, . . .	the same number;
Distilled water	13·50

Another experiment, in which the water of 1-20th was replaced by water of 1–2000th, gave a similar result; the temperature was 59° F.

Each experiment requiring two or three hours for its completion, and the battery being scarcely ready before eleven o'clock, I raised the number of elements to 400; and as it was essential to compare distilled water with acidulated water, I employed only 2 voltameters, one filled with distilled water, and the other containing water mixed with 1–10th of its volume of pure sulphuric acid.

I also replaced the ordinary graduated tubes by tubes presenting, in an extent of 8 centimetres in length, a diameter of a few millimetres; the middle of this narrow tube corresponded to 10 cubic centimetres, and I was thus able to estimate with facility fortieths of a cubic centimetre.

About 10 cubic centimetres were disengaged in 45 minutes.

The pure water became considerably heated, so that the volumes measured by the voltametres would have given inaccurate results. The tubes were placed in a large vessel, full of water, at the ordinary temperature.

We thus made four experiments which agreed with each other.

The mean result was:—

Pure water, 10·19 cc.; acidulated water, 10·09 cc., at the temperature of 54° 14′F.

The volume disengaged in the distilled water was about 1–100th

greater than the volume disengaged in the acid water. This difference is reduced to 1–144th, if we make the correction relative to the elastic force of the vapor, which has not precisely the same value in pure water and acidulated water.

From these various experiments it might be concluded that a current, traversing several voltameters, one of which contains pure distilled water, that is to say, having no acid or alkaline reaction, and not precipitating nitrate of silver, and of which each of the others contains water acidulated to a particular degree, decompose sensibly, within 1-144th the same quantity of liquid.

However, I must own that I do not consider this 1–144th as the smallest limit of possible errors. I do not answer, in this first communication, for more than 1–100th. For various reasons, which it would be useless to enumerate now, I may venture to consider myself in a situation now to put the experiments out of the danger of causes of error, so as to answer for at least 1–200th of the total effect; so that, if the difference between the quantities of efficacious electricity exceeds this fraction, I shall appreciate it.

From the experiments, I think I may conclude that the difference between the quantities of electricity which efficaciously traverse several voltameters, does not exceed one-hundredth. If, in an experiment published in "*The Cosmos*," it has been found that the quantity of gas disengaged in water acidulated to 1–50th, it was because the arrangement and the small distance between the electrodes facilitated the combination of the two gases; at least, that is our opinion.

The decomposition of the distilled water is perfectly characterised by the phenomena which accompany it: the temperature was raised twenty times more even than the water containing only 1–2000th of sulphuric acid. All the water (more than a litre), became cloudy and whitish, an effect produced by thousands of gaseous molecules; whilst the water acidulated to 1–10th or to 1–2000th remains completely transparent, and the disengagement of the gas is manifested only at the extremity of the wires, under the form of a circle of bubbles, having for its centre the extremity of the wire electrode. This dissemination of the gaseous particles had made me think that they were perhaps in a state favorable to combination. In order to submit this idea to the test of experiment, I collected the two gases disengaged by the distilled water, in the same tube, whilst I collected the hydrogen only in the water acidulated to 1–10th. If water was reformed, the volume of hydrogen should exceed 2–3rds of the gas disengaged in the distilled water. If the two gases did not recombine, the hydrogen gas of the graduated tube in the acidulated water should make 2–3rds of the gas disengaged in the distilled water. It was this last result which was given; I had expected the contrary result. It is true that the distance of the wires was about 12 millimetres. I am, however, disposed to think that the combination would have taken place if the distance of the wires had been 1 or 2 millimetres; and still more easily if this distance had only been a fraction of a millimetre. I must necessarily confirm or weaken this view by experiment.

The graduated tube in which I collected the two gases, was like the graduated tubes of which I have spoken above, only the middle of the narrow tube corresponded to 15 cubic centimetres. This mode of proceeding is preferable, I think, to the employment of the eudiometer; it is more expeditious and more accurate. These tubes are analogous to those which were proposed, more than twenty years ago, for the analysis of the air.

We can have no desire to detract the least in the world from the merit of the works of the young and skilful experimentalists mentioned at the commencement of this Note; but the Academy has received with kindness the various researches which I have had the honor of submitting to it concerning the battery since 1848. I consider it my duty to defend them, including even those which may appear least important.

In conclusion, through pure water and aqueous liquids, only a very small inefficacious quantity of electricity passes, as I admitted in 1851, in my seventh and eighth communications.

The volume of hydrogen gas disengaged in several voltameters, containing pure water and water acidulated to a particular degree, is sensibly the same.

Comptes Rendus, No. 21, 1854.

On the PRODUCTION *of* PYRO-ELECTRIC CURRENTS. By M. BECQUEREL.

WE are acquainted with various means of producing the disengagement of electricity; friction, heat, light, the action of magnets, induction, molecular and chemical actions, &c. I have tried, whether it would not be possible, also, to develop the electric power by combining the action of heat at a high degree with that of affinities. My anticipations are realised, and I have arrived at producing currents which I shall call *pyro-electric currents*, by analogy with the currents obtained in the ordinary batteries, and in order to distinguish them from the thermo-electric currents, which are due only to heat.

These currents, which are of constant force so long as the temperature does not vary very considerably, are produced whenever metallic or other substances which are conductors of electricity and solid, are in contact with glass or any other vitreous substance in the state of igneous fusion, or softened by heat; but the maximum effect takes place only when the substance is fused.

In the Memoir which I presented to the Academy on the 1st of May, 1853, on the disengagement of electricity in chemical actions, I have shewn that glass, at a moderate temperature, began to conduct electric currents, and that we might avail ourselves of this property for studying the disengagement of electricity produced in contact with platinum wires with flames. This conductibility begins to be sensible towards 572° F.

I have since tried whether, this conducting faculty increasing with the temperature, it would not be possible, when the glass is fused, or

even before that, to substitute it for the acids and saline solutions in voltaic pairs. The following is the mode in which I operated :—

First Experiment.—If, in a furnace filled with lighted coals, we place a rod of soft iron and a rod of copper, each in connection with the ends of an ordinary multiplier, by means of a copper wire and an iron wire, the magnetic needle is not deviated, whatever may be the temperature ; it does not then disengage electricity. But this is no longer the case when we introduce the copper rod into a tube of rather fusible glass and heat it to the point of fusion. If we place in this circuit a multiplier and a compass with sines, we ascertain that long before the glass has arrived at a red heat, the needle of the multiplier has deviated ; by continuing to heat until fusion takes place, the current increases in intensity, reaches a maximum, and remains constant. Long before this period, the multiplier must be withdrawn, and the compass with sines alone made use of. This current is directed from the fire to the copper; through charcoal and glass, that is to say, that the iron, during its oxidation disengages negative electricity, and the copper, the surface of which remains clear and bright, liberates positive electricity. From this it is evident that the copper, although exposed to a high temperature, is preserved intact, as occurs when, being in contact with zinc or iron, it is steeped in an oxidising liquid. It owes this preservation, therefore, to a high temperature and to its electro-negative state. The current remains constant so long as the temperature does not vary considerably, and so long as the iron is not covered with a thick layer of oxide. But when it happens that the tube partially fuses, and that the copper touches the iron, all signs of electricity disappear. This fact proves that the current is not thermo-electric.

The disengagement of electricity in this circumstance has then a calorific and chemical origin. While the iron is being oxidised, this metal takes negative electricity, whilst the surrounding air takes the positive electricity, which is transmitted to the copper by means of the red-hot coals and incandescent glass with which they are in contact.

Second Experiment.—I sought at first the approximate ratio which exists between the current produced by the pyro-electric pair and the current which results from a Bunsen's battery of equal conductibility, and allowance being made for the loss in passage when there is an inversion in the direction of the currents. For this purpose I placed in the same circuit a pyro-electric pair and a nitric acid pair, the elements of which were of the same dimensions. These two pairs were placed successively in such a manner that the two currents should travel in the same direction and in two contrary directions.

Representing by x the intensity of the current furnished by the Bunsen's pair, and by γ that of the pyro-electric pair, we obtained with the compass with sines,

$$x + \gamma = \text{sin. } 17^\circ = 29237 x — \gamma = \text{sin. } 10^\circ = 17365,$$

whence we deduct $x = 23301$, $\gamma = 5936$

and consequently,

$$x : \gamma :: 3{\cdot}9 : 1.$$

In the circumstances in which I operated, the pyro-electric current had then four times less intensity than that of the nitric acid pair.

We have already seen that it is necessary to avoid heating the glass to complete fusion, since the iron and copper very soon touch, and all signs of electricity gradually disappear; but there is also another cause which diminishes the intensity of the current, and that is the oxidation of the points of junction of the metallic wires and of the iron and copper rods, when they are very near the focus of heat; this serious inconvenience is avoided by operating with very long rods, which enable us to place the points of junction at a distance from the furnace.

Third Experiment.—It might be foreseen that we could replace the oxidation of the iron at a high temperature by the combustion of charcoal at the same temperature; for that purpose it was sufficient to substitute for the iron rod a cylinder of charcoal prepared in the same manner as electrical conductors, to put them in communication, by means of a platinum wire, with the compass with sines, and to heat the other extremity to redness, near the glass tube which contains the copper rod; there is produced a current impelled in the same direction as that which had been furnished by the oxidation of the iron. We have then, in this case, the current resulting from the oxidation of the charcoal.

By comparing, as we had done with the iron, the current of the charcoal and copper pair with the nitric acid pair, we have arrived at the following results:—

$$x + \gamma = \text{sin. } 14°30' = 25038$$
$$x - \gamma = \text{sin. } 8°50' = 15357$$

whence we deduce

$$x = 20197,\ \gamma = 5340,$$

and consequently,

$$x : \gamma :: 3{\cdot}76 : 1.$$

The current of the nitric acid pair was then nearly 3·76 times stronger than the pyro-electric current of iron and charcoal.

Fourth Experiment.—In order to make an accurate comparison of these two sources of electricity, the conducting power of the sources themselves must be determined by means of Ohm's law. The experiments which I have hitherto made shew that in the most favourable circumstances, when the temperature approaches the fusing point of the copper, the conducting power of the two sources is nearly the same; but going away from this term, the resistance to the passage augments more and more in the pyro-electric pairs.

Fifth Experiment.—Pyro-electric currents produce chemical decompositions like other currents.

With two platinum plates and a Bunsen's pair, water is very rapidly decomposed, whilst with a pyro-electric pair acting when it is not near the fusing point of copper, the current is perceptibly stopped, at least until the platinum plate is replaced by a copper one; then the disengagement of hydrogen gas is very abundant; the same thing occurs by

substituting for the acidulated water a solution of sulphate of copper: with two plates of copper the sulphate is decomposed.

Observations.—Pyro-electric pairs may be prepared in several ways; I will give three:—

1. We place in an ordinary reverberatory furnace, an earthen crucible, lined inside with a thick plate of copper, moulded to the form of the crucible, and furnished with a wire of the same metal passed into an earthen tube, in order to preserve it from oxidation. The crucible is filled with powdered glass in sufficient quantity to cover the copper-plate to the extent of 2 centimetres, when it is fused. In contact with the glass is placed vertically by one of its ends a bar of iron sufficiently long to reach above the top of the furnace; to the other end is fitted a wire of the same metal, which serves to maintain it in the position in which it has been placed, and to put the pair in communication conjointly with the copper wire with the compass with sines or with any other apparatus.

2. After having filled the crucible with broken glass, to which had been added 0·25 grs. of carbonate of soda, in order to hasten the fusion, two long rods of iron and copper are introduced, avoiding contact, and maintained in a vertical position by means of iron and copper wires adapted to the free ends and serving as conductors, which wires are attached to points fixed outside. As soon as the glass is fused, the oxide of iron formed is dissolved, and the surface of the stem of the same metal remains always clean; the current produced is also constant. Care must be taken not to heat so as to fuse the copper. I may observe that the current has a certain degree of intensity, even before the fusion of the glass.

3. We take a pistol-barrel and introduce into it a tube of green glass containing a cylinder of copper; after having filled all the interstices of the barrel and tube with powdered glass, the whole is placed horizontally in a furnace arranged for the purpose; the pistol barrel and the copper cylinder are placed in communication with the apparatus by means of wires of the same metals. This arrangement gave me the best results.

Sixth Experiment.—In the pyro-electric currents above described, copper was used as an electro-negative element; but platinum and retort charcoal may also be employed; both, however, present inconveniences. Platinum is attacked by the glass and is disaggregated; charcoal burns very slowly, and produces a current in the inverse direction, which diminishes the action of the current resulting from the oxidation of the iron. It is possible, I think, to obviate this inconvenience by introducing a cylinder of charcoal into an earthen tube, and closing the orifices with clay, in order to prevent the circulation of the air.

Observations.—Glass is not the only vitreous substance that may be employed: among those which I have tried, I may particularly mention borax; but I have given it up, as it too violently attacks the elements of the pair. I have likewise renounced the use of common salt and nitrate of potassa; the effects being too feeble, except

when the latter salt is employed with charcoal, a pair which gives a very powerful disengagement of electricity at the moment of the deflagration of the charcoal; this pair, on account of its rapid effect and of the danger which it presents, cannot be of any use.

Sand and pure quartz, whatever may be the temperature to which they were raised, did not acquire the conducting property, and could not be used in place of glass or the alkaline silicates.

In an early Memoir I shall describe the effects obtained with batteries formed of pyro-electric currents.

The facts contained in this note shew that we may find in the waste heat of manufactories a means of making pyro-electric pairs act, producing electric currents which partake of the nature of hydro-electric and thermo-electric currents. They render probable, also, the existence of a terrestrial electric current in contact with, or proximity to, the solid portion and the portion in fusion of the globe, where it finds solid conducting substances, partially embedded in the fused silicates, in the same manner as pyro-electric currents.

Comptes Rendus, No. 21, 1854.

TOXICOLOGY.

NOTE *on* POISONING *produced by a* POISONOUS SUBSTANCE *at present little known—the Atractylis gummifera of* LINNÆUS—*and on its active principle.* By M. COMMAILLE, *of Douera, Algeria.*

First Observation.—Marie Esther Bartholomo, living in Douera, aged 3½ years, was in good health on the evening of the 7th of March. On the 8th, at eight in the morning, I was called in to her; the following is what I observed:—The child was lying on its back, the arms extended along the body, the legs stretched, the eyes closed, the teeth so clenched that it was impossible to open the jaws; there were large violet stains on the integuments, the face was marbled with violet, the lips were bluish, the pulse was imperceptible, the respiration was slow, and the ribs were raised by shocks. There were no convulsions.

The parents did not know to what cause to attribute the illness of their child, who, that very morning, at break of day, had got out of bed, and would not accept the aid of her sister in getting into it again. The food of the family consisted, on the 7th, of coffee, beans, and rice.

In the presence of such symptoms, I thought it was a case of pulmonary and cerebral apoplexy (the *post-mortem* examination shews that I was not mistaken). But to what was this disease to be attributed? I was far from suspecting the cause. I applied six leeches to the neck, and sinapisms to the lower extremities.

At ten o'clock, I returned to my little patient; there was no change in her state. I auscultated the chest and trachea; the respiration was slow and feeble; but there was no abnormal bruit. Might there not be, however, a pseudo-membraneous laryngitis? Notwithstanding the absence of hissing rattle and croupy cough, might we not suppose

that a respiration so weak, and on the point of ceasing altogether, might still operate through the false membranes, without having the power to make them vibrate so as to produce a sound? Under this idea, I prescribed a draught, with 2 centigrammes of emetic and 2 decigrammes of calomel. The patient vomited a sanguinolent liquid. She died at noon.

Second Observation.—Next day (the 9th), at seven o'clock in the morning, I was sent for in great haste to see the eldest sister of the little girl, the subject of the foregoing observation. This child, Elizabeth Octavia, aged 6½ years, was taken suddenly, at half-past five o'clock in the morning, with the same symptoms as her sister. Although I was scarcely three minutes in getting there, the child was dead when I arrived. I examined the body: there was the rigidity and the same cyanosis as in her sister; I opened the eyelids, to assure myself of the death, when I was struck with the enormous dilatation of the pupils. I examined the eyes of the child who had died the day before, and found the pupils also highly dilated. It instantly occurred to me that there might have been a poisoning by some of the poisonous Solanaceæ, although in both cases there had been no convulsions, delirium, or vomiting.

The parents had not seen in the hands of the children any euphorbia, ranunculus, ricinas, or nightshade, plants which are common along the roads.

Post-mortem examination.—This was conducted by the surgeon in chief of the Civil Hospital, commissioned for the purpose by the mayor, with the assistance of the chief physician, of the physician of the Colony, of MM. Blondel and Negrin, and of myself, in the presence of the justice of the peace and commissary of police.

1. *Autopsy of the Youngest on the 9th, at* 2 P.M.—Integuments cyanosed, pupils enormously dilated, brain and sinus of the dura mater gorged with blood, inflammation of the tunica arachnoïdes, the white substance covered with red when cut in slices, serous effusion into the medulla oblongata, inflammation of the trachial artery and of the bronchiæ, œsophagus healthy, lungs and liver gorged with black blood, which gushed out when they were cut; the right ventricle and right auricle of the heart full of blood; stomach healthy, except in the great cul-de-sac, where it presented a patch of the size of a crown piece, so inflamed that it appeared gangrened, its edges trenched on the rest of the mucous membrane, which was white and healthy. The intestines were half empty and very healthy, and the mucous follicles of the colon were very apparent. The mucous membrane of the bladder was inflamed. There was much urine.

The stomach and intestines contained a half-liquid pultaceous matter, in which was a quantity of ligneous remains, broken and mangled, analogous to the residue of the pulverisation of certain roots. The descending colon was especially full of this matter; some pieces even, in consequence of the vomiting produced by the emetic, had ascended to the pharynx, and then fallen into the trachea. We shewed these

vegetable remains to the father, who informed us that they often ate the roots and stalks of thistles after having cooked them, and that children even eat these roots raw.

2. *Autopsy of the Eldest, March* 10, *at* 9 A.M.—The same cyanosis and lesions of the brain, liver, lungs, and heart, as in her sister; œsophagus healthy; trachea less inflamed; stomach inflamed towards the pyloric orifice, but without any stain, as apparent as in Esther; mucous membrane thickened and softened, and detaching easily in layers on the slightest friction; intense entero-colytis. Some slight ligneous remains in the intestines.

The chief surgeon of the hospital then remembered that some years ago, several children of the Orphanage of Benacknoun had died after eating of a thistle, notwithstanding the care which was bestowed on them by Dr. Tabouret, military physician, then physician to that establishment, and who, I believe, published an account of this poisoning. From information derived from the Abbé Brumeau, director of the Orphanage, it appears: that the poisonous thistle, the cause of the accident at Benacknoun, has no stem; that the flower grows on the ground; that the leaves are thorny, and that a white juice, of which the children make bird-lime, flows from the fruit. I immediately recognised, from this description, that this thistle was the *Atractylis gummifera* of Linnæus, described in the *Prodromas* of De Candolle, under the name of *Carlina gummifera*, Less. The residues found in the intestines and stomach of the children were only undigested ligneous matter and cellulose.

A fact worthy of notice is the time which elapsed between the two deaths—nineteen hours. The younger child, which was weaker, and had, perhaps, eaten more of the poisonous thistle (it appears to me certain that the children had eaten of several kinds), died first. The eldest, presenting greater strength, died a long time after. The former had not had a stool, and had not passed any of the poison; the latter had had a stool, and its intestines were empty, and the inflammation was less limited. The latter child had thus, at eight in the morning, complained of colic, to which no attention had been paid, the parent being occupied with her sister.

The father also found in the house fragments of the root of *Atractylis*, or bird-lime-thistle, as he called it: he knew it well as being very bad to eat.

These children were poisoned by a substance which acted as an irritant (partial or general gastritis, enteritis), and as a stupefyant of the ganglionic system (pulmonary paralysis), and in particular of the ophthalmic or orbital ganglion of Chaussier (dilatation of the pupil); or even of the *trijumeau* (dilatation of the pupil, clenching of the jaws).

Third Observation.—A third child, August Klingler, was poisoned on the 29th of April, at 7 p.m.; he was four years of age. The same symptoms were manifested as in the little girls. No *post-mortem* examination was made; but other children owned to having eaten thistles together, and they exhibited some of them; these were *Scolymus* and *Cardonet*. The deceased child, who was the youngest, must

in ignorance have gathered the *Atractylis*, which grows among the *Scolymus*, and from which, at first sight, when the stem of the latter has not put forth, it is very difficult to distinguish it.

It became interesting to ascertain whether the *Atractylis gummifera* is really a poison; to what substance it owes its poisonous action, and how it acts on living animals. This is the object of the second part of this Note. After an analysis which, made in unfavorable circumstances, does not appear completely satisfactory to the author himself, he tried various preparations, some of which were without action, whilst others shewed a most distinct poisonous power; thus, pure water in which the root of *Atractylis* had been macerated only a very short time, poisoned kittens; and the autopsy demonstrated in these animals disorders quite similar to those which were observed in the children who form the subject of the first two observations: the dilatation of the pupils was also very manifest.

Comptes Rendus, No. 24, June 12th, 1854.

The Poison *of the* Rattlesnake.

A paper was lately read before the Boston Society of Natural History, from Dr. W. J. Burnett, on the character of the rattlesnake. The Doctor had been experimenting on two or three specimens of this animal, and announces the discovery of numerous embryo fangs in the jaws of the snake, immediately behind the outward fangs. The use of these hidden weapons of destruction appears to be to supply the place of the biting fangs of the serpent when they get broken off or worn out in service. It also appears that the long fangs (two in number), which are used in inflicting the deadly bit of the rattlesnake, are naturally shed every few years, when they are not injured by accident or wear, and the reserve fangs are sufficiently numerous to meet the worst emergencies. From minute microscopic examination of the structure of these teeth, Dr. B. concludes that there are two canali in each fang, only one of which conveys the poison to the wound. Respecting the character of the poison itself, the Doctor remarks as follows:—

"There is good reason for the belief that its action is the same upon all living things, vegetables as well as animals. It is even just as fatal to the snake itself as to other animals; for Dr. Dearing informed me that one of his specimens, after being irritated and annoyed in its cage, in moving suddenly, accidently struck one of its fangs into its own body; it soon rolled over and died, as any other animal would have done. Here then we have the remarkable, and perhaps unique philosophical fact, of a liquid secreted directly from the blood, which proves deadly when introduced into the very source (the blood) from which it was derived!"

In order to scrutinise by the aid of the microscope, the operation of this deadly agent on the blood, Dr. Burnett stupified one of the fiercest of his snakes by dropping chloroform on his head:—

"Twenty-five or thirty drops being allowed to fall on his head, one

slowly after the other, the sound of his rattle gradually died away, and in a few minutes he was wholly under the agent. He was then adroitly seized behind the jaws with the thumb and forefinger, and dragged from the cage, and allowed partially to resuscitate; in this state a second person held his tail to prevent his coiling around the arm of the first, while a third opened his mouth, with a pair of forceps, pressed the fang upward, causing a flow of poison, which was received on the end of the scalpel. The snake was then returned into the cage.

"Blood was then extracted from a finger for a close microscopical examination. The smallest quantity of poison being presented to the blood between the glasses, a change was immediately perceived—the corpuscles ceased to run and pile together, and remained stagnant without any special alteration of structure. The whole appearance was as though the vitality of the blood had been suddenly destroyed, exactly as in death from lightning. This agrees also with another experiment performed on a fowl, where the whole mass of the blood appeared quite liquid and having but little constitutable power."

Dr. Burnett is of opinion that the physiological action of the poison of the rattlesnake in animals is that of a most powerful sedative, acting through the blood on the nervous centres. He supports this position by the remarkable fact that its full and complete antidotes are the most powerful stimulants; and of these, alcohol (commonly in the form of rum or whisky) is the first. The remedy is well known in the south, and there are some twenty-five authentic cases on record proving that a person suffering from the bite of a rattlesnake may drink from one to two quarts of clear brandy without feeling the slightest tendency to intoxication, and eventually recover.

Hertford Times.

THERAPEUTICS.

On the Action *of* Sulphate *of* Strychnine *in* Cholera.

By M. Abeille.

From the 26th of April to the 3rd of June I have administered strychnine to twenty-two cholera patients in the stage of collapse, with complete absence of pulse at the wrist, or when it was scarcely perceptible. The following have been the results:—

In nineteen of these patients a progressive reaction took place with an increasing pulse in the space of from four to twenty-four hours, from the moment of its administration or during its use.

The doses of strychnine varied from 0·015 to 0·030 twice a-day, to four doses in four hours. In the three remaining cases no reaction took place, and they died. In the reaction the circulation became sometimes so active, that bleeding was necessary for the double purpose of moderating its power and remedying the organic hyperæmia.

Such a result, during a period of cholera, in which, perhaps, five-sixths of the patients have sunk without the possibility of re-establishing a free circulation, is very remarkable, and proves the great power of strychnine.

But it is not sufficient to save the patient at that dreadful stage of cholera; other dangers attend the reaction. Either in consequence of hyperæmia, or from some other cause, many sink in a torpid condition. This occurred in nine of the nineteen cases which passed safely through the collapse.

From the rapid action of strychnine at the period of collapse, it possesses a value which astonished us, and which nothing else has equalled.

As for the final results, that is, the number of cures which it effected (ten in twenty-two), it is more than has hitherto been obtained in similar circumstances. Moreover, a work which I shall soon have the honor of submitting to the judgment of the Academy, will, I hope, leave no doubt as to the value of strychnine in cholera.

Comptes Rendus, No. 25, June 19th, 1854.

Effects *of* Caffeine.

Dr. Eulenberg has obtained favorable results from the use of caffeïne in the treatment of hemicrania in two instances. Both the patients were men between thirty and forty years of age, and enjoying, in other respects, good health. The attack recurred at intervals of from one to four weeks, becoming more and more aggravated until it reached its height, and was occasionally relieved by emetics. Various remedies had been previously used without success. He exhibited two grains of caffeïne as soon as the attacks commenced, and repeated this dose thrice at intervals of two hours. It was found to have the effect not only of relieving the pain, but also of lengthening the interval between the attacks.—*Allgemeine Medic. Centr.-Zeitung*, and *Dublin Hosp. Gazette.*

NOTES AND QUERIES.

TO THE EDITORS OF "THE CHEMIST."

Gentlemen,—Nothing can be more detrimental to the advancement of any science than the glaring errors committed by those who profess a practical knowledge thereof; this is more particularly the case with chemistry; no wonder that our clients feel dissatisfied, and say within themselves,—these *soi-disant* chemists are either no chemists at all, or the science itself is a mystery, since no two analyses agree—they have no system—their results are so utterly at variance with one another, that it is impossible to arrive at the truth. Such were the sentiments uttered by a non-professional gentleman the other day, and with how much truth I will leave your readers to judge. The circumstances are these:—

The gentleman alluded to being afflicted with *diabetes mellitus*, consulted a Dr. ——, who made an analysis of his urine before going into the country. On the analysis being submitted to me, I was so struck with the large quantity of urea said to exist, that I at once pronounced it an error, since it is generally admitted, and my experience

has also proved it, that in proportion as sugar increases, urea decreases, and in some cases does not exist at all.

Dr. —— states the specific gravity to be 1·036.

and 1 oz. of urine to contain { 33 grs. of sugar. / 10½ „ of urea !

the estimate, therefore, per 1000 grs (2¼3 fld), would be

Sugar	. . . .	74 grs
Urea	. . . .	22½ „
		96½ gross weight.

I would here remark that I had on two other occasions manipulated simultaneously with Dr. —— on the same specimens, and found great inaccuracies, which my friend could not reconcile at all; but as the Doctor wished for a fresh sample of that made during twenty-four hours, one was sent to him on the 2nd of July, of which the above is the analysis; at the same time a sample was put into my hands, and of which the subjoined is an accurate analysis. Before I give it, I will just observe that every one knows the time an analysis of this kind occupies (from thirty to forty hours); yet in less than twenty-four hours my friend had received Dr. ——'s analysis per post.

MR. HORSLEY'S ANALYSIS.

Specific gravity, 1·032 *only* by balance.

Mixed solid contents of 1000 grs. ascertained by evaporation to dryness, 76 grs !

This consisted of

Urea . . .	6 grs
Saline matter . .	22 „
Sugar . . .	48 „
	76

Comparison of the Analyses of same sample.

Dr. ——. Specific Gravity, 1036.		Mr. Horsley. Specific Gravity, 1032 !	
Sugar . . .	74 grs.	Sugar . . .	48grs !
Urea . . .	22½ „	Urea . . .	6 !
		Salts . . .	22
	96½		76

In the first place it will be perceived there is a manifest difference between my own and Dr. ——'s specific gravity of 4. There can be no mistake on my part, as it was carefully weighed in the presence of the party who brought it. Next, the Doctor makes the quantity of sugar alone almost equal to the whole solid contents !! Next, the quantity of urea is marvellously overrated; neither is any allusion made to saline matters, which would have swelled the Doctor's estimate of solids to nearly 50 per cent. more than existed naturally.

I am, Mr. Editor, yours respectfully,

JOHN HORSLEY.

The Laboratory, Cheltenham, July 4th, 1854.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

CHEMICAL TABLES.

COMPILED BY THORNTON J. HERAPATH.

No. II.

THERMOMETRICAL EQUIVALENTS *and* EFFECTS *of* TEMPERATURE.

(Concluded from page 649.)

398°·5 F. Water boils (pressure = 16 atmos.)
399° F. Æthylaniline boils.
400° F. Ley containing 53·8 soda boils; also camphor and pyro-guaiacic acid.
Bichlor. platinum is converted into neutral chloride.
Yellow chrom. potash acquires a crimson color.
401° F.; 205° C. Vitriol of 1·744 or 65 boils.
Cinnamic æther boils.—*Staedeler.*
402° F. Perchlorate potash is decomp.
403°·8 F. Water boils (pressure = 17 atmos.)
406°·5 F. Phorone boils. Mesaconic acid fuses.
408°·9 F. Water boils (pressure = 18 atmos.)
410° F.; 210° C. Cane-sugar is caramelised between 410° and 420°.
Pyrogallic acid sublimes.
Vitriol of 1·757 or 66° boils.
Bisulph. of potash fuses; also inosite.
411° F. Cantharidine fuses, and rapidly sublimes.
412° F. Water boils in Papin's digester.
413°·8 F. Water boils (19 atmos.)
416° F. Ordinary phosphor. acid is changed into a mixture of the pyro and metaphosphoric acids.
417° F. Succinic æther boils; also bisæthylamine.
418°·5 F. Water boils (pressure = 20 atmos.)
419° F.; 215° F. Anhyd. valeric acid boils.
420° F. A sat. aqueous sol. of caustic soda boils; also sulphide of amyl and fused oxalic acid.
Paper becomes brown or slightly singed; 420° to 444°.
422° F. Naphthaline sublimes.
422°·6 F. Vitriol of 1·769 or 67 boils.
Water boils (pressure = 21 atmos.)
424° F. Arsenious acid sublimes.—*Mitchell.*

425° F. Anhyd. camphoric acid fuses.
427°·3 F. Water boils (pressure = 22 atmos.)
428° F.; 220° C. Sulphur becomes viscid.
Terchloride of arsenic boils.
Amylo-urethane fuses and distils.
430° F. Tempered steel becomes of a faint yellow color; temper for lancets, &c.
431°·5 F. Water boils (pressure = 23 atmos.)
433° F. Œnanthic æther boils.—*Delffs.*
434° F. Nitrobenzide boils.
434°·25 F. Vitriol of 1·78 or 68% boils.
435° F. Phenamylole boils; 435° to 437°.
435°·6 F. Water boils (24 atmos.)
436° F. Carbonate of amyl-oxide boils.
437° F.; 225° C. Salicylic æther boils; 437° to 442°.
438°·8 F. Carbonate of amyl-oxide boils.—*Bruce.*
439° F. Terebenzic æther boils.
439°·4 F. Water boils (25 Atmos.)
442° F. Tin fuses.—*Crichton* [Latent heat = 500.—*Irving.*]
Cadmium fuses a little below 442°.
445° F. Aniline boils.—*Fritzsche.*
446° F.; 230° C. Vitriol of 1·791 or 69⅔ boils.
Gun-cotton explodes instantaneously.—*Schönbein.*
Terchloride of antimony boils.—*Capitaine.*
Mean heat of a baker's oven.—*Tilley.*
Chrysene fuses.
450° F. Tempered steel becomes of a pale straw-color; temper for razors and surgeons' instruments.
Acetate of soda fuses and becomes anhyd.; 400° to 450°.
455° F.; 235° C. Sugar of the acorn fuses.
Tin fuses.—*Person.*
457° F. Caprylic acid boils.
457°·2 F. Water boils (30 atmos.)
460° F. Tempered steel becomes straw-colored; pen-knives, &c.
Benzoïc acid *boils.*
Nitrate of ammonia begins to evolve nitrous oxide; 460° to 482°
462° F. Cumonitril boils; 462° to 478°.
463°·5 F. Vitriol of 1·801 or 70⅔ boils.
464° F.; 240° C. Bisulphide of amyl boils; 464° to 500°.
Cinnamic ether and pyromoritannic acid boil.
466° F. Cinnamate of methyl boils.
470° F. Crys. succinic acid boils; also euganic acid.
472°·75 F. Water boils (35 atmos).
473° F.; 245° C. Vitriol of 1·81 or 71⅔ boils.
475° F. Nicotine boils.—*Gerhardt.*
480° F. Cuminic and anhyd. succinic acid boil.
480°·25 F. Bismuth fuses.—*Crichton* [L.H. = 550°.—*Irving.*]

482° F.; 250° C. Oil of turps begins to be modified isomerically between 464° and 482°.
Bicarb. soda is converted into sesqui-carb.
Phosphorus boils.—*Heinreich.*
Oxides of gold and silver are reduced.
485° F. Benzoate of amyl boils; 485° to 489°.
486°·6 F. Water boils (40 atmos.)
487°·4 F. Vitriol of 1·819 or 72⅞ boils.
490° F. Chlorocuminole boils; 490° to 500°
491° F.; 255° C. Water boils (45 atmos.)
495° F. Triamylaniline boils.
496° F. Amylaniline boils.
500° F.; 260° C. Zinc fuses.—*Black.*
Vitriol of 1·827 or 73⅞ boils.
Sulphur takes fire in the air.
Sugar inflames when suddenly heated up to 500°.
Tempered steel becomes bright brownish-yellow; axes, plane-irons, cold chisels, and shears for cutting iron.
Alabaster gypsum changed into "Plaster of Paris."
Oxygen begins to be evolved from fused chlorate of potassa.
Oxide of zinc turns yellow.
Peroxide of iron, hyd. becomes anhyd.
Iodic acid fuses.
Decomp. of fats and fixed oils commences.
Oxalate of amyl-oxide and cinnamic ether boil.—*Marchand.*
Men have been known to remain for some time in ovens heated to this temp., without sustaining any injury; 400° to 500°.
507°·25 F. Bismuth fuses.—*Rudberg.*
509° F.; 265° C. Bismuth fuses.—*Hermann.* [Latent heat = 550°.—*Irving.*]
Chloride of benzide boils; 509° to 414°.
510°·6 F. Water boils (pressure = 50 atmos.)
514°·4 F. Vitriol of 1·833 or 74⅛ boils.
518° F; 270° C. Borate of amyl boils; 518° to 527°.
3-bromide of antimony, camphoric acid and coumarine boil.
Tempered steel becomes purple; temper for table knives, cloth-shears, &c.
Bismuth fuses.—*Person.*
521° F. Capric acid boils.
527° F; 275° C. Helenine boils.
528° F. Cetene boils.
528°·8 F. Aboleone boils.
529°·5 F. Cyanuric acid boils.
530°·6 F. Vitriol of 1·838 or 74⅛ boils.
535° F. Chl. of carbon, C^4Cl^4, boils.
536° F; 280° C. Asarone boils.
537° F. Kyanethine boils.

538° F. Petrolene boils.
540° F. Tempered steel becomes deep blue; sword-blades and watch-springs.
Caryophylline sublimes.
Cedar-camphor and oil of rue boil.
544° F. Carotin is carbonised.
545° F.: 285° C. Vitriol of 1·842 or 76⅔ boils; also nitraniline and picamar.
550° F. Sparteïne fuses.
Orcin sublimes.
558° F. Oil of vitriol of commerce boils?—*Davy.*
Sulphur boils, 558° to 562°.
559·4° F. Vitriol of 1·845 or 77% boils.—*Dalton.*
560° F. Tempered steel becomes of a full blue color: temper for small fine saws and daggers.
Sulphur takes fire in the air.—*Thomson.*
Cinnamic acid boils; 560° to 573°.
563° F.; 295° C. Cyanide of mercury yields cyanogen.
Calomel sublimes.—*Waechter.*
565° F. Nitrobenzoïc acid boils.
570° F. Quick lime, when slackened, evolves heat equal to 570°.
Acetate of potash is decomp.
Nitrocinnamic ether, paranaphthaline, and artificial ozocerite boil.
572° F.; 300° C. Guaiacum fuses; 570° to 572°.
573° F. Phosphorus boils.—*Gregory.*
Metamylene distils.
573·8° F. Vitriol of 1·847 or 78% boils.
574° F. Temp. for manufac. "Minium;" 574° to 590°.
590° F.; 310° C. Vitriol of 1·848 or 79% boils.
600° F. Tempered steel becomes of an uniform deep blue color; 580° to 600°; temper for large saws, &c.
Acetate of soda is not decomp.
Nitre fuses.
Mercury, when heated, in "Boyle's Hell," to nearly 600°, is gradually converted into red oxide.
Linseed oil, and soda-ley of 1·85 or 63·6⅔ boil.
604·4° F. Vitriol of 1·849 or 80% boils.
608° F.; 320° C.
612° F. Lead fuses.—Crichton [L. H. = 162°.—*Irving*].
620° F. Vitriol of 1·85 or monohyd. Sulp. acid boils.—*Dalton.*
626° F.; 330° C.
630° F. Whale-oil boils.
644° F.; 340° C. Lead fuses.—*Schwartz.*
650° F. Selenium takes fire in oxygen.
660° F. Mercury boils in glass.—*Crichton.*
662° F.; 350° C. Mercury boils in an ordinary thermometer.
680° F.; 360° C. Mercury boils in a syphon.

698° F.; 370° C. Red oxide of mercury is decomp., and evolves oxygen gas.—*Waechter.*
700° F. Temp. for calcining copper ores; from 700° to 1200°.—*Napier.*
703° F. Zinc fuses.—*Guyton Morveau.*
Tellurium fuses.
716° F.; 380° C.
734° F.; 390° C.
752° F.; 400° C.
773° F.; 411·66° C. Zinc becomes very brittle between 600° and 773°—*Some Authors.*
775·5° F.; 413·05° C. Zinc fuses.—*Daniell.* [L. H. = 493°.—*Irving.*]
800° F.; 426·66° C. A dull red heat.—*Daniell.*
810° F.; 432·22° C. Degree of incipient luminosity in a dark place.—*Davy.*
Antimony fuses.—*Daniell.*
Arsenic sublimes?—*Most authors.*
955·5° F.; 513·05° C. Antimony fuses.—*Guyton Morveau.*
Cadmium distils.
977° F.; 525° C. Incipient red heat.—*Pouillet.*
Lead glass softens.
Peroxide of manganese yields oxygen.
Temp. required for the complete decomp. of organic matters by oxide of copper or chromate of lead.
Antimoniuretted hydrogen and arseniuretted hydrogen are decomp.
Oxygen or air passed over barytes converts the latter into peroxide of barium.
Vapor of chloroform is decomp.
980° F.; 526·66° C. A red heat.—*Daniell.*
1000° F.; 537·77° C. A dull red heat, visible in daylight.—*Brande.**
1140° F.; 615·55° C. Heat of a common fire.—*Daniell.*
Common salt is volat., and tinges flame blue.
1200° F.; 648·88° C. A full red heat.—*Brande.**
Sulphur will combine with iron and copper.—*Napier.*
1310° F.; 710° C. Dull red heat.—*Pouillet.*
1700° F.; 926·66 C. An orange red heat.—*Brande.**
1800° F.; 982·22° C. Temp. for calcining copper ores; 1700° to 1800°.—*Napier.*
1869° F.; 1020·55° C. Brass melts.—*Daniell.*
1873° F.; 1022·77° C. A bright red heat.
Silver fuses.—*Daniell.*
Tellurium boils and is volatilised.
Lead glass, working heat for.
Hyd. soda, litharge, and protosulphide of iron fuse.
Prussiate of potassa is decomp.
Molybdic acid is volat. in open vessels.
Proper temp. for cupelling.
1996° F.; 1091·11 C. A dull white heat.

Copper fuses.—*Daniell.*
Bismuth boils and is volatilised.
Hyd. of potassa is somewhat volatile.
2016° F. ; 1102·22 C. Gold fuses.—*Daniell.*
2204° F. ; 1206·66° C. Copper fuses.—*Morveau.*
[Bronze and bell metal are more fusible than copper].
2786° F. ; 1530° C. Cast iron fuses.—*Daniell.*
Cobalt fuses.
3000° F. ; 1648·9° C. A white heat.—*Brande.**
3202° F. ; 1761·11° C. Temp. of the sun ; 2661° to 3202.—*Pouillet.*
Temp. for welding platinum.
Melted lead takes fire and is oxidised.
Bohemian glass softens.
Diamond is very slowly burned in the air, but if immersed into oxygen it burns vividly.
3300° F. ; 1815·55° C. Heat of a good wind furnace.—*Daniell.*
Aluminium fuses.

In a smith's forge or smelting furnace :—

Barium, manganese, and malleable iron fuse ; and nitride of titanium is volatilised.
Silica fuses ?
Strontium fuses.

Before the oxy-hydrogen blowpipe, or in the foci of large burning lenses :—

Lime, silver, and gold are volat.
Platinum, rhodium, iridium chromium, molybdenum, and the allied metals are fusible.
[Palladium is more fus. than platinum, &c.]

In large voltaïc apparatus :—

Carbon is volat., and crystallises on condensation in octoëdra ?—*Despretz.*

Palladium and platinum are volatilised.

It is probable that at the centre of the earth the temp. is = 3500° W. or about 331,555° F., and that even at the depth of 120 miles it exceeds 100° W. or 9813° F., which is more than sufficient for the fusion of lava and other rocks, &c.— *Cordier.*

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

On the DECOMPOSITION *of* HYDROBROMIC ETHER *by* POTASSIURETTED ALCOHOL.

By M. MARCELLIN BERTHELOT.

1.—According to Dr. Williamson's experiments, potassiuretted alcohol (a combination of alcohol and potassium) put in contact with hydriodic ether is decomposed and produces ordinary ether :—

$$C^4H^4KO^2 + C^4H^5I = C^8H^{10}O^2 + KI.$$

Whence Dr. Williamson concludes that one molecule of ether, $C^8H^{10}O^2$, results from the union of two molecules of alcohol, $C^4H^6O^2$, and contains twice the quantity of carbon. This point of view has led him to prepare conjugate ethers, formed by the union of alcohol and wood-spirit, of alcohol and the oil of potatoes, &c. For this purpose it is sufficient to cause either hydromethylic ether or hydroamylic ether to react upon potassiuretted alcohol :

$$C^4H^5KO^2 + C^2H^3I = C^6H^8O^2 + KI.$$

I thought that these same reactions might be produced by replacing the combination of alcohol and potassium by a mixture of alcohol and potassa.

2.—In fact, the preparation of common ether by the reaction of hydrochloric ether on an alcoholic solution of potassa was long ago mentioned by M. Balard.

Hydrobromic ether likewise gives rise to common ether, as I have observed. From eight to ten hours' contact in closed vessels, at 212° F, with an alcoholic solution of potassa, suffices for the complete decomposition of hydrobromic ether.

3.—I asked myself whether this production of ether by means of an alcoholic solution of potassa differed from Dr. Williamson's reaction. In fact, the formation of ether in the preceding experiments may be explained in two ways :

1st. Hydrobromic ether changes, directly its bromine, for the oxygen of the potassa :

$$C^4H^5Br + KO = C^4H^5O + KBr.$$

In this way the phenomenon is merely a substitution ; the alcohol simply performing the part of a common solvent.

The reaction thus represented would prove the equivalence of hydrobromic ether, C^4H^5Br and common ether C^4H^5O. It would lead us to retain the formula hitherto admitted for ether, C^4H^5O.

2nd. The reaction may be produced by the simultaneous concurrence of the three bodies thus united—alcohol, potassa, and hydrobromic ether :—

$$C^4H^5Br + KO + C^4H^6O^2 = C^8H^{10}O^2 + KBr + HO.$$

The phenomenon thus represented would be analogous to the re-

actions described by Dr. Williamson, and would lead to the formula $C^8H^{10}O^2$.

4.—These two explanations relate to the formation of the same body by means of the same two substances; but there is an essential difference between them, which is easily proved by experiment.

By the first explanation one equivalent of hydrobromic ether, C^4H^5Br, produces an equivalent of common ether C^4H^5O. The weight of alcohol employed is of no consequence, the alcohol may even be replaced by any solvent of both potassa and hydrobromic ether, without any change in the reaction.

By the second explanation, one equivalent of hydrobromic ether, C^4H^5Br, produces one equivalent of ether $C^8H^{10}O^2 = 2C^4H^5O$, that is, twice as much common ether as in the first case. The alcohol enters into the reaction, and should be taken in equal weight to the weight of hydrobromic ether. Moreover, if alcohol is replaced by wood-spirit, we ought to obtain, not common ether, but ethylmethylic ether, $C^6H^8O^2$.

I devoted my attention to these varying results. The following are my experiments.

5.—(*a*.) 22 grammes of hydrobromic ether decomposed by 15 grammes of potassa and 12 grammes of absolute alcohol, after six or eight hours' contact at 212° F., furnished, on distillation, 12 grammes of common ether.

If the common ether had been produced by the substitution of the oxygen for the bromine, 22 grammes of hydrobromic ether should have given 7 grammes and a half of common ether.

But if the alcohol entered into the reaction, 22 grammes of hydrobromic ether should have given 15 grammes of common ether.

Now the weight of common ether found by experiment, is much greater than the weight corresponding with the direct substitution of the oxygen for the bromine, which proves that the alcohol had entered into the reaction. The weight found approached as nearly to 15 grammes as can be expected in experiments of this kind.

(*b*) 6 grms. of hydrobromic ether were decomposed at 212° F. by an excess of potassa and 2 grms. of absolute alcohol.

If the alcohol enters into the reaction, 6 grms. of hydrobromic ether require at least 2 grms. and a half of alcohol; 2 grms. would consequently not be sufficient, and the decomposition would remain incomplete, all other things being equal. This is what really occurred. After from ten to twelve hours of contact at 212° F., I obtained a liquid which was lighter than water, formed of a mixture of common ether and undecomposed hydrobromic ether.

Thus, in the same conditions of time and temperature, an excess will either completely decompose hydrobromic ether and change it into common ether, or give rise to an incomplete reaction, according to the relative proportion of alcohol.

(*c*) Hydrobromic ether, heated with potassa and wood spirit, gives ethylmethylic spirit. This finally proves the intervention of the solvent in the reaction, and the identity of the reaction with those of Dr. Williamson.

(*d*) I likewise prepared with glycerine, hydrobromic ether, and potassa, a peculiar ethereal compound, *diethyline*, $C^{14}H^{16}O^4$.

This compound was produced in the following manner :—

$$2C^4H^5Br + 2KO + C^6H^8O^6 = C^{14}H^{16}O^4 + 2KBr + 2HO.$$

The preparation of this body is remarkable, because glycerine enters into the reaction without being a solvent. In fact it neither dissolves hydrobromic ether nor diethyline.

e. The facts which I have just mentioned shew, I think, that the action of an alcoholic solution of potassa on hydrobromic ether is similar to that of potassiuretted alcohol. The substitution of potassa for potassium in Dr. Williamson's experiments, renders the preparation of the conjugate ethers which he discovered easier and less expensive.

On the Reproduction of Alcohol by means of Ether.

In a Memoir, published some months ago, (*see* "THE CHEMIST," February, 1854, p. 301), I proved that common ether, heated to 680° F., in closed vessels, with benzoïc, butyric, or palmitic acid, will combine with it and form benzoïc, butyric, or palmitic ether. These ethers, decomposed by potassa, reproduce alcohol.

The preceding reactions are direct, and do not appear to give rise to any accessory product. They transform, in general, but a small portion of the common ether employed.

I have endeavored to carry this transformation farther, by causing the intervention of sulphuric acid, the etherising agent *par excellence*.

When we distil a mixture of 1 part of ether, by weight, 3 parts of butyric acid, and 7 or 8 parts of sulphuric acid, water, a little butyric acid, and a great abundance of butyric ether pass over. Some olefiant gas is likewise disengaged. The reaction commences at 338° F. : at this temperature the mixture becomes black, and the simultaneous disengagement of butyric ether and olefiant gas commences. The temperature rises slowly towards 392° F. At 401° F. the mass carbonises entirely, and becomes a paste. I then stop the operation.

In these conditions, 10 parts, by weight, of common ether, gave .	17·9
Of butyric ether, which answers to 5·7 of transformed ether .	5·7
And 2·1 of olefiant gas,* answering to 2·7 of ether . . .	2·7
	8·4

One-sixth of the ether is lost or undergoes a special decomposition (black matters, oxide of carbon). The analogous loss of butyric acid, collected either free or in the state of butyric ether, amounted to nearly one-fourth.

The butyric ether obtained by this reaction is very pure : it boils at 246° F. Treated in a closed vessel with potassa, its decomposition is slow, and requires fifteen hours at 212° F. to be complete. The dis-

* 1·5 grs. of common ether gave, over water, 270 cubic centimetres of gas ; 100 parts of this gas, treated with bromine, were reduced to 6. The residue burns with a flame like oxide of carbon. Sulphurous acid appears likewise to be disengaged during the reaction.

tilled liquid gave me several grms. of alcohol, a volatile, inflammable liquid, with a characteristic odor and taste, mixing with water in all proportions, and separating from it on the addition of carbonate of potassa, &c. Its origin sufficiently explains its nature.

Thus, sulphuric acid causes a more complete combination of ether and butyric acid: it allows of the transformation into alcohol of the greater part (nearly two-thirds) of the first of these bodies.

Journal de Pharmacie et de Chimie, July, 1854.

On the Coloring Matters *of* Flowers. By M. E. Filhol.

White Flowers.—On subjecting, for some minutes, to the action of ammonia diluted with water, some flowers of the *Viburnum opulus*, of the *Philadelphus coronaria*, of the *Chrysanthemum vulgare*, of white Roses, and many other flowers, they assume a more or less vivid yellow color, which remains for a long time. The flowers of the *Viburnum opulus* acquire under this treatment as beautiful a yellow color as that of the *Cytisus laburnum*. The matter, which becomes yellow under the influence of alkalies, appears to extend throughout all white flowers; it has been merely as an exception that I have found some few flowers which, without being quite free from it, contain but little.

In variegated flowers, whose corolla presents some white portions, these generally take a beautiful tint under the influence of ammonia. The stamens, pistils, and, generally speaking, all the white parts of flowers, behave in a similar manner. The leaves themselves become yellowish, whenever they are accidentally deprived of chlorophylle. I proved this fact on the leaves of a plant of *Convallaria polygonatum*, which presented alternate streaks of white and green. The first became of a bright yellow tint under the influence of ammonia, in precisely the same manner as the flowers. The tissue of some fruits likewise becomes yellow, although in a less marked manner, under the influence of alkalies.

The most convenient method of transforming a white into a yellow flower consists in introducing it into a wide-necked bottle containing a little liquid ammonia, and thus subjecting it to the alkaline vapor. The change is then produced very rapidly. When the greater part of the flower is colored yellow, it may be removed from the bottle and exposed to the air; the portions which remain white become gradually yellow, and the tint is rendered uniform. The flower may likewise be steeped in slightly ammoniacal water, or into alcohol or ether mixed with a little ammonia. This latter method should be preferred when the flowers are covered with any fatty substance which would prevent an aqueous liquid from moistening them.

If, after rendering a white flower yellow, we steep it in acidulated water, it will soon resume its white color.

It is difficult, when making these experiments, not to recall to mind that, when dyers wish to fix the color of woad on a tissue, they add to

their liquor a little carbonate of soda, which brightens the tint considerably. It is easy to prove that acids, however weak, cause the disappearance of the greater part of the color from a decoction of woad. We might, consequently, be led to inquire whether the matter which communicates to white flowers the property of turning yellow on contact with alkalies might not be luteoline. This is a point which I intend hereafter to ascertain.

When the petals of white roses are boiled in distilled water, and a little carbonate of soda and sulphate of copper is added, as if preparing a bath of woad, a bright golden yellow liquid is obtained, which may be used for dying yellow. This liquid communicates to thread and cotton fabrics a very beautiful yellow color of considerable stability. Almost all white flowers furnish similar results.

The matter to which white flowers owe the property of becoming yellow in contact with alkalies dissolves very well in water, and still better in alcohol; it does not dissolve so well in ether. On removing the outer portion of the tissue of petals of white flowers, and examining them with a microscope, after subjecting them to the action of very dilute ammonia, all the cellules may be seen filled with a yellow juice, in which no granules are perceptible.

Dark Red Flowers.—When scarlet poppy flowers are subjected to the action of boiling-water or alcohol, a solution is obtained of a violet-red color. This solution takes a beautiful scarlet tint under the influence of even the weakest acids. If ammonia is poured into the liquor thus acidulated, it becomes of a beautiful violet color, without the least admixture of green; but if, instead of pouring ammonia into the previously acidulated liquor, it is added directly to the infusion, whether aqueous or alcoholic, of the flowers, it becomes of a very dull green. On exposing the flowers themselves to the action of ammonia they become of a beautiful violet, similar to that obtained with the previously acidulated infusion. The coloring matter of the poppy consequently differs considerably from the cyanine of MM. Fremy and Cloëz,* for alkalies do not render it green.

The flowers of the *Pelargonium zonale* become also of a beautiful violet under the influence of ammonia; their coloring matter behaves like that of the poppy. Those of the *Pelargonium inquinans* take a pure blue tint, without the least mixture of green. The dark red vervain cultivated in gardens communicates to alcohol a violet red tint. The alcoholic liquor treated with ammonia becomes of the color of wine leys with a slight greenish tinge. If the alcoholic infusion of these flowers is digested with a little dry hydrate of alumina reduced to powder, the alumina assumes a pale yellow color, and the supernatant liquor takes a beautiful red color under the influence of the acids, and a pure blue, without the slightest green tinge, under the influence of the bases. There must be, therefore, in the flowers of the vervain two distinct matters, one of which becomes blue under the influence of acids, whereas the other becomes yellow; it is the mix-

* *See* THE CHEMIST, June, 1854, p. 541.

ture of these two matters which causes the green color of the alcoholic tincture of these flowers.

The petals of the *Anemone hortensis* behave like those of the vervain. The flowers of the red Peony become of a pure blue under the influence of ammonia. These flowers are rapidly decolored by alcohol; the tincture which they furnish has but little color, but it becomes of a deep bright red, on the addition of the least trace of acid. The acidulated liquor becomes blue in contact with ammonia, whereas the unacidulated alcoholic solution has a greenish tinge. The petals of red roses, whose color is very dark, become blue when exposed to the action of ammoniacal vapors, but the color soon passes to a greenish blue.

Alcohol easily dissolves the coloring matter of roses, but acquires very little color. The smallest addition of acid communicates a deep red color to the alcoholic solution; ammonia poured into the acidulated liquor renders it of a greenish blue.

Pink Flowers.—The flowers contain a mixture of two juices, one of which is colorless in acid liquors, whereas the other is red; the first becomes yellow when mixed with the alkalies, the second becomes blue, and the mixture of these two colors produces the green tint observed. It does not require much practice, when acquainted with the above facts, to indicate beforehand the colors which would be assumed by pink or red flowers when exposed to the action of ammoniacal vapors. It is clear that the green color will be more yellow when the flower is pale, and will approximate more closely to blue when the flower is darker.

Blue Flowers.—What I have said with respect to pink and red flowers, will also apply to blue flowers. An examination of the tints which the petals of blue flowers assume under the influence of dilute ammonia, will suffice to shew that the green color thus developed is yellower in proportion as the flower is of a paler blue.

Effects of the Mixture of White and Colored Juices of Flowers.—On infusing in alcohol the flowers of irises, violets, peonies, *Cercis siliquastrum*, &c., we are struck with the want of richness of the alcoholic solution, even when the petals are completely decolored. It appears natural at the first glance, to attribute this decoloration to the influence of the alcohol, which would act on the coloring matter as a reducing body, but a more careful examination of facts prevents our being content with this explanation, and, without denying that alcohol may exercise the power attributed to it by MM. Frémy and Cloëz, I think that the following theory, either alone or combined with that of which I have just spoken, would enable us the more readily to explain the facts observed. In fact, if, instead of treating the above-mentioned flowers with alcohol, we treat them with boiling water, the aqueous solution is scarcely at all more colored than the alcoholic tincture; consequently, we should be obliged to consider water itself as a reducing body, which is by no means probable.

If we pour into these solutions, whether aqueous or alcoholic, the smallest quantity of a soluble acid, they become immediately of a

bright red, much deeper than the original liquor. The nature of the acid is of no consequence, and this is true to such an extent that even sulphurous acid will instantly brighten the tint and cause the reappearance of the color, which was, in fact, only concealed. The prolonged action of this last acid destroys the color. Is it to be believed that the coloring matter would reappear instantly under the influence of the smallest quantity of any acid *whatever*, if it were reduced? Could we, in accordance with this hypothesis, account for the action of sulphurous acid? I think not.

In my opinion, the decoloration is due to the mixture of the juice enclosed in the colorless cellules with that contained in the colored cellules. When we cause boiling water or alcohol to act on a flower, we destroy its organization; the juices contained in distinct cellules become mixed, and the coloring matter disappears. The following experiment supports this explanation.

If we take two equal volumes of a slightly acidulated infusion, whether aqueous or alcoholic, of peony-flowers, and dilute one with four times its volume of water, adding to the other four times its volume of an infusion of white flowers, we shall perceive that the color of this latter liquid will be much paler than the other.

The white juices then destroy, or rather conceal, the coloring matter. But do these juices act as reducing agents, or do they merely form colorless combinations? That is what the above experiments will enable us to determine, if I am not mistaken, for if there were reduction, sulphurous acid would not cause the re-appearance of the color. I therefore think that the coloring matter is not reduced, and that it forms a colorless compound with the elements of the white juices. In the infusions prepared by acting on the flowers, whether with alcohol or with water, one part of the coloring matter is free, whereas the other is engaged in the combination of which I have just spoken. It is easy to separate the colored part from that which is not so, by stirring into the liquid a little artificial phosphate of lime or dry hydrate of alumina; the colored part is first fixed, whereas the portion whose color is concealed remains in great part dissolved. If the liquid is filtered, it passes through quite colorless. It is easily colored red by acidulating it, and green or blue by pouring in an alkaline solution.

Comptes Rendus, No. 4, July 24th, 1854.

On Caprylamine. By William S. Squire.

The remarkable alcohol discovered by M. Bouis, in the reaction of hydrate of potassa upon castor oil, has been repeatedly analysed by himself and other chemists; the results obtained have been translated into the formulæ $C^{16}H^{18}O^2$, and $C^{14}H^{16}O^2$, whence it is evident that the new alcohol must be either caprylic or œnanthylic alcohol. The opinions of chemists remaining undecided upon this point, I was induced by Dr. Hofmann to study the ammonias corresponding to this

alcohol. The analyses of these compounds, and especially of the higher bases, appeared to be particularly calculated to solve this question; since in forming the fourth bases, for instance, we obtain a much greater difference in the carbon and hydrogen equivalents than in the alcohols from which they are derived. Thus:—

Caprylic alcohol . .	$C^{16}H^{18}O^{2}$	Œnanthylic alcohol . .	$C^{14}H^{16}O^{2}$
Iodide of Tetracapryl-ammonium . .	$C^{64}H^{68}NI$	Iodide of Tetrœnanthyl-ammonium . .	$C^{56}H^{60}NI$

The castor oil was saponified by means of solution of caustic potassa, and the resulting soap was distilled with a portion of caustic potassa amounting to about one-third of the weight of the oil; an energetic action ensued, with an abundant evolution of hydrogen gas and an aromatic oil distilled over, mixed with water. This oil, when separated and rectified, boils at the temperature of 354° F. The quantity of alcohol obtained in this way is about one-fifth of the oil originally employed. If the distillation be continued further, various empyeumatic products pass over, which I have not yet examined.

In order to determine the composition of the alcohol, it was considered desirable to form the ammonia bases of the series: with this object, the alcohol was saturated with an equal weight of iodine, and the solution decolorised by heating it with phosphorus in a water-bath. The oil thus obtained, when washed with water, is the iodide of the alcohol-radical in a state of tolerable purity. When this body is distilled in a retort, it is found to boil at the temperature of 379°F.; it does not, however, boil without some decomposition, hydriodic acid being copiously evolved, with formation, probably, of hydrocarbons of the series $C^{n}H^{n}$.

The iodide was then enclosed with alcoholic ammonia in strong soda-water bottles wired down, and exposed to the heat of boiling water: in about two days combination was effected. A portion was also tried with aqueous ammonia, but at the end of fourteen days no effect had been produced.

The bottles were then opened, and the contents distilled off to one-fourth; the residue was evaporated in a water-bath to a small bulk, in order to get rid of any free alcohol or iodide: it was then distilled with potash. The distillate, dehydrated by means of caustic potassa, and rectified, was a colorless liquid, of a strong alkaline reaction, bitter taste, and fishy odor, acting on the skin like potassa: it boiled at 327°F.; the sp. gr. was 0·786.

When dissolved in dilute hydrochloric acid, and mixed with bichloride of platinum, it furnished a platinum salt of a magnificent golden color, soluble to a certain extent in cold water, much more so in hot, pretty soluble in alcohol, and very much so in ether. It was purified by crystallisation from hot water, from which it was deposited in large thin plates. It furnished the following results on analysis:—

0·4816	grm.	yielded	0·1414	grm.	of platinum	= 29·36	per cent.	
0·1982	„	„	0·0584	„	„	= 29·46	„	
0·2485	„	„	0·2583	„	of carbonic acid	= 28·35	„	of carbon.
0·1982	„	„	0·2096	„	„ „	= 28·39	„	„

The base itself, when burned with oxide of copper, gave the following results :—

0·2000 grm.	yielded	0·5450	grm.	of carbonic acid	= 74·34	per cent.	of carbon.
„ „	„	0.2650	„	of water	= 14·72	„	of hydrogen.
0·1745 „	„	0·4750	„	of carbonic acid	= 74·24	„	of carbon.
„ „	„	0·2277	„	of water	= 14·49	„	of hydrogen.

If we compare the per-centages obtained with those calculated from the theoretical formulæ of caprylamine ($C^{16}H^{19}N$), and enanthylamine ($C^{14}H^{17}N$) we shall see that the results obtained correspond much more closely with that of the former base :—

	Mean of Experiments.	Theory. Caprylamine.	Enanthylamine.
Carbon . .	74·29	74·41	73·04
Hydrogen .	14·61	14·72	14.78
Nitrogen .	11·10	10·97	12·18
	100·00	100·00	100·00

PLATINUM SALT.

	Mean of Experiments.	Theory. Caprylamine.	Enanthylamine.
Platinum .	29·41	29·5	30·7
Carbon . .	28·37	28·6	30·6

These numbers, I think, must lead us to consider the base produced under the above mentioned circumstances as caprylamine, and to assign to it the formula $C^{16}H^{19}N$, and to its platinum salt the formula $C^{16}H^{20}N,PtCl^3$; and, therefore, to regard the alcohol from which it is derived, as the alcohol of the capryl series, a conclusion at which my friend M. Moschnin* has likewise arrived.

When I shall have completed the investigation of the other bases, and especially of the fourth, I hope to present still further experimental evidence of the view which I have ventured to advance.

On the Molecular Constitution *of* Tannin and Gallic Acid.

By M. E. Robiquet.

In my late researches on gallic fermentation (*see* "The Chemist," 1853-4, p. 339,) I left undecided the question whether tannin was a simple isomer of gallic acid, or a combination of gallic acid with a hydro-carburetted substance analogous to the sugars and gums. The following facts, which I had observed, were not of a nature to answer the question: in fact, whatever be the method employed to convert tannin into gallic acid, the metamorphosis is never complete; one part of tannin is transformed

* Ann. Pharm., lxxxvii. 8.

into an amorphous and mucilaginous substance, possessing some of the properties of the gums. If we operate by fermentation, this matter is found in the liquors; but if the conversion is effected under the influence of dilute sulphuric acid, there is produced, at the same time as the gallic acid, a substance analagous to the humic acids; and we are naturally led to think that the sulphuric acid has decomposed the tannin into gallic acid and a gummy matter, which it has afterwards carbonised.

I must, moreover, add, that no process enables us, exercising even the very greatest care, to obtain as hydrated gallic acid more than half the tannin employed. This last fact, the constant result of experiment, is a further argument adduced by those chemists who believe in the pre-existence of gallic acid in tannin, in which it would be under the same form as benzoïc acid in hippuric acid. In fact, the formula of tannin is confounded with that of gallic acid, dried at 212° F.; consequently, if the second of these bodies is merely an isomeric transformation of the first, the product obtained (gallic acid), should weigh at least the same weight as the product employed (tannin); I say, at least the same weight, because the metamorphosis being accomplished in water, the gallic acid is hydrated, and no longer corresponds to the acid dried at 212° F.

Nevertheless, on more minute examination, we find that in converting tannin into gallic acid, it is impossible to avoid decomposing a large proportion, and we easily see that the alteration of the tannic molecule is accomplished at once and without decomposition. Once modified in this manner, tannin no longer gives gallic acid, and is transformed into an amorphous, mucilaginous substance. It is for this reason that the gallic acid, obtained by Scheele's process, is far from corresponding in weight, even with half the tannin existing in the gall-nuts used. It is likewise for this reason that the tannin contained in oak bark, being already altered, never gives gallic acid.

Here are some experiments which appear to me convincing on this point:—

An aqueous solution of pure tannin was subjected, in a closed vessel, to the influence of pectase, and a temperature of from 77° F. to 88 F., for eight days. The small quantity of tannin not attacked was precipitated by gelatine, and the gallic acid by neutral acetate of lead. After twenty-four hours' respose, the complex precipitate was carefully separated from the supernatant liquor, which was clear and colorless. This liquor, tried with Soleil's saccharimeter, made no change in the tint of the plate with a double rotation, a certain indication that there no longer existed the smallest quantity of sugar or of gum. For greater security, I decomposed, with sulphuretted hydrogen, the acetate of lead contained in the solution; and, after filtering, I evaporated it with precaution; there was no residue, although I had used more than 200 grms. of liquid.

Thus, tannin contains neither sugar, gum, nor any other principle whatever in combination with the gallic acid. When, under the influence of the pectic fermentation, or sulphuric acid, the tannin is

transformed into gallic acid, a simple phenomenon of molecular metamorphosis, followed by hydratation, just as when cane sugar, subjected to the influence of the ferment of beer is transformed into grape sugar before its conversion into alcohol and carbonic acid.

I still have to confirm these views by the minute analysis of all the tannates and gallates which I can possibly obtain of a stable composition.

Journal de Pharmacie et de Chimie, July, 1854.

STUDIES *on the* SALICYLIC ETHERS. By M. CH. DRION.

IN a Memoir presented to the Academy on the 2nd January, 1854, M. Gerhardt mentions some new salicylic compounds, obtained by causing certain organic chlorides to react upon oil of gaultheria or salicylate of ethyle.

For example, on heating oil of gaultheria with chloride of benzoïl, a compound is obtained, differing from the first only by the substitution of one molecule of benzoïl for one molecule of hydrogen.

On comparing these facts with those observed by M. Cahours, to ascertain whether it would be possible, in the salicylic ethers, to replace one molecule of hydrogen by a metallic molecule, M. Gerhardt thought that oil of gaultheria should be represented, not by a molecule of water, in which half the hydrogen is replaced by salicyle, and the other half by methyle, but by a molecule of water in which half the hydrogen should be replaced by the methylsalicylic group, the half being capable of being, by double decomposition, substituted by a metal or by a radical, such as benzoïl. The constitution of oil of gaultheria would therefore be :—

$$\left.\begin{matrix} C^7H^4(CH^3)O^2 \\ H \end{matrix}\right\} O ;$$

That of M. Cahours' gaultherate of potassa :—

$$\left.\begin{matrix} C^7H^4(CH^3)O^2 \\ K \end{matrix}\right\} O$$

That of benzoate of methylsalicyle :—

$$\left.\begin{matrix} C^7H^4(CH^3)O^2 \\ (C^7H^5O \end{matrix}\right\} O$$

By the same process M. Gerhardt procured cuminate and succinate of methylsalicyle.

The above applies equally to salicylate of ethyle. By treating this body with chloride of benzoïl, benzoate of ethylsalicyle is produced :—

$$\left.\begin{matrix} C^7H^4(C^2H^5)O^2 \\ C^7H^5O \end{matrix}\right\} O$$

which demonstrates that salicylate of ethyle has the same constitution as salicylate of methyle.

M. Gerhardt having expressed to me a desire to see his results confirmed and generalised by fresh experiments, I undertook the analysis of the above compounds ; the numbers which I found accord perfectly with those previously obtained.

As salicylate of ethyle had only been subjected to the action of chloride of benzoïl, I have acted on this body with chloride of succinyle. I chose this chloride in preference to any other, because, succinic acid being bibasic, the composition of succinate of salicyle derives from it not one sole molecule of salicylate of ethyle, but two of those molecules in which all the free hydrogen is replaced by the succinylic group. The formula of *succinate of ethylsalicyle* is therefore—

$$\left.\begin{matrix} C^7H^4(C^2H^5)O^2 \\ C^7H^4(C^2H^5)O^2 \end{matrix}\right\} O,$$
$$\left.\begin{matrix} C^4H^4O^2 \end{matrix}\right\} O.$$

This body crystallises in long needles; it is insoluble in water, scarcely soluble in ether, very soluble in boiling alcohol. It may be boiled in a concentrated aqueous solution of potassa without any alteration.

Finally, I tried to produce *salicylate of amyle*, so as to ascertain whether this ether will enter into the same combinations as the preceding. Experiment has, in this respect, fully justified my opinion.

Endeavors had been previously made to produce salicylate of amyle by the ordinary processes for the preparation of ethers, but without success. I succeeded in preparing it by causing chloride of salicyle to act on amylic alcohol. It is important, for the success of the operation, to act only on small quantities of matter at a time, otherwise the reactions are made in a confused manner, and but little ether is obtained, whereas we obtain a great number of products of decomposition, amongst which will be found an abundance of hydrate of phenyle.

Salicylate of amyle is a very refringent, colorless liquid, heavier than water, in which it is insoluble; its odor is agreeable; it boils at 518° F.

Treated with a boiling, very concentrated solution of potassa, it disengages amylic alcohol and salicylate of potassa remains. Its composition is explained by the formula

$$\left.\begin{matrix} C^7H^4(C^5H^{11})O^2 \\ H \end{matrix}\right\} O$$

When treated cold with a concentrated solution of potassa, it becomes a mass, and gives the body

$$\left.\begin{matrix} C^7H^4(C^5H^{11})O \\ K \end{matrix}\right\} O$$

which is analogous to the gaultherate of potassa of M. Cahours.

Finally, treated with chloride of benzoïl, it behaves like other salicylic ethers, and gives *benzoate of amylsalicyle*—

$$\left.\begin{matrix} C^7H^4(C^5H^{11})O^2 \\ C^7H^5O \end{matrix}\right\} O$$

This product remains viscid for a very long time, and solidifies with great difficulty.

The *chloride of salicyle* which I employ for the preparation of salicylate of amyle, was obtained for the first time by M. Gerhardt, by

causing perchloride of phosphorus to act on oil of gaultheria. In this remarkable reaction only traces of oxychloride of phosphorus are formed; but much hydrochloric acid is disengaged, and I have likewise ascertained the abundant production of chloride of methyle. Chloride of salicyle may be heated to about 392° F., without decomposing, but it cannot be distilled.

For the purpose of obtaining it pure, I tried to distil it under a lower pressure than that of the atmosphere. But abundant fumes of hydrochloric acid soon came from the pump, and forced me to renounce the use of this apparatus. I continued the distillation under atmospheric pressure, and collected in the receiver a fuming liquid, presenting all the characters of the organic chlorides.

When heated with water, it attacks this liquid very powerfully, and dissolves in it. After cooling we obtain crystals formed of a mixture of salicylic and chlorobenzoïc acids. The first of these acids being far more soluble in water than the second, the latter may be obtained perfectly pure by repeated washings of the mixed crystals. The analysis of the *chlorobenzoïc acid* enabled me to determine that the chloride resulting from the decomposition of the chloride of salicyle was *chloride of chlorobenzoïl* C^7H^4ClO. It had already been obtained by M. Chiozza, by causing perchloride of phosphorus to act on salicylic acid.

It is impossible to separate chloride of chlorobenzoïl from chloride of salicyle by distillation. A portion of the latter is decomposed in each rectification, and the boiling points of each appear to approximate closely to each other. Nevertheless, chloride of chlorobenzoïl appears to be the less volatile of the two; if we collect, separately, the part which distils above 482° F., we find, on treating it with water, that it is transformed into almost pure chlorobenzoïc acid. Doubtless we should obtain chloride of chlorobenzoïl in a state of purity by treating the chlorobenzoate with oxychloride of phosphorus. But the extreme irritation of the eyes and chest caused by manipulating the volatile chlorides prevented me from continuing my experiments.

Finally, to acquire a complete certainty as to the production of chloride of chlorobenzoïl, I have caused this body to act on carbonate of ammonia, so as to produce *chlorobenzamide*. This experiment succeeded admirably. Chloride of salicyle does not produce amide when in contact with carbonate of ammonia: the reaction is very great, and we only obtain salicylate of ammonia and some other products, all soluble in water. According to that, by treating carbonate of ammonia with chloride of chlorobenzoïl containing chloride of salicyle, the only body insoluble in water which is formed, is chlorobenzamide.

This new amide crystallises, in very beautiful pearly needles, from its solution in alcohol or ammonia. Treated with boiling caustic potassa, it disengages ammonia.

Comptes Rendus, No 2, July 10th, 1854.

On the ACTION *of the* PROTOSALTS *of* IRON *on* NITRONAPHTHALINE *and* NITROBENZINE; *a new Method of forming the artificial organic bases of Zinin.* By M. A. BÉCHAMP.

I HAD lately the honor of presenting to the Academy a note, in which I shewed that by causing the salts of protoxide of iron to react on pyroxyline and the analogous nitrous products derived from starch and gum, we easily reproduce the original substances, cellulose, gum, and starch. This is especially remarkable with gun cotton. All the nitrogen of the product is disengaged in the state of pure binoxide of nitrogen. At the same time, the protoxide of iron is peroxidised, and the cotton is regenerated, preserving its texture and ordinary physical properties.

The nitrogenous products of which I speak, behave, therefore, in these circumstances, like nitrates. We know that the nitrates, in the presence of the salts of protoxide of iron, disengage all their nitrogen in the state of pure binoxide of nitrogen, whilst the protoxide of iron becomes peroxidised under the influence of the remainder of the oxygen of the nitric acid.

If we use reducing substances of another nature, especially hydrosulphuric acid, or pyroxyline, very different results are produced.

I studied the action of the protosalts of iron on the nitrous products derived from the hydrocarbons, such as nitronaphthaline and nitrobenzine. The reaction was quite different to that with pyroxyline. It more nearly resembled that of sulphuretted hydrogen. All the nitrogen of the nitrous product remained combined with the elements of the hydrocarbon and the corresponding organic base was produced. But, whereas the production of aniline by sulphuretted hydrogen is very difficult, it is made, on the contrary, with remarkable ease, by means of the protosalts of iron; and, I think, that of all the processes for the preparation of this base, so often studied by chemists, there is none which is so convenient and so inexpensive as that which I propose. This has been my motive for publishing this note now, although it belongs to a more extensive work in the same direction.

The preparation of naphthalidam by nitronaphthaline, by means of this new method, is likewise very easy; and there is reason to believe that the process which I am about to describe may be usefully applied to the production of the artificial organic bases derived from the hydrocarbons.

The sulphate, oxalate, and protochloride are without any perceptible action on nitronaphthaline and nitrobenzine, the only bodies on which I have as yet operated.

But it is no longer the same if we take a salt of iron produced by a weak acid, the acetate of the protoxide for instance. This salt is powerfully attacked: it does not disengage gas, sesquioxide of iron is precipitated, and naphthalidam or aniline is formed.

I give in my Memoir, which will shortly be published, all the practical details of these preparations. The cost of aniline, by using commercial benzine, seldom exceeds 20 francs per kilogramme.

I shall shortly investigate the action of the protosalts of iron on the nitric derivatives of benzoïc acid and its homologues, as well as on the superior degrees of nitric compounds of the various hydrocarbons.

Comptes Rendus, No. 1, July 3rd, 1854.

ANALYTICAL CHEMISTRY.

ESTIMATION *of* POTATO FECULA *mixed with* WHEAT FLOUR.

By M. CAILLETET.

A GIVEN weight of the flour to be tested is mixed in a graduated burette with a given volume of an aqueous solution of potassa. There is afterwards added a likewise determined quantity of an alcoholic solution of bromine. After some time, a precipitate is formed, the volume of which is noted. Now, as we know beforehand the very different volumes of the two deposits which would be formed, in such circumstances, on one hand, with pure wheat flour, and on the other with flour mixed with one-fourth of fecula, we may conclude, by a simple arithmetical operation, the proportions of fecula in the flour submitted to examination.

Comptes Rendus, July 31, 1854.

INORGANIC CHEMISTRY.

On some LITTLE-KNOWN REACTIONS *of* BORACIC ACID.

By M. CHARLES TISSIER.

THE work which I have the honor of submitting to the judgment of the Academy is the result of some observations which I have been enabled to make on the dissolving action of boracic acid by the humid way, an action which is in general but little known; I hope thus to draw the attention of chemists to this acid, whose very peculiar properties may be rendered of great use in analysis. I intend, if my other occupations will allow me, to make a complete work on the various reactions produced by boracic acid in solution, that is to say, to study its action on the most widely diffused binary and tertiary compounds. In the meanwhile, the following results will serve to give an idea of the manner in which this acid acts upon several bodies which are insoluble, or sparingly soluble, in water, such as lime, magnesia, alumina, protoxide of manganese, protoxide and sesquioxide of iron, protoxide of zinc, the carbonates of baryta, lime and magnesia, and finally phosphate of lime.

I.—*Boracic Acid and the Protoxides.*

Boracic Acid and Lime.—Hydrated lime dissolves very readily in a boiling solution of boracic acid; the proportion of the crystallised acid necessary for rendering the solution complete is from twenty-five to thirty times the weight of the lime.

Boracic Acid and Magnesia.—The hydrocarbonate of magnesia is the body which this acid dissolves with the greatest ease; it is not the same with calcined magnesia, which resists the action of this acid for a long time.

Protoxide of Iron and Protoxide of Manganese.—These two bases (hydrated, be it understood) dissolve readily in a boiling solution of boracic acid. A very considerable relative proportion of boracic acid is, however, necessary (twenty-five to thirty times its weight for protoxide of manganese, and from fifty to sixty times for protoxide of iron.) The solution of the acid borate of manganese appears to be unalterable in the air; that of the acid borate of iron becomes, on the contrary, immediately cloudy and deposits sesquioxide.

Boracic Acid and Protoxide of Zinc.—Oxide of zinc dissolves in a boiling solution of boracic acid, even after being calcined at a red heat; in the same way as for protoxide of iron, the proportion of boracic acid requires to be fifty or sixty times the weight of the oxide.

II.—*Boracic Acid and Sesquioxides.*

Alumina and Sesquioxide of Iron.—Boracic acid in solution does not dissolve the least trace of these two oxides, whether we operate directly on the hydrated oxides, or whether we operate by double decomposition by precipitating a salt of one or other of these two oxides, by borate of soda in the presence of an excess of boracic acid.

III.—*Boracic Acid and Anhydrous Carbonates.*

Carbonate of Baryta and Carbonate of Lime.—Boracic acid has so little action on these two bodies, that it may be considered as having none.

Carbonate of Magnesia.—I have said above how boracic acid acts on the hydrocarbonate. It appears to be without action on dolomite.

IV.—*Boracic Acid and Insoluble Phosphates.*

Phosphate of Lime.—The action of boracic acid on this salt is without contradiction one of the most interesting, for it enables us to form phosphoric acid into a compound whose formula is constant.

In fact, if to an acid solution (hydrochloric or nitric) containing phosphate of lime (a soluble phosphate or chloride of calcium) and an excess of boracic acid, we add enough borate of soda to saturate the acid which holds the phosphate in solution, it does not precipitate borate of lime, whereas all the phosphoric acid is precipitated under the form of phosphate of lime. The phosphate of lime precipitated in these circumstances, has not a variable composition as when it is precipitated by saturating with ammonia, but it has a constant composition and a well defined formula. It corresponds to that to which M. Berzelius assigns the formula $8CaO, 3PhO^5$, and which contains, calculating with equivalents such as are now admitted:

Phosphoric acid	49·09
Lime	50·91
	100·00

This mode of precipitating phosphoric acid from its solutions, joined

to a process which I submitted to the Academy last year, for separating this acid from alumina and sesquioxide of iron, renders very easy the estimation of earthy phosphates, manures, &c.

Comptes Rendus, No. 4, July 24th, 1854.

INDUSTRIAL CHEMISTRY.

On CHINESE GUNPOWDER. By M. RONDOT.

WE find the composition of Chinese gunpowder given in many works on chemistry; the proportions which I have most frequently seen are as follows:—

Saltpetre	61·52
Charcoal	23·10
Sulphur	15·38

I visited the government gunpowder manufactury at Canton, and noted the quantities employed at the moment that they were weighed; the proportions always appeared to me the same for war-powder; hey were as follows:

Salpetre . . .	16 *catties*	=	77·1
Charcoal . . .	40 *taëls*	=	12·0
Sulphur . . .	36 *taëls*	=	10·9

The price at Fo-Kienn of 10,000 *catties* of powder is 500 *liang* of silver, or 60 centimes the kilogramme.

Since the war of 1842 the Chinese have manufactured fulminating capsules, the existence of such a manufacture in China is a curious fact hitherto unknown.

The Chinese use fulminate of silver for the preparation of the priming; obtaining this fulminate by dissolving silver in hot nitric acid, and adding *peh-siao* (rice brandy), distilled two or three times.

Many travellers have asserted that the more energetic mineral acids had never been produced in China, and were even almost unknown there; I saw the nitric acid necessary for the manufacture of the fulminate, prepared by Pwann-tse-Ching, and I was much surprised, for I did not expect to find, at Canton, a laboratory conducted by a Chinese, and in which are incessantly employed four apparatus, with earthenware and glass retorts made in the country.

They follow our old process of manufacture, the decomposition of the nitre by argil, and the isolation of the nitric acid by the formation of an aluminate of potassa.

Into an earthenware retort are put 8 *liang* (302·32 gr.) of a very aluminous argil, and 16 *liang* (604·64 gr.) of refined saltpetre. The retort consequently contains a total weight of 907 gr. The body of the retort is covered with a luting composed of sugar, argil, and *samchou*. The apparatus being prepared, the fire is applied.

In about six hours they state that they obtain 15 *fenn* (56·69 grs.) of acid, and Pwann pretends that each *liang* of acid costs him 2 *liang* of silver, that is to say, that the 100 grms. cost him 40 francs 40 centimes.

The preparation of nitric acid, which the secretary of Pwann always designated to me as the *medicine, the very strong medicine*, had been taught them by a stranger from Macao.

The Chinese use for gunpowder and nitric acid, only the purest nitre which costs 8 *taëls* the *picul* (1 franc the kilogramme); they refine this substance in the following manner, and both the manufacture and sale of it are monopolies conceded at a high price.

A cast iron pot set over a brick furnace is filled with spring water, and into it are put 60 *catties* of crude Indian saltpetre, and 4 or 5 *catties* of rasped radishes. After boiling for an hour these are taken out; three hours after a thick solution of glue is poured in, and the scum is removed as it rises. The boiling is continued until the liquor attains a certain degree of concentration. It is then poured into an earthen vessel to crystallise; it is decanted, and the cake of saltpetre is subjected to two or more crystallisations. If we are to believe the workmen, nitre cannot be obtained in beautiful, clear crystals, without adding to the bath a little Indian camphor, and *pet-siao* distilled three times. I received from Pwann some extremely limpid crystals in prismatic fasciculi 24 centimetres in length.

Journal de Pharmacie et de Chimie, July, 1854.

On the Resistance *which* Hydraulic Lime *and* Cement *offer to the* action *of* Sea Water. By MM. Malaguti and Durocher.

For some years past the attention of learned men and builders have been occupied by the destructive action which sea water exerts on hydraulic mortar. In endeavoring to explain this disastrous phenomenon, M. Vicat has shewn that sea water acts by its tendency to dissolve the lime of the mortar which is then replaced by magnesia; but hitherto no one has indicated any efficacious means of preventing or neutralising this dissolving influence: we only know that in general the most powerful hydraulic mortars, the cements or mixtures of lime and pouzzolanes which adhere the most rapidly, are those which appear to resist best the causes of decomposition. Nevertheless, even among the cements of an equally rapid adherence and of nearly equal strength, there are some which possess very different powers of resistance, without our being able *à priori*, or by analysis, or a short trial, to ascertain in which we can place entire confidence.

Amongst these uncertainties, M. Durocher and I thought that by studying those cements which resist the decomposing influence of sea water, conjointly with the analysis of those hydraulic limes and cements which do not present that resistance, as well as the products of the decomposition, we might hope to throw some light on a question whose difficulty equals its importance.

The samples, to the number of sixteen, on which we have experimented are, the hydraulic limes of Paviers and Doué, the mortars composed from them, Boulogne, Portland, Pouilly, Vassy and Parker's cements; we are indebted for them to MM. Jebuvier, Watier, and Bellanger, to whom we return many thanks.

The course which we have followed in our investigations consists in examining the modifications in the proportions of the various elements, by comparing the composition of the products immersed in sea water with that of similar substances which have not been plunged in it; but as we had no samples of lime and sand mortars which had been hardened in soft water, to compare them with those which had been immersed in sea water, the examination of these latter could only be made by comparing their composition with that of the lime used in their fabrication. In these comparisons we have been obliged to abstract the sand and bring the composition of the mortars to what it would have been had there been no sand in them. We cannot give all the results to which our analyses have led us, and which are given in full in our Memoir; we only mention the most important ones, which will shew how complicated these phenomena of decomposition are.

Two cylinders of Paviers hydraulic lime were immersed in sea water for eighteen months in precisely similar conditions. One had lost an enormous quantity of lime and had gained very little magnesia, but, on the other hand, it had fixed a quantity of carbonic acid almost equivalent to the two earthy bases. As for silicic acid, a considerable portion had been removed, and a little alumina. It would appear that a hydrosilicate of alumina had separated from the mortar at the same time as the lime, whilst carbonic acid had replaced the elements which had disappeared. The alteration of the other cylinder was much less considerable; the loss in lime, and the gain in carbonic acid, had been less; but the quantity of magnesia substituted for lime was twice as great, and the loss of silicate of alumina was rather less. An analogous fact was repeated with a mortar made with this same Paviers hydraulic lime.

Two prisms of this mortar were immersed for eighteen months in sea water. One of the two prisms was but little affected, whereas the other was found in a very advanced state of decomposition. However, in the prism which was the least altered, we proved that the lime had been eliminated, that a large proportion of carbonic acid had been fixed, and that the proportions of magnesia, silica, and alumina, had undergone but little change. The prism whose alteration was far advanced, had undergone a complete transformation in its composition. A considerable quantity of lime had been replaced by an atomically larger proportion of magnesia, and the carbonic acid had not changed perceptibly: the silicic acid and alumina had augmented considerably.

To explain these very different results, can we assert the non-homogeneousness of the hydraulic lime which was used in these experiments? We must mention that in the quarries of Paviers the various strata of hydraulic lime have not the same composition. The alteration undergone by the mortar produced by Doué lime is explained by a considerable loss of lime without any substitution of magnesia, and by the fixing of a large quantity of carbonic acid.

As for the alteration of cements, Boulogne cement, previously hardened in fresh water, began to crack after having been immersed

for eight months in sea water; still its chemical composition had undergone no perceptible change.

It was very different with Portland cement, which, under the action of sea water, cracked, fixed almost as much carbonic acid as it originally contained, and finally lost a little lime, which was replaced by a small quantity of magnesia. Finally, a mortar prepared with 1 vol. of Portland cement, and 2 vols. of quartz sand, after being immersed in sea water for a year, presented no alteration, except that of having become richer in carbonic acid.

To sum up, the facts which we have now mentioned, and those detailed in our Memoir, shew that the decomposition of lime cements and mortars by sea water do not always take place in the same manner; the substitution of magnesia for lime often takes place, but not always, and, as it is accompanied by the addition of carbonic acid, the altered mortar presents the re-union of a *hydrosilicate of alumina* and of a *double carbonate* which approaches the composition of *dolomite*. But there are cases in which the lime disappears without the introduction of magnesia, and then phenomena appear to take place as if it were in a water which contained no salt, but which was charged with carbonic acid. Moreover, in the alteration of moderately hydraulic mortars, the elements of the mortar are divided into two compounds, one rich in earthly carbonates, the other rich in alumina, forming, on the surface, a snowy deposit, which the waves remove. This division does not take place, or, at least, it is produced very slowly in very hard cements and mortars which adhere rapidly. The alteration manifested by these latter consists simply in the mass cracking, and the disappearance of a small quantity of lime, with or without being replaced by magnesia, and in either case it tends to produce an elimination of volume, wherein results the cracking of the mass.

We have still to speak of the cements which are considered as best resisting the action of sea water. Hitherto the cements of Pouilly, Vassy, and Parker, have passed for the most stable. One circumstance struck us during the analysis of these three cements: they are very rich in oxide of iron, and that of Parker, which resists the best, contains the greatest quantity. We found about 7 per cent. of oxide of iron in the cements of Pouilly and Vassy, and very nearly 14 per cent. in that of Parker. This led us to ask whether the presence of oxide of iron does not contribute largely to give those cements the property of resisting the decomposing influence of sea water. To justify this idea, it was necessary to execute experimental investigations, by making ferruginous mortars, and exposing them to the action of sea-water; but it was necessary first to ascertain whether the oxide of iron contained in cements and mortars, does not behave like an inert matter. Thus we had to investigate to what point this oxide is susceptible of forming, by the hurried way, combinations with lime. For this purpose, we formed a species of ponzyolane of mixtures of silica and a little lime with alumina and oxide of iron; then we studied the action of lime water on these mixtures previously heated to dull

redness. After having been immersed for some time, these substances had augmented in volume, and shewed the most remarkable characteristics. Each of them had divided into two distinct compounds; one had sunk to the bottom of the vessel, and had contracted very considerable cohesion and adherence; whereas, the other had assumed a flocky aspect, and, swelling more and more, had raised itself 15 or 16 centimetres from the bottom. On analysing the different compounds, we found that the quantity of lime precipitated is independent of the presence of alumina, whereas it is augmented by the presence of oxide of iron; moreover, we found that the flocky compound was the richest in alumina, and the concreted deposit contained most oxide of iron.

The activity of the oxide of iron in hydraulic materials appearing demonstrated by these synthetical experiments, we thought that we might conclude that the presence of this oxide contributes to give stability to mortars and cement immersed in sea water. It is true that we still have to prove whether cements or artificial hydraulic limes, formed by the admixture of lime with ferruginous argil, or mixtures of argil and hydrated oxide of iron, or again mixtures of argil and substances capable of engendering oxide of iron, are unattackable by sea water. But these experiments require a great deal of time, and we have thought it best to make known the results which we have already obtained, because they may be useful to builders of hydraulic works; and, moreover, because we much wish that they should be practically verified. Whatever the future may prove with respect to our inferences, the following facts are distinctly proved.

1st. The cements, which are considered as offering the most resistance to the destructive action of sea water, always contain notable quantities of oxide of iron.

2nd. Certain combinations of silica, alumina, and lime, give, *ceteris paribus*, very different reactions, according to whether they contain much, or are free from, oxide of iron.

Comptes Rendus, No. 4, July 24th, 1854.

AGRICULTURAL CHEMISTRY.

Researches on Vegetation. By M. Boussingault.

(*Concluded from page* 671.)

Experiment made with White Lupins.—I took the weight of a certain number of seeds; after each weighing, each seed was wrapped in a paper, numbered, and put into a flask.

Estimation of the Nitrogen in the Seed.—Normal acid equivalent to 0·0875 of nitrogen.

I. One seed weighed 0·413 gr.

		cc	
Amount of acid,	Before the experiment	32·7	
	After . . .	23·6	
	Difference. . .	9·1	equivalent to nitrogen 0·0248 grs.; 5·90 pr. ct.

II. Three seeds weighing 1 gramme.

		cc	
Amount of acid,	Before the experiment	34·8	
	After . . .	11·8	
	Difference . . .	23·0	equivalent to nitrogen 0.0578 gr.; 5·78 pr. ct.

III. One seed weighing 0·335 gr.

		cc	
Amount of acid,	Before the experiment	34·8	
	After . . .	27·3	
	Difference	7·5	equivalent to nitrogen 0·0189; 5·64 per cent.

IV. One seed weighing 0·374 gr.

		cc	
Amount of acid,	Before the experiment	34·8	
	After	25·95	
	Difference	8·85	equivalent to nitrogen 0·0223; 5·96 per cent.

Mean of the four experiments.—Nitrogen per cent. 5·82.

Vegetation of the Lupin during six weeks.—(*First Experiment.*)—Seed No. 12, weighing 0·410 gr. } 0·825 gr. should contain 0·0480 of
Seed No. 13, weighing 0·415 gr. } nitrogen.

The seeds were put into the apparatus on the 17th of May. The pummice soil was mixed with the ashes of dung and of lupin seeds.

June 3.—The two plants were very fine. The leaves, as well as the cotyledons were of a deep green.

June 18.—The vegetation was magnificent.

June 25.—From the 18th, the cotyledons began to lose their beautiful green color, and were now discolored; five perfectly yellow folioles had fallen from one of the leaves. The plant was still vigorous; there were several buds.

June 28.—Since the cotyledons had lost their green color, they had become wrinkled; as several folioles had now fallen, the experiment was terminated.

The height of the lupins, above the soil, was from 15 to 16 centimetres. The roots were highly developed, one of the fibres being 30 centimetres long: the petioles were 7 to 8 centimetres, and each plant bore seven of these petioles. The color of the leaves was not so deep as that of the plant grown in the open air and in a manured soil. There remained scarcely any vegetable débris in the pummice.

After dessiccation, one of the plants weighed . .	0·76 gr.
„ the other plant weighed . .	0·96 „
Dry crop	1·72

Estimation of the Nitrogen of the Crop.—Ten cubic centimetres of normal acid were equivalent to 0·0875 grs. of nitrogen. The whole of the crop was operated on.

Amount of acid, Before the experiment	32·7	
After . . .	14·9	
	17·8	equivalent to nitrogen 0.0472 grs.

Estimation of Nitrogen in the Soil.—Ten cubic centimetres of the normal acid employed for estimating the nitrogen of the soil, were equivalent to 0·04375 grs. of nitrogen. This acid being saturated with about 32 cubic centimetres of alkaline liquor, it is evident that each tenth of a cubic centimetre of the burette represented 0·13 milligrammes of nitrogen. Admitting, in the most unfavorable cases, an error of two divisions, from the determination of the strength, we see that we may certainly answer for 0·3 milligrammes of nitrogen in the estimation. Owing to the matter of the soil containing very little nitrogen, I preferred using more dilute, and, consequently, more delicate normal liquors.

The pummice used, weighed, when dry, 114·90 grs. This was analysed in five portions of 22·98 each. The operation was performed without accident, and the whole of the ammonia produced in the five combustions was condensed into a single pipette of normal acid.

	cc	
Before the experiment	32·2	
After . . .	31·7	
Difference . . .	0.5	equivalent to nitrogen 0·0007 gr.

Summary of the first experiment.

In the gathered plants . . .	nitrogen .	0·0476 gr.
In the soil	„ .	0.0007 „
In the crop, nitrogen . . .	„ .	0·0483 „
In the seeds	„ .	0·0480 „
Increase of nitrogen during the vegetation . .		0·0003 „

Conclusion.—No appreciable quantity of nitrogen was fixed during the vegetation.

Vegetation of lupins during two months.

Second experiment.—On the 25th of May, I planted in pummice soil in one of the larger apparatus B, six seeds of white lupin. These seeds weighed, No. 2, 0·354 gr.; No. 7, 0·358 gr.; No. 18, 0·375 gr.; No. 19, 0·370 gr.; No. 15, 0·372 gr.; and No. 17, 0·373 gr.; total weight, 2·202 gr. which should contain 0·1282 gr. of nitrogen. The pummice was mixed with ashes of dung and of lupin seeds. The flask in which the vegetation was carried on was of the capacity of 86 litres; the confined atmosphere contained consequently about 7 litres of carbonic acid gas at the commencement of the experiment.

June 3.—The six lupins had sprung up.

June 26.—The vegetation had a fine appearance; the cotyledons were full, and of a deep green.

July 7.—For several days the cotyledons had been assuming a yellow color; several folioles were discolored; two of the small leaves had fallen.

July 21.—The six plants were remarkably fine; the few leaves which were detached were replaced by new shoots; there were several leaf-buds on each plant. The cotyledons had perished, and nearly separated from the stems.

As the vegetation seemed to have arrived at the point at which, in a soil containing no manure, it remains stationary, and at which all that is developed lives at the expence of that which dies, I terminated the experiment.

The height of the lupins was from 20 to 25 centimetres; some of the radicular fibres were 40 centimetres in length. Each plant had seven or eight petioles furnished with leaves, and the stems terminated with a bud.

After having removed the six plants and collected the detached folioles, there remained in the soil numerous *débris* of the roots. But, during the dessiccation of the pummice soil, the presence of ammonia could not be detected. The six plants, dried, with the detached leaves, weighed 6.73 grs.

Estimation of Nitrogen in the Crop.—The analyses were made in tubes of Bohemian glass of large dimensions, in order to admit of a large proportion of soda lime being used, and by operating successively on half of the plants gathered. Ten cubic centimetres of normal acid were equivalent to 0·0875 of nitrogen.

First half of the crop :—		cc	
Amount of acid, Before the experiment		32·6	
After . . .		16·0	
Difference . .		16·6	equivalent to nitrogen 0.0446 gr.
Second half of the crop :—			
Amount of acid, Before the experiment		32·6	
After . . .		18·4	
Difference. . .		14.2	equivalent to nitrogen 0·0381 gr.
In the plants gathered, nitrogen . . .			0·0827 „

I made two operations on account of the weight of the matter, and also in order to avoid the risk of losing, by accident, the result of an experiment so successfully concluded. It is evident that the two estimations did not give nearly the same proportion of nitrogen, although the matter was divided into two equal parts. This was probably because the mixture of the roots, leaves, petioles, stems, and shells remained imperfect, although all the parts of the plants were cut very small. The two analyses were carefully conducted, powerfully

heated, and the cleansing by the gas arising from the decomposition of 3·50 grs. of oxalic acid being continued. The tubes having been broken after cooling, I ascertained that there did not remain any perceptible quantity of charcoal mixed with the soda lime.

Nothing shews better than the difference established in these analyses, how preferable it is, in such delicate researches, to operate on the whole of the plants gathered, to employing only even a very large portion. Indeed, if we had determined the quantity of nitrogen in the six lupins from either analysis, we should have obtained, by doubling the result :—

In one case, nitrogen . . .	0·0892 gr.
In the other „ . . .	0·0762
Difference	130

Estimation of the Nitrogen of the Soil.—The pummice soil, after dessiccation, weighed 840 grms. Ten cubic centimetres of normal acid were equivalent to 0·04375 gr. of nitrogen.

I heated at each time 42 grms. of pummice mixed with soda-lime; it was estimated after having collected in the normal acid the ammonia resulting from five operations. Two large Bohemian glass tubes, which were allowed to cool slowly, sufficed for this long and tedious work.*

630 grms. of the pummice soil contained 0·314 gr. of nitrogen; allowing 0·105 gr. for the remaining 210 grms. of soil, we have—

In the 840 grms. of pummice soil, nitrogen, 0·419.

Summary of the Second Experiment.

	grms.
In the plants gathered, nitrogen	0·0827
In the soil	0·0419
In the crop	0·1246
In the six seeds	0·1282
Loss of nitrogen during the culture . . .	0·0036

Conclusion.—No nitrogen was fixed during the vegetation.

Vegetation of the lupin during seven weeks.

Third Experiment.—On the 4th of June, in the prepared pummice, containing ashes of dung and of lupin seeds, we planted seeds No. 1 and No. 20. Each weighing 0·300 grms. = 0·600 grms., which should contain 0·0349 of nitrogen.

* I performed, without any assistance, all the estimations of nitrogen mentioned in this third series of my researches, and I relied only on myself for mounting the apparatus and conducting the observations, in the three years which have just terminated.

Summary of the Third Experiment.

	grms.
In the plants gathered	0·0319
In the soil	0·0020
In the crop	0·0339
In the two seeds	0·0349
Loss of nitrogen during the culture . . .	0·0010

Conclusion.—No nitrogen was fixed during the vegetation.

Vegetation of the lupin during six weeks.

Fourth Experiment.—In this experiment, I added to the prepared pummice, already containing the ashes of dung, 2 grms. of powdered bone-ashes, in order to increase the proportion of phosphates in the soil. One seed, marked No. 6, weighing 0·343 gr., and which should contain 0·0200 of nitrogen, was planted on the 28th of June.

July 12—The plant had a beautiful appearance.

July 25—The cotyledons, which were very fleshy, were of a deep green; the plant was covered with leaves.

August 8—The cotyledons had perished, having been exhausted for several days. Two leaves had already a yellow tint: the experiment was terminated.

This lupin was one of the most beautiful which I had obtained, either because the very high temperature of July had favored its development, or because the phosphate of lime added to the soil, in addition to the ashes of the dung, had really exercised some influence. The plant was 20 centimetres in height; it had eleven branches furnished with leaves of a deep green, and almost as large as those of a lupin grown in the open ground. This lupin, after dessiccation, weighed 1·05 gr.

Summary of the Fourth Experiment.

In the plant gathered, nitrogen	0·0199 gr.
In the soil	0·0005
In the crop	0·0204
In the seed	0·0200
Gain of nitrogen during the culture . . .	0·0004

Conclusion.—No appreciable quantity of nitrogen was fixed during the vegetation.

Vegetation of the lupin during six weeks.

Fifth Experiment.—I employed, as soil, pounded and calcined brick, into which were introduced ashes of dung and 5 grms. of powdered bone-ashes; on the 5th of July I sowed two seeds, No. 10, weighing 0·345 gr., and No. 22, weighing 0·341 gr. = 0·686 gr., which should contain 0·0399 gr. of nitrogen.

Summary of the Fifth Experiment.

In the plants gathered, nitrogen	0·0369 gr.
In the soil	0·0028
In the crop	0·0397
In the two seeds	0·0399
Loss during the culture	0·0002

Conclusion.—No nitrogen was fixed during the vegetation.

Vegetation of a dwarf bean during two months.

Sixth Experiment.—The beans employed in this experiment and the following were from the crop of 1850; they were weighed when the estimations were executed, which fixed their contents in nitrogen at 4·475 per cent. Since then they had been kept in a flask, each bean having a label indicating its weight.

May 7.—A bean, weighing 0·792 gr., and containing, according to the preceding analyses, 0·0376 gr. of nitrogen, was planted in pummice mixed with the ashes of dung, in a large apparatus.

July 9.—Several nascent flowers were perceived. The plant was altogether remarkably fine; unfortunately, its upper extremity having reached the top of the flask, I was with much regret compelled to terminate the experiment.

I counted twenty well formed leaves; the largest ones 5 and the smallest 25$^{mm.}$ in length, measured from the point to the petiole. The root presented some fibres of 30 centimetres in length. The diameter of the stem, at the thickest part, was about half a centimetre: its height was 50 centimetres.

The humid pummice, removed from the flask, had not the least odor of mouldiness; a portion of this pummice, dried in a close vessel, gave no indications of ammonia.

The green plant weighed 11 grms.; after careful dessiccation it weighed 2·35, or 79 of water per cent.

Estimation of Nitrogen in the Crop.—Ten cubic centimetres of normal acid were equivalent to 0·0875 of nitrogen.

In order not to compromise the result of this experiment, two estimations were made, half of the matter being employed for each.

	cc.	
First half of the crop :—		
Amount of acid; Before the experiment	32·6	
After . .	25·7	
Difference ,	6.9	equivalent to nitrogen 0·01852 gr.
Second half of the crop :—	cc.	
Amount of acid; Before the experiment	32·6	
After . .	26·7	
Difference .	5·9	equivalent to nitrogen 0·01584 gr.
Nitrogen in the entire crop . . .		0·03436

These estimations prove once more the impropriety of analysing less than the whole crop.

Thus, the first half gave :—

Nitrogen, 0·0185 gr. ; or, for the entire plant, 0·0370 gr.

The second half gave :—

Nitrogen, 0.0158 gr. ; or, for the entire plant, 0·0316

Difference . . . . . 0·0054

A much greater difference than could arise from an error in the analytical process.

Summary of the Sixth Experiment.

In the gathered plant, nitrogen	0·0344 gr.
In the soil	0·0016
In the crop	0·0360
In the seed	0·0376
Loss of nitrogen during the culture . . .	0·0016

Conclusion.—No nitrogen was fixed during the vegetation.

Vegetation of a dwarf bean during two months and a half.

Seventh Experiment.—On the 17th of May I planted in pummice mixed with the ashes of dung, a seed weighing 0·468 gr., which should contain 0·0298 of nitrogen. The pummice was put into a small perforated pot, which was introduced into the flask of a B apparatus.

On the 6th of July the plant had six perfectly opened flowers, nearly as large as those of beans grown in the garden. The cotyledons and the seminal leaves were dried up, but still adhered to the stem.

August 1.—The leaves being on the point of falling, I proceeded to the dessiccation. There were twelve leaves, of medium size, and an equal number of small leaves : the most perfectly developed were 4 or 5 centimetres from the point to the petiole, and 2 centimetres in the broadest part. The height of the stem was 30 centimetres ; the dried plant weighed 2·80 grs.

Summary of the Seventh Experiment.

In the gathered plant, nitrogen	0·02363 gr.
In the soil	0·00164
In the pot	0·00244
In the crop	0·02771
In the seed	0·02980
Loss of nitrogen during the culture . . .	0·00209

Conclusion.—No nitrogen had been fixed during the vegetation.

Vegetation of the lupin during five months.

Ninth Experiment.—In one of the largest of my B apparatus, the large flask of which contained, as soil, pummice, with which had been

mixed ashes of dung and ashes resulting from the combustion of twenty seeds, I placed, in various parts of the soil, eight lupins which had been deprived of the germinative faculty by steeping them in boiling water, which was afterwards poured on the pummice soil, because it must necessarily contain some soluble principles. These seeds weighed respectively:—No. 3, 0·316 gr.; No. 4, 0·310 gr.; No. 5, 0·316 gr.; No. 6, 0·316 gr.; No. 7, 0·312 gr.; No. 8, 0·312 gr.; No. 9, 0·316 gr.; No. 30, 0·314 = 2·512 gr., which should contain 0·1462 gr. of nitrogen.

On the 4th of June, I put into the soil thus manured, two lupins, weighing respectively 0·312 and 0·315 gr. = 0·627 gr., which should contain 0·0365 gr. of nitrogen.

July 25.—The two plants were very forward; all the cotyledons had perished.

August 8.—The vegetation was magnificent; and, although, since the 25th of July, the cotyledons had fallen, both plants continued to thrive. Not a yellow leaf was to be seen.

August 14.—The leaves were of a fine green color; the plants appeared as strong or stronger than those sown on the 4th of June in the garden, beside the apparatus.

Sept. 1.—A sudden decrease of temperature which had occurred during the night, occasioned the fall of several petioles furnished with leaves.

The lupins grown in the air bore the cold better.

The experiment was terminated on the 15th of October. Since the 1st of September several petioles had fallen; but some new shoots had sprung forth.

During this observation, the influence of the lupin seeds added by way of manure was manifest. After the fall of the cotyledons, the vegetation followed its ordinary course, the green parts continued to be developed without the first leaves turning yellow and falling, as always occurs when the plant grows in a soil deprived of nitrogenous organic matters.

When the apparatus was dismounted, a slightly herbaceous odor was noticed. It was impossible to detect, either in the pummice or on the fallen and blackened leaves, the slightest indication of mouldiness. As I have already remarked, this circumstance was reproduced in almost all the experiments which I made on confined atmospheres. I attribute it to the care with which I prepared the pummice, the ashes, the distilled water, and the vessels in which these various matters had remained.

The two lupins and their debris were carefully, but very rapidly removed. Their stems were 30 centimetres in height: the longest of the five fibres of the roots were 35 centimetres. In the pummice, with the exception of the shells, there was no trace of the seeds employed as manure.

The two plants, dried, weighed 5·762 grs.

Estimation of the Nitrogen in the gathered Plants.—Ten centimetres of normal acid—equivalent to 0·0875 of nitrogen.

The matter, having been cut very small, was divided into two equal parts, each weighing 2·881, which were separately analysed in large Bohemian glass tubes.

I. Matter, 2·881 gr. :—

		cc.	
Amount of acid;	Before the experiment .	32·9	
	After . . .	11·7	
	Difference . .	21·2	equivalent to nitrogen 0·0564 gr.

After the analysis, the tube was broken, and it was ascertained that the soda-lime, at the point where it was mixed with the matter, was of a very light grey.

II. Matter, 2·881 gr. :—

		cc.	
Amount of acid;	Before the experiment .	32·9	
	After . . .	10·5	
	Difference . .	22·4	equivalent to nitrogen 0·0596 gr.

The soda-lime which had been in contact with the matter was of so deep a color as to lead me to fear that the combustion of the carbon had not been sufficiently complete. I collected this soda-lime, in order to analyse it, after having mixed it with twice its weight of fresh soda-lime.

III. Matter.

		cc.	
Amount of acid,	Before the experiment	32·9	
	After . . .	32·7	
	Difference . .	0·2	equivalent to nitrogen 0·0005 gr.

The soda-lime removed from the tube did not contain a trace of carbon.

We have thus, for the nitrogen of the gathered plants, a total of 0·1165.

Estimation of the Nitrogen in the Water eliminated during the dessiccation of the Soil.—Ten cubic centimetres of normal acid were equivalent to 0.04375 gr.

As it might be supposed that the lupins employed as manure had, in putrefying, given rise to volatile ammoniacal salts, I proceeded to the dessiccation by introducing the pummice soil into an alembic furnished with a sand-bath. I put into the still a saturated solution of common salt boiling at 230° F. It was heated until no more water was condensed in the worm. This water, which should have retained the ammonia, had, however, no odor; it was perfectly limpid. The ammonia was estimated in the apparatus which I had used for determining the very small quanties of this alkali contained in rain water. I found 0·0033 gr. of it, equivalent to 0·0028 of nitrogen.

Estimation of Nitrogen in the dried Soil, weighing 926·65 grs.—The estimation was made with half the matter, that is to say, with

463·325 gr. after the whole had been well intermixed. Two determinations of nitrogen were made, each corresponding to 231·66 of matter, analysed in five operations.

Ten cubic centimetres of normal acid were equivalent to 0·04375 of nitrogen.

Summary of the Analyses.

Nitrogen in the half (463·325 gr.) operated on	0·0252	gr.
„ in the remaining half	0·0252	„
„ in the pummice soil	0·0504	„
„ in the water contained in the soil	0·0028	„
„ in the soil	0·0532	„

If we compare the plants gathered with the seeds from which they sprung, we find that, during the five months of vegetation, they had acquired a very considerable proportion of nitrogen; indeed, there was :—

In the two plants, nitrogen	0·1165	gr.
In the two seeds	0·0365	„
Gain in nitrogen	0.800	„

The gathered plants contained then, nearly three times as much nitrogen as the seeds; but if we introduce into the comparison the eight seeds placed in the soil after the destruction of their germinative faculty, we arrive at the conclusion that the nitrogen acquired evidently resulted from these seeds, in putrefying, having acted as a manure.

Summary of the Ninth Experiment.

	gr.	
In the gathered plants, nitrogen		0·1165 gr.
In the soil		0·0532
		0·1697
In the two seeds, nitrogen	0·0365	
In the eight seeds used as manure	0·1462	0·1827
Loss of nitrogen during the vegetation		0·0130

Conclusion.—The dead seeds, in acting as manure, did not determine the assimilation of the nitrogen of the air during the vegetation of the lupin.

In this experiment, the duration of which was five months, the nitrogen which disappeared represented one-tenth of that which the manure contained.

It results, from the whole of these experiments, that the nitrogen of the air was not assimilated during the vegetation of beans, oats, cress, and lupins. In another Memoir, I shall shew what are the conditions most favorable to the assimilation of this element, when the plants, placed in a sterile soil, are cultivated in the open air—that is to

say, when they are developed under the double influence of ammoniacal vapors and of the organic corpuscles which the atmosphere contains.

M. Dumas remarked, at the conclusion, that this beautiful Memoir of M. Boussingault has not only the result of confirming his former works, and of establishing as one of the laws of the chemical statics of plants, that (at least those on which he operated) they do not derive nitrogen from the air; his work will have another important consequence, by dispelling the doubts which have been raised on this subject, he will re-animate studies of the highest interest, having for their object the economical manufacture of the nitrates, of the ammoniacal salts, and of the cyanides.

Indeed, if the nitrogen of the air cannot supply the nitrogen of the manures, the only known means of replacing the animal matters which form part of natural manures, and which it is not in the power of chemistry to constitute directly, consists in producing, by means of the air itself, chiefly, those nitrogenous combinations which alone can, so far, replace animal matters—namely, the nitrates, ammoniacal salts, and cyanides.

It is unnecessary to add that it will still be indispensable to mix with these matters the phosphates and mineral salts which form part of farm manure. It can only be a question, indeed, when we call attention to the important part which the artificial nitrogenous compounds might play, of the substitution of nitrogenous organic matters which are found in all efficacious manures.

The Academy will comprehend, that by means of the simple and ingenious apparatus with which M. Boussingault has just enriched the laboratory of the physiologist, all the problems relative to the utility of the nitrates, of the ammoniacal salts, and of the cyanides in vegetation, may henceforward be entered upon and solved by our learned colleague.

Comptes Rendus.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

On Silicate *of* Iron *as a* Urinary Deposit.

By John Harley, Esq., Stockport Infirmary.

While engaged in examining the urine of patients suffering from various diseases, the secretion containing the above deposit came under my notice.

The subject is a strong, plethoric woman, aged forty-six, unmarried. Catamenia ceased for the last six years. She states that she had fever about two years and a half ago, which chiefly affected the kidneys, since which she has experienced shooting pains in the lumbar regions, accompanied with scalding and irritation on micturition. This has continued with greater or less severity up to the present time, but has never prevented her from following her employment, which is altogether confined to the house, and that principally during the night.

Appetite and digestion extremely good, as is the health, with the exception of the urinary irritation, which is sometimes severe, and at others amounts to nothing, and some œdema of the legs.

My attention was first called to the urine in the early part of December last, from which date up to the present time I have had the opportunity of daily examining it. The results are the following :—

December 4th.—Urine amber-colored, acid, specific gravity 1·020, having an abundant, light-colored, flocculent, gelatinous-looking precipitate, occupying about one-third of the volume of the urine, together with a heavier, sandy-looking one ; irritation excessive.

Dec. 4th to Dec. 30th.—The urine still retains the same characters, but the sandy-looking deposit has decreased in quantity ; specific gravity averaging about 1·013 ; irritation much diminished.

Dec. 30th to Jan. 12th.—The secretion is for the most part pale-colored ; specific gravity ranging from 1·010 to 1·013. The appearance of the urine is much the same, having a light-colored precipitate floating on a now scanty, sandy-looking one, whose color is generally dirty-white, but occasionally it has a reddish tint.

The average quantity of urine passed in twenty-four hours for one week was forty ounces. The irritation and lumbar pains have now almost entirely ceased.

Jan. 12th to March 9th.—Urine presenting the same features, with now and then an additional deposit of urate of ammonia, and occasionally one of uric acid, sometimes in thick lozenges, and at others in beautiful, aggregate prisms. The sandy-looking deposit varies in quantity, but for the most part remains now as little more than a sprinkling at the bottom of a broad vessel, while the flocculent one occupies about an eighth of the volume of the urine. Quantity voided in twenty-four hours varying from forty-three to fifty-five ounces ; once attaining eighty-seven ounces. Average quantity for a fortnight between these last dates, fifty-one ounces. Specific gravity generally about 1·012, falling as low as 1·008 in that whose quantity amounted to eighty-seven ounces.

From these statements it will be seen that these two deposits were constantly present in the urine, and evidently bearing some ratio to each other, and to the amount of irritation experienced.

Examination of the deposits.—On decanting the supernatant urine, and boiling the remainder, the flocculent deposit was dissipated, and the heavier one quickly subsided. This latter was separated from the urine, well-washed, and boiled in water, after which it remained as a dirty looking, fine, sandy powder, which was apparently unaffected by boiling with the dilute or concentrated mineral acids, but the presence of sesquioxide of iron was plainly demonstrable in all, after being so treated, the hydrofluoric excepted, in which the deposit was partially soluble. Exposed to heat it blackened, from the decomposition of a trace of organic matter present, but was with great difficulty fusible before the most urgent flame of the blowpipe. Mixed with an excess of pure carbonate of soda, and fused in a platinum spoon, and the hard, greyish, semi-transparent mass boiled in a quantity of water, a

portion remained undissolved. The watery solution treated in the usual way with hydrochloric acid yielding pure silica. The above insoluble residue dissolved immediately in dilute hydrochloric acid, forming a yellow solution, which on the application of the suitable reagents proved to be one of sesquioxide of iron. It was unaffected by ferrcyanide of potassium, shewing the absence of any protoxide. Repeated analyses proved these two substances, silicic acid and sesquioxide of iron, to be present in the deposit in equivalent proportions, $Fe\ O^{1\cdot5} + SiO^2$—that is in nearly equal parts.

With regard to the flocculent deposit, a microscopic examination shewed it to consist of separate and distinct nucleated cells, which, under a quarter-inch object-glass, appeared as beautiful globules, rather larger than those of ordinary mucus, having a well-defined margin and containing from three to five distinct nuclei. I have not the least hesitation in referring these cells to what Dr. Golding Bird has provisionally termed "organic globules;" and this case agrees with his experience, that their presence is indicative of great irritation in the urinary organs. Mixed with these globules could be seen, under the microscope, transparent irregular particles of the siliceous deposit, a portion of which was always suspended amongst them in the urine, and would become deposited on boiling it.

The decanted urine was apparently normal, always possessing an acid reaction. The urea was found deficient in quantity, amounting to only 8·77 grains in 1000. We must not forget, however, that the amount of secretion is nearly double that given as the usual average.

From the following analysis of the bright, filtered urine, it will be seen that it contained a comparatively large amount of silica.

In 21,000 *grains of urine, specific gravity* 1·013	Gr.	*In* 1000 *gr.* Gr.
Chloride of sodium, phosphate of soda, chloride of ammonium	216	10·28
Phosphate of lime and magnesia	23	1·09
Sesquioxide of iron, a trace	—	—
Silicic acid	5	0·238
Total amount of inorganic salts	244	11·608

The quantity of urine voided on this occasion was fifty-two ounces in the twenty-four hours, thus giving 5·4 grains of silicic acid present in the fluid portion of the secretion for that period.

Another quantity of urine, (eighty-seven ounces, specific gravity 1·008,) passed in twenty-four hours, furnished exactly the same quantity.

The presence of so large a quantity of silicic acid in this secretion, and its combination with sesquioxide of iron, as a distinct urinary deposit, is remarkable, and cannot fail to be of interest, both in a physiological and pathological point of view.

Every care was taken to ascertain, with regard to the ingesta,

whether anything unusual was taken, or whether there existed a partiality to any of the ordinary articles of diet, such as bread; but these precautions only served to prove that the woman, although a hearty eater, was not an extraordinary one.

P.S.—For the week intervening between July 9th and 16th, I again had an opportunity of examining the urine. The siliceous deposit had increased in quantity, the secretion still retaining the same character as noticed throughout. Multitudes of the "vibrio subtilis" were developed in the still acid and undecomposed urine, after keeping the samples a few days.—*Lancet*, Aug. 12, 1854.

METALLURGY.

Separating Silver from Lead.

At the last annual gathering of the Royal Cornwall Polytechnic Society, Mr. J. A. Phillips, of London, at the request of the chairman, addressed to the Society some observations, in which he stated that one of the most important improvements which had recently been made in the metallurgical art, came into operation last year, and is the separation of silver from lead by means of zinc. After describing the old process of separation, and the subsequent process discovered by Mr. Pattinson, of Newcastle-on-Tyne, involving several crystallisations and a fine cupellation; he stated that still more recently a patent had been taken out by Mr. Parkes for a process by which he separates the silver entirely by one operation. To do this, the alloy of silver and lead is melted in the usual way in a large iron pot. To this a small quantity, a few pounds of zinc per ton, is added, the whole mixed up and allowed to remain a short time. By this means the silver is brought to the surface in the form of alloy with the zinc, and this mixture is subsequently skimmed off and treated for the silver it contains. In order to do this the zinc is first partially separated by oxidation, and the residual alloys afterwards treated in the cupel. In connection with the purification of metals, he might mention some of his own experiments in regard to tin. The tin from Peru and some other countries contains a large amount of tungsten or wolfram, which very much depreciates its value. Till recently this tin could only be employed for very common purposes, such as making tin pipes and other things, which did not require tin of good quality. But in analysing some of this tin he happened to discover a process by which the separation was very easily effected, and this process has been recently patented. It consists in taking impure tin, containing from 5 to 10 per cent. of tungsten (worth 25*l.* per ton less than tin of ordinary purity), granulating it by melting it in a reverberatory furnace, and allowing it to flow into a vessel containing water. This granulated tin is then placed in a pan with common hydrochloric acid, which may be obtained from the soda manufacturers at almost a nominal price. This being heated, hydrogen gas is evolved, and a solution of chloride of tin is obtained. In this operation it is necessary the tin should be

present in excess; unless it be so a certain portion of tungsten is dissolved. Should, however, the operation be carried on too far, and a portion of tungsten be dissolved, the addition of a small quantity of impure tin precipitates the tungsten, and chloride of tin, free from tungsten, is obtained. This is turned off into a vat, in which more granulated impure tin is placed, and any arsenic or antimony remaining is there deposited, and a pure solution of chloride of tin is obtained. From this we have to get the chemically pure tin we require, and which is quite as good as the stream tin of Cornwall. Into this bath we put bars of metallic zinc, which precipitate the tin in a spongy mass, when instead of chloride of tin we get chloride of zinc. The tin thus produced may be fused into bars, or sold as the best tin. The chloride of zinc must be so used as to lower the expense of the whole process. To do this it is precipitated by milk of lime, or common chalk; we then get oxide of zinc, which is largely used as a pigment; and to give it sufficient opaqueness for that purpose, the washed oxide of zinc is heated to redness, when it is found to be equal to the ordinary oxide of zinc obtained by sublimation.—*Mechanics' Magazine.*

PHOTOGRAPHY.

On the Colored Impressions produced *by the* Chemical Action *of* Light.

By M. Edmond Becquerel.

The chemical action of light has enabled me, as is well known, to render perceptible the electrical effects produced by reactions operating under the influence of luminous rays. On the other hand, it is more then six years since I was led to the observation of this fact—that it was possible to prepare a surface which should be chemically capable of taking impressions from light, so that it should become of precisely the same color as the luminous rays which should strike it. The sensitive matter possessing this remarkable property is a chloride of silver, which may be called the violet chloride, containing less chlorine than the white chloride, and, in general, mixed with the latter.

The chloride of silver, of which I speak, being able to be placed in such conditions that it will only be affected between the limits of refrangibility of the rays which are perceptible to the organ of vision, it was important to study attentively in what manner it would behave in the apparatus which I have named the *electro-chemical actinometer;* what would be the effects resulting from the action of the different luminous rays whose intensity would be caused to vary within the determined limits; and, finally, whether it would be possible to establish a photometrical method based on different principles from those generally in use. In a former Memoir I commenced this study, but I have found it necessary to re-examine the different circumstances which accompany the preparation of the sensitive mat-

ter, and the modifications which heat and light produce before the luminous rays have given their color to it; such is the aim of the work which I have now the honor of presenting to the Academy.

In former publications I have described various means by which we can obtain colored impressions on surfaces covered with violet chloride of silver; but that which gives the best effects consists in decomposing rapidly, by an electric current, a solution of hydrochloric acid in water, and causing the chlorine to pass to a plate of silver placed at the positive pole of the battery. This process is rendered of easy and certain application, by determining, in each circumstance, and at every minute, the quantity of chlorine which passes over the silver plate. For this purpose we interpose, in the voltaïc circuit, a voltameter of water, so that the current which decomposes the hydrochloric acid and carries the chlorine over the silver, likewise decomposes the acidulated water; the electro-chemical decompositions taking place in definite proportions, the same volume of chlorine is carried over the silver as there is disengaged of hydrogen gas, in the eprouvette, placed above the negative electrode of the voltameter. The other side of the plate is protected by a varnish, so that the chlorine passes only over one side. The proportion of chlorine necessary for the experiment varies in the following proportions:—

Volume of chlorine at the pressure, per square decimeter of surface.	Thickness of the sensitive layer.	
	According to the order of tints of the thin layers.	Supposing the specific gravity of the chloride of silver equal to 5·27.
cub. cent.		mm.
2·80	Commencement of the layer of the second order.	0.00068
From . 3·80	Layer of the third order.	From . . . 0·00092
To . . . 3·90		To . . . . 0·00095
From . 6·50	Layer of the fourth order giving beautiful colored reproductions of the luminous spectrum.	From . . . 0·00158
To . . . 6·90		To . . . . 0·00168

In the Memoir will be found all the indications relating to the different circumstances of the preparation of the sensitive layers, circumstances which should not be neglected.

By employing a thicker layer than those above mentioned, the results obtained will not be so satisfactory. We should therefore operate between the limits of from 4 to 7 cubic centimetres of chlorine at the ordinary pressure per square decimeter of the surface of the silver;

but, in these conditions, the thinner the layer, the more sensitive will the surface be, but the less beautiful will be the tints obtained.

If we project a luminous spectrum on to a sensitive surface thus prepared, we very soon gain an impression beginning with yellow and orange, that is to say, with the most luminous portions of the prismatic image, and extending to the red and violet extremities. This impression, as I have shewn in a previous Memoir, reproduces the different shades of color of the spectrum. But the shades, although very bright, are very dark; and on the red side, between the lines *B* and *A*, and beyond *A*, the impression becomes violet, and darkens rapidly. When the preparation has been made according to the directions in this Memoir, no impression is reproduced beyond the violet; and except the black color on the red side, the image does not extend much beyond the limits *A* and *H*, and occupies the same extent as the visible spectrum.

If mixed luminous rays strike upon the sensitive surface; they leave, like the rays of the spectrum, a picture colored of the same shades as they themselves possess.

But this same substance, when subjected to the influence of heat or light before the action of the luminous rays, leads to remarkable results, of which I am now about to speak.

The action of heat modifies violet chloride of silver very much. An elevation of temperature of from 212° F. to 302° F., causes a change in the color of the prepared plate, without causing it to lose all trace of chlorine, but at the same time it changes the mode of action of the various luminous rays; the diffused or direct solar light acts as white, instead of giving an impression of a grey color, and besides the colored tints are light instead of dark as before the operation. But the most remarkable part is, that by keeping the temperature up to 86° F. or 97° F. for several days, the same end is attained, and with even better results. The yellow and green colors which, except the action of the luminous spectrum at a high temperature on a plate which has been operated on, are not produced with clearness, appear in these conditions; moreover, the sensitive matter is more rapidly sensitive. The plates, thus prepared, may likewise be rendered useful for the reproduction of colored images in the camera-obscura.

We cannot attribute to a chemical action the effect produced on chloride of silver by a difference of temperature which is so slight, but maintained for several days. It is probable that in these circumstances a modification of the physical state of the sensitive substance takes place. This would then be an effect of the same kind as that which takes place in the formation of red phosphorus.

The action of the least refrangible rays of light is likewise very curious, for it leads to a result analogous to that obtained by prolonging the elevation of temperature on the plates. It appears then that in both cases molecular effects of the same order are produced. The luminous spectrum acts in the following manner on the chloride of silver, modified by the extreme red rays. The action begins as before with the orange, yellow, and green, then extends gradually towards

the violet and red. All the tints corresponding to the colors of the spectrum are as bright as if the plates were annealed, but the prismatic impression is far more beautiful; and even the green, the yellow, and the orange have more brilliant tints than before the action of the extreme red rays. Therefore, to the advantage which is possessed by the chloride modified by the least refrangible rays over that which has been annealed, of giving the black ground upon which the different prismatic tints are depicted, is added that of preserving perfectly the yellow and green tints. On the red side, the image of the spectrum is brilliant only as far as *B*; beyond that limit, the black tint which is produced being the dominant one, no effect takes place during the first few moments. However, if in the first place the chloride has remained an insufficient time under the action of the extreme red rays, the solar spectrum still gives a dark impression beyond *B* and even *A*.

We obtain with the matter thus modified by heat or by light, very beautiful colored reproductions of the luminous spectrum. The figures of colored rings, and those given by crystallised plates traversed by polarised light, are likewise well represented in their various tints. We can likewise reproduce the images of the camera-obscura, which are, so to speak, painted by the light; but these reproductions, although brighter in color than any I have obtained before, have as yet only a scientific interest, and I cannot, as yet, hope for any application of them, as the impressions are only retained in darkness. I have hitherto been unable to prevent the action of diffused light, which by degrees destroys these images; it is only, so to speak, in a state of passage that the sensitive matter has the power of reproducing colors.

It will be seen, therefore, that the sensitive substance whose method of preparation is described in this work, enables us to obtain, not only very remarkable effects of color, but likewise results which are susceptible of comparison, so as to observe the electrical effects due to the chemical action of light. I intend treating of this subject in a future communication.

Comptes Rendus, No. 1, July 3, 1854.

Further Researches *on the Methods of* Preserving *the* Sensitiveness *of* Collodion Plates. By John Spiller and William Crookes.

The object of our former paper (in the "Philosophical Magazine" for May), was to draw attention to the principle of preserving the collodion surface moist by taking advantage of the deliquescent character of certain bodies; and we preferred that method of communicating our discovery, as, although theoretically correct, it was not sufficiently developed to warrant our laying it before practical photographers, but required further experiments by which the comparative merits of the different substances at our disposal could be determined.

Having decided against the use of nitrate of zinc, we tried other

substances, and among the rest the acetate of potassa; but although by its use we succeeded in obtaining very good results, yet the sparing solubility of acetate of silver necessitates so many precautions, that we determined if possible to find an equally efficacious salt among the nitrates.

On giving the nitrate of magnesia a further trial, under circumstances which later experience had shewn to be necessary, our former difficulties vanished, and we are now enabled to communicate to the Society the following process, which, in our opinion, scarcely admits of an improvement.

The plate coated with collodion in the usual manner is to be rendered sensitive in a 30-grain nitrate of silver bath, in which it should remain rather longer than is generally considered necessary (about five minutes), it must then be slightly drained and immersed in a second bath, consisting of—

Nitrate of magnesia . . .	4 ounces.
Nitrate of silver	12 grains.
Glacial acetic acid	1 drachm.
Water	12 ounces.

and there left for about five minutes, then removed and placed in a vertical position on blotting-paper, until all the surface-moisture has drained off and been absorbed; this generally takes about half-an-hour, and it may then be packed away in any convenient box until required for use.

Not only is the sensitiveness unimpaired by this treatment, but we think, on the contrary, that it is slightly increased; instantaneous negatives have been taken on plates which had been prepared some days previously. We are not yet in a position to give the length of time that may elapse between the preparation of the plate and development of the picture; such experiments necessarily require a more lengthened period than we have at present been able to give, but as long as they have yet been kept (upwards of three weeks), there has been no appearance of deterioration.

Before the development, we find it advisable to moisten the collodion film by immersion in the silver-bath for about half a minute, as otherwise the pyrogallic acid or iron solution would not flow evenly over the plate. The fixing, &c., is of course conducted as usual.

It will be as well to draw attention to a few points which, although not absolutely essential, may possibly be found useful in practice. The glass plates should be cleaned with more care than is necessary when they are to be used immediately; we have found strong nitric acid applied with a tooth-brush most convenient. With regard to the collodion, we have tried very many different samples, and with tolerably uniform success. The greater number of our experiments have been made with a tolerably thick collodion, the alcohol and ether of which were in the proportion of 1 : 2, made sensitive with four grains of iodide and half a grain of bromide of ammonium to the fluid ounce. We have also employed a collodion containing iodide and bromide of cadmium with good success.

Of the 30-grain silver solution for exciting the plate we have only to recommend the use of acetic instead of nitric acid, to give the bath that faintly acid reaction which is by some operators considered desirable.

There are one or two circumstances to be attended to in the preparation of the magnesia-bath. Commercial fused nitrate of magnesia is very liable to contain chlorine, and also to have an alkaline reaction on account of the fusion being carried too far. Of course the quantities of acetic acid and nitrate of silver given in the formula for the bath are on the supposition that the nitrate of magnesia is pure; if this be not the case, it should be rendered perfectly neutral with acetic acid, the chlorine exactly precipitated with nitrate of silver, and then the proper amounts of acid and silver added. However, if the impurities are very considerable, it will be safer to reject the salt at once. This bath will keep in good order for a long time; the only point to be attended to is to drain the plates slightly after coming from the silver-bath, and, if necessary, to remove the liquid from the back with blotting-paper, so as to introduce as little silver as possible into the nitrate of magnesia. A solution of one grain of silver to the ounce is quite sufficient to keep the plates sensitive; and when the strength rises, as it will in time, to above a certain limit, the slight evaporation that always takes place will render the silver solution sufficiently strong to dissolve off the iodide in small holes. If this occur, the bath can be restored by nearly, but not quite, precipitating the silver with a solution of chloride of magnesium, and then filtering.

One of the most important things to be attended to is the necessity of preserving the plates where they are perfectly free from any light. It will be evident to all, that anything short of absolute darkness, when the sensitive surface is exposed to its action for day after day, and perhaps week after week, must be fatal to its subsequent cleanliness. The necessity for protecting the plates from any deleterious gases, ammonia, for instance, is too obvious to require comment.

Journal of the Photographic Society, July 21, 1854.

PHYSICS.

On Melting Points.

By B. C. Brodie, Esq., F.R.S.*

When the temperature of certain substances is raised, they pass from the solid to the fluid, and from the fluid to the gaseous condition. These transitions are attended with the absorption of heat. There are other bodies which, by elevation of temperature, undergo a transformation of a different kind. Thus, when liquid phosphorus is heated, in such a manner that its change into the gaseous condition is prevented, at a certain temperature it becomes solid, and passes into the red

* The substance of a paper recently read at the Royal Institution.

modification; these allotropic changes also are invariably attended with evolution or absorption of heat.

Considerable anomalies are found in the statements which different experimenters have made as to the melting point of sulphur. The cause of these discrepancies lies in the facility with which the allotropic condition of sulphur is altered by heat. The melting-point of octohedral sulphur lies very close upon the point at which it undergoes a change into the oblique-prismatic condition. When this sulphur has been melted, it passes more or less completely into a third allotropic form. For these reasons, the melting point taken was never that of a *pure* sulphur. However, by certain precautions in experimenting, the true melting points of sulphur have been ascertained.

The experiment cannot be made in the usual manner of taking a melting point, namely, by placing a thermometer in the fluid substance, and observing the point of solidification. Fluid sulphur is always a mixture of more than one modification. The experiment is made by placing minute fragments of sulphur in thin glass tubes, immersing the tubes in a bath of dilute sulphuric acid, and observing the temperature of the fluid at the melting of the substance. Experiments thus conducted have shewn that the melting point of octohedral sulphur is 237° F., and of the oblique sulphur 248° F. This latter sulphur is obtained in a pure condition by heating the octohedral sulphur at a temperature below its melting point, from 212° to 230° F. This change invariably takes place when the sulphur is exposed, even momentarily, to this temperature in a state of powder.

The solidifying point of melted sulphur varies according to the temperature to which it has been raised in the melted condition. Powdered sulphur, carefully melted so as not to raise its temperature above 1° beyond its melting point, will solidify precisely at its melting point, 248° F. If, however, the temperature be raised to 572° F., it will solidify at about 230° F. The cause of this difference is, that the sulphur in the latter case always contains a large portion of a third modification; namely, the viscid form of sulphur.

There are some remarkable anomalies in melting points which do not so readily as the above admit of explanation. Under certain circumstances, drops of sulphur will remain in the liquid condition at a temperature far below the true point of solidification, and solidify instantaneously when touched. The same is the case with phosphorus. Water contained in a capillary tube may be immersed without freezing in a mixture cooled to 166° F. The same experiment may be made with a considerable quantity of water, if the surface be protected by a thin layer of ether. In these cases the water instantly freezes by agitation, or by touching the surface with a solid body. Similar observations have been made in the crystallization of certain salts. A solution of sulphate of soda, made at 86° or 104° F., will not crystallise on cooling, provided the fluid be not disturbed; but instantly crystallises on touching the surface with a wire. This phenomenon does not take place with all solutions. A solution of nitre crystallises normally. There is also a difference of degree in this property. A solution of

borax will remain in an open flask in the supersaturated condition, and crystallises only on violent agitation.

An experiment was shewn, by which a connection was established between this class of facts and those of which mention was first made. Two tubes were exhibited, each containing the same quantity of sulphur dissolved in the same quantity of bisulphide of carbon. One tube had crystallised in the normal manner, the other had deposited no crystals. The sulphur had in both tubes been dissolved at the same time and in the same manner. But the tube in which the sulphur did not crystallise had been exposed to a higher temperature than the other tube. In this case, therefore, it was evident that the cause of the supersaturated condition was an alteration of the substance induced by heat. On breaking the point of the tube and agitating the fluid with a wire, the sulphur instantly crystallised. The analogy was pointed out, of the sudden alteration of this condition by agitation and contact, and the decomposition which many chemical substances, such as the iodide of nitrogen, undergo by similar causes.

Mechanics' Magazine.

On the DEPENDENCE *of the* CHEMICAL PROPERTIES *of* COMPOUNDS *on the* ELECTRICAL CHARACTER *of their* CONSTITUENTS.

By Dr. E. FRANKLAND, F.R.S.

THE lecturer first directed attention to the remarkable continuity and correlation of the natural forces, owing to which the philosopher seeking to eliminate the effects legitimately due to each, frequently experienced the greatest difficulty in separating the true results of a single force from the cognate influence of other forces. Such difficulties were more especially encountered in the manifestations of the chemical force or chemical affinity, which rarely or never acted singly and alone, but was constantly accompanied, modified, and controlled by collateral forces, which alternately exalted, depressed, or altogether inverted it.

The powerful influence of cohesion and heat especially attracted the attention of Berthollet, and so impressed that profound philosopher with their potency, as to lead him to ignore completely the existence of a separate chemical force. Notwithstanding the otherwise singularly ingenious and sound conclusions of this chemist, the lecturer believed that later researches had demonstrated the total denial of a distinct chemical force to be untenable.

The influence of electricity upon chemical affinity was perhaps even still greater than that of cohesion or heat; the most powerful combinations being broken up by this agent, if its operations were favored by the two conditions—mobility of particles (fluidity), and conductibility of the electric current. The phenomenon of the evolution of the separate elements of a binary compound, at the opposite poles of the decomposing cell, was one of the most remarkable attending the resolution of compounds into their elements by the electrical force. It immedi-

ately attracted the attention of philosophers, and almost forced upon them the conclusion that such elements were oppositely electrified.

Davy was the first to seize upon these facts and model them into an electro-chemical theory, which, notwithstanding its defects, was at least as soundly philosophical as those which succeeded it. Davy supposed that the elements in their uncombined condition did not contain free electricity, but that by contact they became excited. Thus, a particle of sulphur became negative when placed in contact with a particle of copper, which last was simultaneously rendered positive; the application of heat intensified the charge, until at a certain point the tension of the two electricities became so high, that they suddenly recombined, carrying with them the molecules of copper and sulphur, which were thus intimately mingled, whilst evolution of heat and light resulted from the combination of the two electricities. Ampère and Berzelius subsequently attempted to remove some of the difficulties which were encountered in endeavoring to make Davy's theory embrace all chemical phenomena. Ampère considered each element to be permanently endowed with a definite amount of one or the other electricity, being thus invariably either electro-positive or electro-negative to an extent dependent upon the intensity of the charge. Such a naturally charged molecule Ampère imagined to attract around it an atmosphere of the opposite electricity of corresponding intensity, and that when two molecules oppositely charged were brought into contact, their atmospheres of electricity united, giving rise to the heat and light of chemical combination, whilst the original charge retained the attracting molecules in permanent union. Although this theory elucidated some points which Davy's view left unexplained, yet it would not be difficult to start several very serious objections to it. The attempted removal of these gave rise to the electro-chemical theory of Berzelius, who supposed that each element contained the two electricities, but that the one was more powerfully developed than the other, as in the case of a magnet, in which one pole, by being divided, was apparently weaker than the other. In chemical combination, Berzelius imagined, that one of the electricities of each element was discharged, producing the heat and light of chemical action, whilst the other was retained and served to hold the elements in combination.

But these attempts of Ampère and Berzelius to improve the theory of Davy succeeded perhaps less in perfecting our views of electro-chemical phenomena than in demonstrating the necessity for much further research, before these phenomena could be satisfactorily interpreted; for these theories, in which different degrees of affinity were explained by differences in the degree of electrical excitement, have been proved radically defective by the remarkable discovery of Professor Faraday, that compounds, whose elements were united by the most dissimilar degrees of affinity, required equal quantities of electric force for their decomposition.

Such defects in the attempts to account for chemical phenomena by electrical agency, led Dumas and other chemists to reject altogether the idea of electro-chemical combination. Dumas regarded a chemical

compound as a group of molecules connected by a single force in a manner analogous to a planetary system, and the chemical character of a compound as dependent upon the position of the separate molecules, and not upon their individual character. This beautiful and highly poetical view would neither have received such an extensive adoption, nor have been the parent of such numerous and brilliant discoveries in the organic portion of the science, if it had not contained a profound truth. Nevertheless the lecturer conceived that the total abnegation of the influence of the electrical character of elements upon the chemical properties of their compounds, implied by this theory of types, was directly opposed to many of the phenomena of chemical combination, which invariably revealed such a connection.

The effect of successive additions of oxygen to an electro-positive element, in gradually weakening its basic, and consequently electro-positive, qualities, and finally converting it into an acid or electro-negative body, was well known in the case of manganese, iron, chromium, gold, &c., but the effects of the juxtaposition of two or more elements of similar electrical character had not hitherto been much studied. Granting the existence of an electrical charge associated with the molecules of matter, it was evident that such a union of atoms as that just mentioned would resemble two approximated globes similarly electrified. Now, the effect of the approximation of two such globes would be the intensification of the charge of each; and, therefore, if there were any connection between electrical and chemical character, it would be exemplified by an increased energy of affinity under such circumstances. Examples of such an approximation of atoms of similar character were not wanting, even amongst inorganic bodies; thus, the compounds of chlorine with oxygen were remarkable instances of the union of like atoms; and we saw in several of them the truth of the foregoing proposition fully borne out. Hypochlorous, chlorous, and chloric acids were all distinguished by the intense energy of their affinities and contrasted strongly with the compounds of oxygen or chlorine with electro-positive elements.

The compounds of phosphorus with hydrogen also exemplified the same effect. Phosphorus, though usually regarded as an electro-negative body, was yet far more closely associated in its general character with the metals than with the metalloïds; we were, therefore, entitled to regard a compound of this element with hydrogen as a juxtaposition of two similarly electrified atoms. Now, two of the compounds of phosphorus with hydrogen, viz., bin-hydride and ter-hydride of phosphorus, were remarkable for the intensity of their affinities, the one being spontaneously inflammable and the other merely requiring a diminution of pressure, when mixed with atmospheric air or oxygen to determine its combustion.

But the influence of the electrical character of elements upon the chemical properties of their compounds was, perhaps, most strikingly seen in the behavior of the organo-metallic bodies, nearly all of which had only recently been discovered. Most of these bodies, which, in their isolated condition, consisted of two or more similarly electrified

atoms, were distinguished by an intensity of affinity which was quite foreign to their proximate, or even elementary constituents. Zinc and methyl, for instance, were neither of them distinguished for any remarkable energy of affinity in their free state; but united as zinc-methylium, they formed a compound whose combining energy surpassed that of all known bodies, and this behavior was shared in also by the corresponding compounds of zinc with ethyl and amyl. In cacodyl, stanethylium, stibethylium, and the new compounds of arsenic with ethyl, we had additional and striking evidence of the same law, for the affinities of arsenic, tin, and antimony, were, in these compounds, exalted in a most remarkable manner by the approximation of similarly electrified atoms.

These examples seemed to prove clearly the great influence of the electrical character of elements upon the chemical properties of their compounds; but further study of the subject also revealed the paramount influence of molecular structure, which modified and controlled the effects of electrical character, and limited all affinity however heightened by electric induction. To this effect of molecular arrangement was no doubt to be attributed the occurrence of some apparent anomalies which, at first sight, appeared to contradict the general law just laid down, such as perchloric acid, biphosphide of hydrogen, &c.; but the pursuit of the subject into this ramification would have far exceeded the limits of the lecture, the chief object of which was to point out that, although all the electro-chemical theories hitherto proposed were far from satisfactory, yet that amongst the factors of chemical action, the electrical character of elements could not be denied a place without ignoring and leaving unexplained some of the most remarkable of chemical phenomena.

The substance of a Lecture lately delivered at the Royal Institution.

TOXICOLOGY.

Contributions *to the* History *of* Poisoning *by* Curare.

By M. Alvaro Reynoso.

All processes for delaying the absorption of curare, are intended to prevent too large a proportion of the poison from penetrating into the economy, and of enabling the latter to eliminate it, so that it shall never accumulate in sufficient quantity to cause the death of the animal. The means which are chiefly employed are ligatures, caustic, and cupping.

Ligatures.—We placed a ligature round the leg of a guinea-pig, and introduced under the skin, below the ligature, 0·060 gr. of curare. For three quarters of an hour the animal appeared to perceive nothing; we then unfastened the ligature, and eight minutes afterwards the poison began to act, and the animal died within twelve minutes more.

Caustics.—We consider that iodine, which has lately been proposed

by MM. Brainard and Greene, as an antidote for curare, should be included in this category.

First Experiment—0·06 gr. of curare mixed with 0·5 of iodide of potassium and 8 cubic centimetres of water were injected under the skin of a guinea-pig. For the first four hours the animal continued quite well, but was dead in two more.

Second Experiment.—We mixed 0·5 gr. of iodide of potassium, 0·4 gr. of iodine, and 0·06 gr. of curare with water. We added hyposulphite of soda, drop by drop, until the iodine had completely disappeared; so as to have the liquor decidedly alkaline we added a a little concentrated solution of carbonate of soda; we then injected the mixture. The animal died in twenty-eight minutes.

Third Experiment.—We maintained, for twenty minutes, at a temperature of 122° F., a mixture of 0·4 gr. of iodine, 0·06 of curare, and 0·5 of iodide of potassium.; we caused the disappearance of the iodine as in the preceding experiment, and the mixture, when injected, occasioned death at the end of twenty minutes.

Fourth Experiment.—We injected, under the skin of a guinea-pig, a mixture composed of 0·06 gr. of curare, 0·4 gr. of iodine and alcohol. The animal lived.

Fifth Experiment.—We kept at 100° F., for forty minutes, a mixture of 0·06 gr. of curare, 0·4 gr. of iodine and sufficient alcohol to dissolve the iodine. We treated the mixture with hyposulphite, etc., and when injected into the animal it killed it in an hour and forty minutes, the action having commenced five minutes after the injection. We repeated the experiment; the action then commenced four minutes after the injection, and the animal died at the end of an hour and a quarter.

We conclude, from these experiments, that iodine does not destroy curare, but that it alters its action, and, moreover, that the same proportion of this body is more efficacious when it is dissolved in alcohol than in an aqueous solution with iodide of potassium.

Sixth Experiment.—We mixed 0·4 gr. of iodine with 0·5 gr. of iodide of potassium, caused the disappearance of the free iodine by means of hyposulphite of soda, added to the mixture 0·6 gr. of curare, and injected it under the skin of a guinea-pig, which died in eleven minutes.

Seventh Experiment.—0·06 of curare was triturated with hypochlorite of soda. We decomposed the hypochlorite by means of hyposulphite of soda; the mixture had a very alkaline reaction, and, injected under the skin of a guinea-pig, it caused its death at the close of four minutes.

Eighth Experiment.—We injected, under the skin of another subject, 0·06 grs. of curare mixed with hypochlorite of soda, and the animal died in seven minutes.

Consequently, hypochlorite of soda has no effect upon curare but that of delaying its absorption.

Ninth Experiment.—We mixed hypochlorite of soda with 0·06 gr. of curare, we then added a few drops of hydrochloric acid. After neutralising the liquor by means of carbonate of soda, we poured into

it hyposulphite of soda; the liquor had an alkaline reaction, and, when injected, produced no effect.

Tenth Experiment.—0·06 gr. of curare was triturated with chloruretted water; we added to this mixture a little carbonate of soda and a few drops of hyposulphite of soda. The mixture had an alkaline reaction, and when injected under the skin of a guinea-pig, produced no effect. Nevertheless, sometimes the animals died after a longer or shorter time, in consequence of the wound. Therefore chlorine, whether in the nascent state, or free, completely destroys curare. The common salt, which is formed is these reactions, does not prevent the absorption of the curare. In fact, 0·06 gr. of curare, mixed with a solution of chloride of sodium, caused the death of the animal in seven minutes.

Eleventh Experiment.—We triturated 0·06 gr. of curare with 7 cubic centimetres of water and 10 drops of bromine. The mixture instantly assumed a peculiar appearance, which shewed that a reaction had taken place. We added some carbonate of soda and a few drops of hyposulphite of soda. The mixture, which had an alkaline reaction, was injected under the skin of the animal. It did not appear to suffer for eight hours, but, at the close of twenty-four hours, it was found in the complete collapse which characterises poisoning with bromide of sodium. It died thirty hours after the injection. Death, in this case, was due simply to the bromide of sodium and not to the curare. We repeated this experiment several times, and always obtained the same result.

We undertook the two following experiments to ascertain the exact manner in which bromide of sodium acts.

Twelfth Experiment.—One gramme of bromide of sodium, dissolved in water and injected, caused the death of the animal in the course of twenty-four hours, accompanied by the same symptoms as in the preceding case, symptoms which differ entirely from those presented by poisoning with curare; in the latter, the animals lose the power of moving towards the end of their lives, whereas bromide of sodium excites them, and they throw themselves about.

Thirteenth Experiment.—After adding bromine to curare, and permitting the reaction to finish, we put the mixture in the sand bath until the bromine was completely evaporated. We then added a few drops of carbonate of soda and some hyposulphite of soda. The mixture, when injected, produced no effect.

Bromine, then, completely destroys curare, in the same way as chlorine, and it has the advantage over this latter of being easily preserved and used. Its action is completely decomposing; in fact, in the regular substitutions of chlorine, bromine and iodine for the hydrogen of organic compounds, the resulting bodies present the same volume, the form, color, capacity of saturation, and the same rotatory power, and the fundamental chemical properties are unchanged. Moreover, as M. Laurent proved for chloride of strychnine, the species resulting from the substitution act on the animal economy, in equal quantity, in the same manner as the normal alkaloïd. It therefore appears probable that curarine, the active principle of curare,

does not suffer a simple substitution of the hydrogen by the chlorine or bromine, for it should then continue poisonous. In our opinion, bromine might probably be used with advantage, and certainly with as much success as any other means, to cauterise the wounds in which venom has been deposited. In the first place, it is a powerful caustic, whose effects can be stopped; moreover, it very probably would destroy venom as it destroys curare.

Fourteenth Experiment. — Fontana made some experiments with this acid; but he concluded from them that sulphuric acid removed the injurious qualities from the American poison. But he was mistaken; he thought that he had removed all the acid from the curare. The latter probably retained a certain quantity, or else it had been destroyed by the action of the heat used to evaporate the acid. It results from several experiments that we have made, that sulphuric acid, according to the dose, may delay the action of the curare, which will then only cause death after a longer or shorter time, or even occasion so much delay, that the animal may not die.

Fifteenth Experiment.—We left, for twenty-four hours, a mixture of 24 drops of sulphuric acid at 65°, 0·06 gr. of curare, and 6 drops of water. At the end of this time, the liquor was neutralised with carbonate of soda, and injected under the skin of a guinea-pig, which died at the end of six minutes. In another experiment, conducted in the same manner, the animal died at the close of four minutes.

Sulphuric acid consequently does not alter curare.

Sixteenth Experiment.—We put 6 cubic centimetres of nitric acid at 36° with 0·06 gr. of curare, and saturated the acid with carbonate of soda; the injection having been made, the animal died in a quarter of an hour. As neither nitrate of soda, nor carbonate of soda (we have made the experiment) retards the action of curare, we must conclude that nitric acid a little alters curare. Caustic potassa, which prevents the rapid absorption of the curare, and thus retards or prevents the poisoning, likewise alters this poison a little. Lime water and ammonia retard the absorption of curare a very little. Finally, there are some salts which, without being really caustics, retard the action of curare. Thus, iodide and bromide of sodium have a very feeble action on the absorption, whereas bromide, and, especially, iodide of potassium will retard it even for twenty minutes. What is very curious is, that iodide of potassium, starting from 1 gramme, produces the same effects, whatever be the quantity employed, the dose of curare remaining the same (0·06 gr.) We think that this delay in the appearance of the symptoms of poisoning proceeds from a local effect, and not from a general reaction, for iodide of potassium, introduced into the system previously, does not retard the action of the curare.

Action of Curare on Vipers.—Curare poisons vipers in a very short time, and, in this respect, it differs from the venom of these reptiles, which, according to Fontana, is not poisonous to them.

Action on Fishes.—A small fish lived for four days in 1 kilogramme of water, into which had been put 0·6 gr. of curare; at the end of this time a small wound was made in it, and it died in eight minutes after having been replaced in the water containing the curare.

This experiment shews that the membranes of the gills are not endosmotic for curare.

Comptes Rendus, No. 1, July 3rd, 1854.

Cases of POISONING *by the* EXTERNAL APPLICATION *of* CORROSIVE SUBLIMATE. By H. R. DE' RICCI, Medical Officer of the Ballymahon Union Workhouse.

CASES of poisoning by mercurial preparations are of so rare occurrence, that I am induced to lay before the profession the following account, in which death was the result of the outward application of corrosive sublimate for the treatment of porrigo of the scalp.

P. and W. B., brothers, one aged eleven, and the other seven years, had been suffering from tinea favosa, or porrigo, for a very long period. As well as I could discover, the eldest had labored under it for about six years, and the youngest for about three.

They first came under my notice about a year ago, when they applied for relief at my dispensary; but finding, I suppose, that the cure was not proceeding sufficiently quickly, and that I was not torturing their heads with painful applications, they soon gave up attending; and eventually, in April last, they applied to one Corney Mack, a shoemaker by trade, but well known through the country as a "skilful man," and he engaged to cure them in *a week*.

I am induced to believe that they were displeased with my mode of treatment, because on several occasions they *complained* that the ointment I gave them *caused no pain*, and they several times asked me for pitch plasters, which I always refused; for I have found by experience, in my workhouse hospital, that Dr. Neligan's mode of treatment is decidedly the only one I know of, which offers a chance of cure. I speak thus decidedly upon this matter, because when I began the profession I started with the generally received idea that porrigo is incurable; but having had my hospital at one time filled with paupers drafted from three other workhouses, and among them having chanced to get a large proportion of children affected with porrigo, every one of whom had been under some kind of treatment or other; I instituted a series of experiments in order to test Dr. Neligan's mode of treatment, and satisfy my own mind. The result was as follows:—Of the children treated constitutionally, as he recommends, some recovered; whereas of those who were treated by local applications of the most varied kind, very few were even improved. But to return to my cases.

The father of these unfortunate children having made a bargain with the quack, sent them to his house on or about the 15th of April last, and whilst there, Mack rubbed into their heads a white ointment (which I subsequently learned from himself was made with two drachms of corrosive sublimate, and one ounce of tallow). If my applications had been painless, this made ample atonement for all my deficiencies; pain most agonising at once set in, and before the doctor shoemaker had done *rubbing* in the second, the first was in torture, screaming that his head was on fire.

The operation being completed on both the children's heads, they returned home, and by the time they reached there, their sufferings were so intense that they could be heard screaming from every part of the village where they lived, and, in about *forty minutes* from the application of the ointment, they were completely delirious. Vomiting of green matter, to a large amount, also set in, together with pains in their bowels, diarrhœa, and bloody stools, all in less than three quarters of an hour from the application of the ointment. Thus they continued from bad to worse, till death put an end to their sufferings, the youngest on the seventh day, and the eldest on the ninth day of their illness. During the whole of this time they had not one intermission, and from the moment they returned from Mack's to the hour of their death, the screaming, the vomiting, and the purging never once ceased.

The youngest child having died, an inquest was held, and I was directed by the coroner to make a dissection of the body, which I did thirty-eight hours after death, and gave in my written opinion "that deceased had died from the effects of a mineral poison, probably a preparation of mercury or of arsenic."

These were the appearances I found:—Body well formed, and not at all emaciated; cadaveric rigidity well marked. On examining the head I found the scalp studded with round, depressed, circular ulcers of about one inch to an inch and a half in diameter, with fragments of dock leaves adhering to them; these being scraped off, the bottom of the ulcer presented a peculiar yellow tint, and on making an incision into it perpendicularly to the surface of the cranium, this yellow appearance was seen to penetrate the entire substance of the scalp; it was of firmer consistence than the adjoining sound pieces of skin, and felt under the knife like cutting through a piece of brawn. I removed the calvarium, and found nothing worthy of note except a peculiar dryness of the entire surface of the brain, which was also present in the ventricles and the spinal canal. The substance of the brain itself was firm and white, sprinkled with minute red points, but not in great number. On opening the abdomen I was again struck by the extreme dryness of all the peritoneal surface. The liver was large, but not extremely so, and its substance on section appeared normal; the gall bladder was distended with bile, contrary, I believe, to what has been generally stated to be the case in poisoning by corrosive sublimate. The intestines, as they lay in situ, appeared blotched with pink, purple, and brown spots, and a perfect mass of intus-susceptions,—I counted twenty-three. On opening the stomach, which was moderately distended, I found the mucous membrane injected with red blood throughout, presenting the most beautiful arborescent appearance, but not the smallest ulceration or softening. The duodenum was healthy, but the jejunum, ileum, colon, and rectum, were all in the highest state of inflammation, especially the lower third of the ileum, and the commencement of the colon, about the ileo-cœcal valve, which was not only inflamed, but studded with some small patches of ulceration, about the size of a pea.

The pancreas, kidneys, and spleen, were all healthy; the bladder empty, and contracted to the size of a chestnut.

While this examination was going on, the elder brother died. On the following day, sixteen hours after death, I made an examination of his body also, the details of which I need not give, as the appearances were exactly similar to those just described, with the following exceptions:—The gall bladder was empty and highly contracted; the urinary bladder quite full; the ulcerations of the intestines were also more extensive, and the stomach presented, in addition to the beautiful pink arborescence above described, some few black spots of extravasated blood.

I had seen this last child alive about two hours before he expired; his countenance was then expressive of extreme anxiety and pain; round his mouth, for the space of about an inch and a half, there was a rash, such as appears often in poisoning by arsenic; he was quite delirious, and died shortly after in convulsions.

The principal points of interest which appear to me to be worth noticing in the two foregoing cases are, first, the extreme rapidity with which all the severe symptoms set in. So far as I have been able to learn, from the very few cases of external poisoning by corrosive sublimate which are on record, pain has set in immediately in some of the cases; but vomiting and bloody stools have not commenced for hours and days. Now in the foregoing, although, for the sake of greater certainty, I have said that forty-five minutes elapsed between the inunction and the commencement of the bloody stools, yet I believe that I could reduce that interval to thirty or thirty-five minutes, as the children were attacked at once on reaching home, and the distance is easily walked in half an hour or less.

Another interesting feature was the total absence of ptyalism in both cases, and the appearance of cancrum oris in the youngest; whilst a rash very similar to that which occurs after arsenical poisoning appeared round the mouth of the eldest.

The eldest child passed water throughout his illness, although in diminished quantity, and his bladder was found full on dissection.

The youngest had complete suppression of urine from the commencement; and in him I found the urinary bladder empty and contracted, whilst the gall bladder was distended with bile, contrasting with the elder brother, in whom these conditions were reversed.

The following is the verdict, which was unanimously agreed to by twenty-three highly intelligent jurymen at the inquest; and before giving it I must premise that, in addition to the other evidence, the quack admitted that he had applied an ointment to these children's heads on the day they were taken ill:—"That deceased came by his death from the effects of a poisonous substance applied to his head for the cure of a disease of the scalp by a person or persons unknown to us."—*Dublin Quarterly Journal of Medical Science*, August, 1854.

The German Poison Eaters.

Dr. Tschudi has published, in the *Wiener Medizinische Wochenschrift*, two letters, the translation of which is to be found in the

Journal de Médecine de Bruxelles, containing some curious details relative to a class of people who are habitual arsenic-eaters.

In some countries of lower Austria and of Styria, especially in the mountains which separate these parts from Hungary, there exists among the peasantry the singular habit of eating arsenic. They purchase it under the name of hedri (*hedri*, *hedrich*, *hatterrauch*) from wandering herbalists, or pedlars, who, on their part, obtain it from workers in Hungarian glass, from veterinary surgeons, from charlatans.

These poison-eaters (*toxicophagi*) have a double aim; first, they wish to give themselves, by this dangerous habit, a fresh and healthy appearance, and a certain degree of embonpoint. Many of the peasant girls, and even the men, have recourse to this expedient from coquetry and a desire to please; and it is remarkable what success they attain, for the young toxicophagi are distinguished by the freshness of their complexion and by the aspect of flourishing health. The following is one of many instances. A girl who attended cows, in good health, but pale and thin, was employed at a farm in the parish of H——. Having a lover, whom she wished to attract yet more by her personal charms, she had recourse to the usual method, and took arsenic several times a week. The desired result was soon obtained; and after some months, she became fat, chubby-cheeked, and, in short, quite to Celadon's taste. To carry the effect further, she increased the dose, and fell a victim to her "coquetterie;" she died poisoned. The number of deaths from the abuse of arsenic is by no means inconsiderable, especially among the young people. Every ecclesiastic in those parts can speak of several victims, and Dr. Tschudi states that his researches among the clergy were very interesting. He learned that so careful were the victims of this practice to conceal what they had done, that the secret often was revealed only on the death-bed.

The second advantage gained by the toxicophagi is, that they become more "volatile," more free in respiration, and able to ascend high mountains with ease. Upon every long excursion into the mountains they take a little bit of arsenic, which they let dissolve in the mouth. The effect is surprising. They ascend without difficulty heights which would have been almost insurmountable without this practice. The author adds, that upon this experience, he has advantageously administered Fowler's solution in cases of asthma.

The toxicophagi commence with a bit of arsenic the size of a lentil-seed, or about half-a-grain. They keep to this dose, which they swallow several times a-week, morning and evening, for a long period, to become accustomed to it. Then they increase the quantity insensibly, but with precaution, until the desired effect is produced. A countryman, named R——, of the commune Ag——, a sexagenarian, and in excellent health, was in the habit of daily-taking four grains. He had followed the habit forty years, and had transmitted it to his son. There was no trace of arsenical cachexia in this individual, no symptoms of chronic poisoning. It is to be remarked, however, that, when the practice is dropped, emaciation generally ensues from some cause,

either from the withdrawal of the stimulus, or from accidental or acquired disease. The custom does not diminish the sexual passion, as is the case with the opiophagi of the East, or with those who use the betel in India and in Polynesia. On the contrary, the feeling becomes more strong.

It may be as well to bring to mind a general use of arsenic in Vienna, among the stablemen and coachmen of the great houses. They mix a good pinch of the powder with corn, put a piece the size of a pea in a linen bag, and attach it to the bit of the horse. The saliva dissolves the poison. This produces a bright aspect of the skin, roundness and elegance of form, and foam at the mouth. The coachmen of the hills adopt the same practice before commencing a laborious journey; and horse-dealers carry with them small balls of arsenic, to be given to those animals which they are leading to the market. Should a horse thus treated pass into the hands of a master who does not employ arsenic, he gets thin, loses his freshness, becomes dull, and in spite of abundant food, does not acquire his former sleekness.

Gaz. des Hôpitaux, May 16th, 1854.

THERAPEUTICS AND MATERIA MEDICA.

Observations *on the* Antimonial Powder *of the last* Dublin Pharmacopœia (1850), *and on the* Medical Effects *of the* Teroxide *of* Antimony. By Jonathan Osborne, M.D., King's Professor of Materia Medica, Physician to Mercer's Hospital, &c.*

It is well known that the antimonial powder of the Pharmacopœias was first adopted as an imitation of Dr. James' fever powder, but the opinion has for a long time been gaining ground among practitioners, that it is nearly, if not altogether, inert. I have given it in various doses, large and small, and long ago made a series of trials expressly on this subject, but could never perceive any sensible effect except when combined with calomel. A powder of two grains of calomel and four of antimonial powder, taken at night, was not unfrequently followed by perspiration, but when given alone it never appeared to me to have any effect, and I am thus fully enabled to confirm the statements as to its inefficiency, made by Mr. Hawkins, Dr. Duncan, and Dr. Elliotson.

That it should be thus inactive may be explained from the fact, that the antimony is almost entirely in the form of antimonious acid, and that the proportion of teroxide of antimony it contains is insignificant, never amounting to 4 per cent., according to Dr. Maclagan, and totally absent in some samples, according to the experiments of Mr. Phillips.

In the Philosophical Transactions for 1801, Mr. Chenevix described a mode of preparing antimonial powder in the humid way, in which teroxide of antimony was precipitated from the hydrochlorate by ammonia. It was strange that although this process was referred to

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* Read before the Association of the King and Queen's College of Physicians.

in terms of high commendation by almost all the succeeding writers on pharmacy, and was admitted to possess the great advantage of uniformity of oxidation, of which the process by heat was unsusceptible, yet that it never was admitted into any of the pharmacopœias. It was not till the publication of the last edition of the Dublin Pharmacopœia in 1850, that a mode of preparing the powder by precipitation appeared, and in this a great improvement was introduced by using tartar emetic instead of the solution in hydrochloric acid which Mr. Chenevix had employed. This preparation, however, though bearing the name of antimonial powder, is yet different from it in one respect, and that the most important, in all the antimony it contains being exclusively in the state of teroxide. It has therefore appeared to me desirable to ascertain its medical effects by actual experiment. The only account of the effect of the powder, as prepared by Mr. Chenevix, that I can find, is contained in the following words, with which his paper concludes:—"I gave some of my powder to Dr. Crichton, Dr. Babington, and Mr. Abernethy, gentlemen whose extensive practice and acknowledged skill sufficiently enabled them to judge of its medical properties. They all concur in opinion, that in its general effects it agrees with Dr. James' powder and the pulvis antimonialis, but that it is more mild, and consequently may be given in larger quantities, seldom producing nausea or vomiting, in doses of less than eight or ten grains." The results I have obtained are very different.

I have tried it in twenty cases, selected for careful observation in Sir Patrick Dun's Hospital. The powder was prepared according to the process of the Dublin Pharmacopœia, 1850, by Mr. Morgan, whose accuracy and ability are well known. The dose given in every instance was five grains in the evening and the same at bed-time. The cases were chiefly rheumatism, pneumonia, and bronchitis, and the patients were all adults. In order to present a view of the percentage of the effects, and to facilitate recollection and comparison with other observations, I have reduced them to the form of decimals.

TABLE *of the Effects of the Pulvis Antimonialis of the Dublin Pharmacopœia*, 1850. *Dose, five grains evening and night.*

More or less gentle Action on the Bowels	Nausea.	Vomiting.	Perspiration.	Perspiration without Nausea.	No Perceptible Effect.
·50	·45	·20	·65	·20	·10

In order to ascertain the effect of the teroxide of antimony taken separately, I tried some prepared by Mr. Morgan according to the Dublin Pharmacopœia, 1850. It was given in three grain doses evening and night, and to the same average class of cases as the above selected in Sir Patrick Dun's and Mercer's Hospitals.

TABLE *of the Effects of the Teroxide of Antimony (Algaroth's Powder), given in doses of three grains evening and night.*

More or less gentle Action on the Bowels	Nausea.	Vomiting.	Perspiration.	Perspiration without Vomiting	No. Perceptible Effect.
·60	·40	·15	·70	·40	·05

In order to try how far the action of the teroxide could be influenced by the presence of acids, I selected six of the cases in which there had been no effect, or only perspiration, and added to each dose the same weight of citric acid. The result was in every case either nausea, vomiting, or purging. Hence it appears that the teroxide is capable of combining with acids in the stomach, and of forming salts resembling tartar emetic. I find also that the addition of one or two grains to small doses of either rhubarb or aloes produces a remarkable augmentation of the purgative effect of these articles.

The conclusions to be deduced from my observations are :—

1st. That the antimonial powder of the present Dublin Pharmacopœia (1850), differs from that hitherto prepared, not only by containing the antimony exclusively in the state of teroxide, but by medical effects of which the older preparation is nearly if not entirely destitute.

2nd. That as it has not been identified by a distinct name (which is to be regretted), the prescriber should, to avoid confusion, always distinguish it as the antimonial powder of the Dublin Pharmacopœia of 1850.

3rd. That the teroxide of antimony (Algaroth's powder), inasmuch as it contains all the active part of antimonial powder, may be safely substituted for it, the phosphate of lime not contributing to its virtues, and having been at first accidentally associated with it in consequence of the imperfect chemistry of the time when the original process was devised.

4th. That the average maximum dose of the teroxide of antimony, as a diaphoretic for an adult, is three grains evening and night.

5th. That the addition of acids renders it more emetic and more purgative.

6th That the occasionally violent effects ascribed to it by some of the older writers were most probably due to the presence of chloride of antimony, from want of care in the preparation, and that this may be most effectually excluded by precipitating it from tartar emetic by means of an alkaline solution.

Dublin Quarterly Journal of Medical Science, August, 1854.

DIGITALIS *in the* TREATMENT *of* PNEUMONIA.

DR. HEUSINGER, from the peculiar effects of digitalis on the circulation, was led to use it in the treatment of pneumonia. He employs an infusion (made from fifteen to thirty grains of the digitalis in five or six

ounces of water), which is to be taken in doses of a tablespoonful every hour. It is prescribed without any adjunct, and from the very commencement of the disease. He also employs cupping, but only when there is pleuritic stitch present; and if there are signs of gastric irritation, he directs from one-half to one grain of tartar emetic every hour, and continues the treatment until there is a decided amendment, when he replaces it by the infusion of digitalis. He asserts that usually at the end of the first or on the second day, the symptoms which denote the constitutional effects of the digitalis appear, and to these effects, which consist in a perversion of the digestive functions, and a considerable diminution in the frequency of the pulse, is added a marked remission of the symptoms of the disease. He then omits the digitalis, and directs a simple mucilaginous mixture. It is further stated that convalescence is much more rapid than when the disease has been treated antiphlogistically, by repeated blood-lettings.

Gaz. Medicale, and *Dub. Hosp. Gazette.*

PHARMACY.

On Extract *and* Syrup *of* Acorns. By M. Guichard, Pharmaceutist at Besançon.

Some physicians having ordered extract of acorns, and having found no formula for the preparation of this product, I have thought it might be useful to supply this deficiency by addressing to you the following:—

1. One kilogramme of dry acorns, deprived of their cupules, were reduced to a coarse powder, moistened, then piled up lightly in the displacement apparatus, and finally exhausted with distilled water by the ordinary method. The liquor evaporated in a sand-bath, strained and evaporated, yielded 100 grms of extract of pillular consistence.

2. One kilogramme of acorns, treated in the same way with alcohol of 56°, furnished 95 grms. of extract of pillular consistence.

These extracts are of a beautiful brown color, deeper in the aqueous extract, of a sweetish and astringent taste. After a long time they liquify and easily become mouldy; dissolved in distilled water the alcoholic extract leaves a very considerable apothem, which, on the contrary, is less abundant in the aqueous extract.

At first sight, the latter appears to me to deserve the preference:—

SYRUP OF ACORNS.

℞	Aqueous extract of acorns . . .	1
	Distilled water	8
	Simple syrup	30

Dissolve the extract in water, filter, add the solution to the boiling syrup, and maintain a temperature of 194° F.

30 grs. of this syrup contain exactly one of extract.

This formula is based on that of syrup of rhatany. It has the objection, it is true, of being too strong; but this cannot apply to the syrup of acorns, its extract being much less active than that of rhatany.

Journal de Pharmacie, June, 1854.

NOTES AND QUERIES.

CENTIGRADE TESTING.

TO THE EDITORS OF "THE CHEMIST."

SIRS,—In your Journal for June, I charged Mr. William Ackland with publishing, as a new discovery of his own, a system of Centigrade Testing, which he had appropriated from me. His reply, in your Journal for July, virtually admits the truth of my charge, though he tries to gloss over his conduct by specifying certain processes as being not only undoubtedly his own, but as sufficiently important to warrant his appropriating the rest of my system without acknowledgment.

His first special claim to originality is for a method of making a standard test acid. The plan he follows is this:—1. He prepares an acid solution stronger than the required standard; 2. He tests the strength of that acid solution by means of a standard solution of carbonate of soda; and, 3, he dilutes the acid liquor to the proper standard in a test-mixer. *Every step in this process is mine*, and all the details given by Mr. Ackland as original are copied from my unpublished work on *Chemical Testing in the Arts*, which, as I have already said, had been entrusted to him in confidence. Moreover, I published an account of the process, in 1847, in the ninth edition of *Chemical Recreations*, p. 320, and again in January, 1854, in the tenth edition of that work, p. 104.

His second claim is for a method of determining the *per-centages* of solutions containing potash, soda, and their carbonates, sulphuric acid, muriatic acid, acetic acid, and ammonia, by a direct centigrade process, without the use of tables or calculations. Again I say, *the processes are all mine*, and have been published by me repeatedly—the last time in January, 1854; see *Chemical Recreations*, pp. 106, 107, where every acid and alkali named by Mr. Ackland is specified, with the quantity of each that it is necessary to take for analysis by standard test liquors, to give, on Mr. Ackland's plan, "the per-centage without the use of tables or calculations."

Thus, the very processes which Mr. Ackland now claims as being unquestionably his own, are essential parts of my general system. There is not, in his whole performance, one single particle of originality; not a principle, nor a process, nor an application of the art of centigrade testing, that is not taken from my papers.

Yet Mr. Ackland boasts that he is to receive a grant of money from the French Academy of Sciences, in recompense for his invention of a new system of Centigrade Testing.

The second paragraph in his letter to you is a tissue of abuse of me and my apparatus, upon which I shall merely remark, that nearly every clause of every sentence of it is untrue. That my process of testing was unintelligible; that my method of calibration was crude; that my instruments were inaccurate; and that he, at his own cost, and by his own unaided skill, made every instrument perfect, put chaos into order, and threw light upon darkness, while I stood by to ridicule and to check his triumphant career, &c. &c. &c., are assertions made without the slightest regard to truth. I could disprove them by evidence as conclusive as that with which I have exposed the hollowness of his pretensions to chemical originality; but I do not choose to carry on a controversy with a man who has no regard for truth or honor.

Mr. Ackland obtained a prize medal from the Great Exhibition by the same process that has procured him a grant of money from the French Academy—namely, by an abuse of my misplaced confidence, and an unjust appropriation of the results of my labors. The public will, no doubt, in due time, place a proper value upon honors procured by such proceedings.

I am, Sirs, your obedient servant,

10, *Finsbury Square.* JOHN JOSEPH GRIFFIN.

We have received from Mr. Ackland a letter, in which he informs us, that in making the statement that his paper on Centigrade Testing had been translated and published in the *Annales de Chimie*, and rewarded by the Academy of Sciences, he relied on a communication made to him by a person who introduced himself to him as a corresponding Member of the Academy. In answer to a letter addressed by him to the Academy, he is informed that such is not the case, and thus he has been imposed upon by the *soi-disant* Corresponding Member of the Academy of Sciences.